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CONTENTS

Volume 125—Year 1962

Part 1—1962

		Page
Coalminers' morbidity (with Discussion)	F. D. K. Liddell	1
Sampling inspection and Defence Specification DEF-131 (with Discussion)	I. D. Hill	31
On the analysis of interaction in multi-dimensional contingency tables	B. N. Lewis	88
University students, staff and recurrent grants, pre-war and post-war	E. B. Butler	118
The statistics of accidents collected by H.M. Factory Inspectorate, Ministry of Labour	J. Rudd	144
Reviews		151
Current Notes		173
Book list		178

Part 2—1962

Learning and inference (with Discussion)	V. R. Cane	183
The prospect for householders' local rates (with Discussion)	A. R. Ilersic	210
The structure of English milk distribution	E. Strauss	232
The megalithic unit of length	A. Thom	243
Estimating efficient production functions under increasing returns to scale	M. J. Farrell & M. Fieldhouse	252
The estimation of T-year survival rate in patients treated for cancer	J. L. Haybittle	268
Reviews		284
Current Notes		314
Book list		318

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Part 3—1962

		Page
Likelihood inference and time series (with Discussion)	G. A. Barnard, G. M. Jenkins & C. B. Winsten	321
Food supplies and population growth (with Discussion):		
The mathematics of hunger	R. Calder	373
World population trends	B. Benjamin	378
Estimation of food requirements	R. Passmore	387
Future sources of food supply: scientific problems	N. W. Pirie	399
Future sources of food supply: economic problems	Colin Clark	418
Concluding remarks	Brinley Thomas	449
The Reverend Thomas Bayes, F.R.S. (1702–61)	J. D. Holland	451
Estimated expenditure on road transport in Great Britain, 1960	R. F. F. Dawson	462
Statistics of human conflict	Ian Sutherland	473
Reviews		484
Current Notes		515
Book list		520

Part 4—1962

The statistical evaluation of educational allocation and selection (with Discussion)	D. J. Finney	525
A remark on the likelihood principle	Charles Stein	565
Comments on Stein's "A remark on the likelihood principle"	G. A. Barnard	569
Wheat yields in England, 1815–59	M. J. R. Healy & E. L. Jones	574
A supplement to "Note on the history of sampling methods in Russia"	S. S. Zarkovich	580
The chance of being killed in a road accident for people born during different years	Grace O. Jeffcoate	583
A note on small value transactions in India's foreign trade	A. S. Prakasa Rao	588
The recent controversy over resale price maintenance: a rejoinder	P. W. S. Andrews & F. A. Friday	592
Wholesale prices in 1961	The Editor of *The Statist*	596
Regulations of the Research Section		603
Annual Report of the Council		605
Proceedings of a Special General Meeting		626
Proceedings of the one hundred and twenty-eighth Annual General Meeting		627
Reviews		628
Current Notes		664
Obituary:		
Sir Ronald Aylmer Fisher		668
Luigi Einaudi		668
Sir William Palin Elderton		669
Johan Frederik Steffensen		672
Book list		674
Index to volume 125 (1962)		680

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Page

321

373
378
387
399
418
449
451
462
473
484
515
520

525
565
569
574
580
583
588
592
596
603
605
626
627
628
664
668
668
669
672
674
680

Coalminers' Morbidity

By F. D. K. Liddell

Medical Statistics Branch, National Coal Board

[Read before the Royal Statistical Society on Friday, November 17th, 1961, the President, Dr M. G. Kendall, in the Chair]

Summary

Mining is an extremely hazardous occupation and so it is not surprising that the incidence of incapacity due to industrial injury and to the prescribed occupational diseases is high. However, the published figures indicate that coalminers also have very much higher than average rates of absence attributed to "other" sickness. This paper attempts to review the evidence to see whether coalminers are a very unhealthy group or whether there is a substantial amount of malingering among them or whether there are other explanations.

The evidence consists of (a) such figures of morbidity as have been published in official documents and (b) the results of two studies carried out by the National Coal Board on material supplied by the Ministry of Pensions and National Insurance. For these studies, the data consisted of punched cards, one for each spell of certified incapacity due to sickness, for a sample of miners and for a sample of men in other occupations. The studies related to spells terminating in the years 1951 and 1955–6 and covered some 60,000 spells in each year.

The studies showed that the excess of "non-occupational" sickness was largely due to short spells of incapacity. When comparing miners with other occupational groups in terms of long spells due to serious conditions, little difference was found.

In the discussion, which is necessarily to some extent speculative and controversial, it is suggested that the true excess among miners of morbidity due to conditions other than industrial injuries and prescribed occupational diseases may be slight and more or less confined to certain diseases. Most of the reported excess can be explained in terms of the great physical demands of mining. Some weight is lent to this hypothesis by the pronounced occupational gradient: heavy occupations other than mining also showed an excess, although much smaller, while for the mainly sedentary occupations there was a marked decrement.

1. Introduction

It has long been recognized that different occupations have different health hazards. Estimation in terms of mortality has been possible in Great Britain since the Census in 1851 (Registrar-General, 1855); see for example Stocks (1938). Recently, Liddell (1960) has discussed some of the problems of measuring occupational mortality. They include the different age structures of the various occupational groups, the great variation in death rates with age and the inaccuracies which can arise owing to misstatement of occupation at death registration and on Census schedules. Further, a

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particularly arduous occupation can only be performed by fit men and the less fit may have to move from it and take up other work. Such men may die prematurely because of the occupational hazards encountered during their earlier employment, but by convention their deaths should be debited to their later occupations.

Corresponding or greater problems arise in the measurement of occupational morbidity. They are enhanced because no unequivocal definition of sickness is available: it is only possible to "measure" it in terms of such indicators as frequency of consulting medical practitioners or benefit paid because of incapacity for work; and Logan (1960, p. 12) has pointed out that consultation rates would be influenced by differences in the extent to which patients in different occupations have to obtain medical certificates of incapacity; while the same condition may, of course, have quite different effects on the ability to work of men in different occupations. Probably because of these difficulties, progress in occupational morbidity comparisons has been relatively slow. Early statistical work was concerned with particular occupational groups, often quite small, or with particular diseases; see for example Hart (1922), Smith *et al.* (1927), Hill (1929, 1930 and 1937).

The first attempt known to the author to provide comparative figures for major occupational groups on a common basis was the Government's Survey of Sickness from 1943 to 1952, as described by Logan and Brooke (1957). However, as they explained, not only did all the difficulties mentioned above arise in that survey, but additional problems were created by the dependence on interview and the "necessary reliance upon human memory".

Perhaps the most significant step forward in recent years was the preparation by the Ministry of Pensions and National Insurance (1955) of statistics for the year 1951 allowing some form of comparison between major occupational groups. Their material is of course restricted to certified incapacity for work and so does not embrace such sickness as does not lead to absence or, normally, absences lasting three days or less.

Table 1 has been compiled from the results of these two investigations. The figures indicate excess morbidity among miners†, but of an extent which depends greatly on which index of morbidity is considered.

Mining is a dangerous occupation, as illustrated by the rates of fatalities and fractures—the least equivocal indices. In British collieries, the rate of fatal accidents has been decreasing fairly steadily for many years and is now substantially lower than in collieries of most other countries; nevertheless, more than 300 men of a labour force of about 600,000 are killed each year (Ministry of Power, 1960), compared with roughly the same number of men killed in factories out of a labour force of nearly 5,000,000 (Ministry of Labour, 1960). The incidence of spells of incapacity due to fractures (W.H.O. Classification N.800–N.829) sustained at work is over three times higher than average in the coalmining industry (Ministry of Pensions and National Insurance, 1960). The miner is also subject to a number of specific occupational health hazards which can cause prescribed occupational diseases; see for example McLintock (1960). Thus, the very high rates for miners, shown in Table 1, of the

† In this paper, we follow convention by using the term "coalminer" to refer to men in "coalmining occupations", i.e. which are specific to and only practised in coalmining (see General Register Office, 1951); men in the many other occupations carried out within the coalmining industry are not included. The term "miner" is used in a more general sense. In the Survey of Sickness, the classification "operatives and other grades: mining and quarrying" was used. The majority of men within this class would in fact be coalminers.

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incidence of incapacity from industrial injuries and prescribed occupational diseases are not unexpected.

Logan and Brooke (1957, p. 35) have pointed out that, in the Survey of Sickness, the indices of sickness and prevalence, which were based on all reported ailments however slight, thus tended to be inert, i.e. to show much less variation from one group of people to another than is shown by other indices. Nevertheless, that these rates for miners were so little above average, despite their inflation by ailments arising

TABLE 1

Miners' morbidity, 1951

	Miners	*All males*	*Ratio*
M.P.N.I. Statistics			
Number of spells of certified incapacity per 100 men, in a year, due to:			
Industrial injuries	34·5†	4·75	7·26
Prescribed occupational diseases	2·4†	0·27	8·89
Sickness	84·9‡	33·6	2·53
All causes	121·8§	38·6§	3·16
Results of the Survey of Sickness			
Sickness Rates:			
Number of men per 100 reporting some illness or injury in a month	68‖	64¶	1·07
Prevalence Rates:			
Number of illnesses and injuries, per 100 men, reported in a month	129‖	115¶	1·12
Medical Consultation Rates:			
Number of visits per 100 men to or by a medical practitioner in a month	69‖	40¶	1·75
Incapacity Rates:			
Number of days away from work, per 100 men, in a month	215‖	105¶	2·04

† Rate for men in the coalmining industry.
‡ Rate for coalminers, i.e. men in coalmining occupations.
§ Sum of the rates above.
‖ Rate for operatives and other grades in mining and quarrying.
¶ Rate for all men excluding retired, part-time, unoccupied or of unstated occupation.

from the miners' enhanced risk of accident and specific occupational disease, suggests that miners do not differ very markedly from average in their true overall morbidity. Of the high rate of medical consultations, part at least is due to those arising from industrial injuries and prescribed diseases. The ratio of 2·04 for days of incapacity is not incompatible with the ratio of 3·16 for spells of incapacity for all causes, as the rate based on days is comparatively inert because of the high proportion of very short absences which are not normally included in the Ministry of Pensions and National Insurance data.

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Thus the one ratio which requires investigation is that of 2·53 for sickness. The remainder of this paper is devoted to this investigation. In what follows, the term "sickness" is used to refer to conditions other than injuries caused by industrial accidents and other than prescribed occupational diseases.

Table 2 gives information (from Ministry of Pensions and National Insurance, 1955) about the incidence of incapacity due to the six most common causes of sickness, for each of a number of major occupational groups. According to this table, coalminers had incapacity rates $2\frac{1}{4}$ to $4\frac{1}{4}$ times as high as those for all men for each group

TABLE 2

Sickness and occupation, 1951

Number of spells per 100 men of certified incapacity for the six most common causes of sickness

Figures in brackets give each rate as a ratio of the corresponding rate for all occupations

Occupational group (with order and sub-order of the classification of occupations)	*All causes of sickness*	*Influenza*	*Bronchitis*	*Arthritis and rheumatism (exc. rheumatic fever)*	*Disease of stomach and duodenum (exc. cancer)*	*Non-industrial accidents, poisoning and violence*	*Acute naso-pharyngitis*
All occupations . . .	33·6	7·8	3·3	2·9	2·5	2·3	1·3
Coalmining . . .	84·9	18·1	8·1	9·5	7·6	6·7	5·6
(III: 1)	(2·5)	(2·3)	(2·5)	(3·3)	(3·0)	(2·9)	(4·3)
Air and "other" transport .	68·8	13·8	8·8	6·2	5·0	3·8	2·5
(XVII: 4 and 5)	(2·0)	(1·8)	(2·7)	(2·1)	(2·0)	(1·7)	(1·9)
Other mining . . .	50·0	14·6	4·2	6·2	4·2	4·2	2·1
(III: 2)	(1·5)	(1·9)	(1·3)	(2·1)	(1·7)	(1·8)	(1·6)
Foundry work . . .	46·1	8·5	6·4	5·7	3·5	3·5	2·1
(VI: 4)	(1·4)	(1·1)	(1·9)	(2·0)	(1·4)	(1·5)	(1·6)
Rail transport . . .	38·0	10·1	4·1	3·5	2·5	2·2	1·6
(XVII: 1)	(1·1)	(1·3)	(1·2)	(1·2)	(1·0)	(1·0)	(1·2)
Road transport . . .	34·5	8·2	3·3	3·6	2·5	2·2	1·3
(XVII: 2)	(1·0)	(1·1)	(1·0)	(1·2)	(1·0)	(1·0)	(1·0)
Building and contracting .	32·0	7·1	3·5	3·4	2·7	2·4	1·1
(XIV)	(1·0)	(0·9)	(1·1)	(1·2)	(1·1)	(1·0)	(0·8)
Clerks, typists, etc. . .	31·1	8·4	2·9	1·5	1·9	1·4	1·4
(XXIII)	(0·9)	(1·1)	(0·9)	(0·5)	(0·8)	(0·6)	(1·1)
Agriculture . . .	23·2	5·5	1·4	2·1	1·4	2·4	0·5
(II: 1)	(0·7)	(0·7)	(0·4)	(0·7)	(0·6)	(1·0)	(0·4)
Professional and technical .	23·2	6·8	1·3	1·2	1·0	1·3	0·9
(XIX)	(0·7)	(0·9)	(0·4)	(0·4)	(0·4)	(0·6)	(0·7)
Commercial, etc. . .	19·4	5·1	1·6	1·3	1·1	1·1	0·5
(XVIII)	(0·6)	(0·7)	(0·5)	(0·4)	(0·4)	(0·5)	(0·4)
Engineering foremen . .	13·7	3·1	0·8	1·5	0·8	0·8	0·8
(VI: 1)	(0·4)	(0·4)	(0·2)	(0·5)	(0·3)	(0·3)	(0·6)

Derived from Ministry of Pensions and National Insurance (1955).

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of causes. Also, the ranking of the other occupational groups by incapacity rates remains remarkably consistent for the various groups of causes. For example, road transport and building and contracting, which occupy the middle ranks (6th and 7th) for all causes of incapacity, do not have an incapacity rate for the individual causes higher than 5th or lower than 8th.

One possible explanation of these relativities could not be discarded without examination: that the material is misleading because of inaccurate occupational description. The National Coal Board therefore sponsored an examination of this possibility as it might affect miners. It showed (Heasman *et al.*, 1958) considerable inaccuracy in the numerators of sickness rates for coalminers and to some extent in the denominators also. This was mainly because the coalmining industry embraces many occupations (e.g. bricklayers, electricians, fitters) which are not treated in the classification (General Register Office, 1951) as coalmining occupations. However, the men concerned are employed by the National Coal Board and most are members of the National Union of Mineworkers; many of them follow the common habit of quoting their occupation on claims for sickness benefit as "miner" or "N.C.B." and they are then classified as in coalmining occupations. The information obtained by Heasman *et al.* was necessarily limited: of 311 spells of sickness among men classified as in coalmining occupations, 263 were found to be for men definitely in such occupations. Thus "promotion" into coalmining was, at most, 48 on 263 or 18 per cent. The investigations also showed a slight understatement of the population of men in coalmining occupations. From this information, the upper limit of the extent of the error due to inaccurate occupational description was a "promotion" of about 20 per cent. and clearly this is not sufficient to account for all the reported excess sickness among miners.

2. Data and Methods for a Study of Incapacity in 1951

To allow an examination of the excess, the Ministry of Pensions and National Insurance kindly provided the National Coal Board with duplicates of some 60,000 punched cards relating to spells of sickness benefit for both coalminers and others in 1951. No form of identification of individuals was possible and the agreement of the National Union of Mineworkers to the investigation was obtained.

The cards related to spells of sickness, covered by medical certificates, for which sickness benefit was claimed. For their own purposes, the Ministry had collected information about a 10 per cent. sample† of the entire insured population; a 1 in 12 sub-sample had been selected of spells of the six most common causes of sickness. The material available was:

	Coalmining occupations	*Other occupations*
Common causes . . .	1 in 12 of spells in a 10% sample of men (2,000 spells)	(16,000 spells)
Other causes . . .	All spells in a 10% sample of men (23,000 spells)	All spells in a 1% sample of men (19,000 spells)

† All sampling was in terms of National Insurance numbers and, in view of the method of allocation of these numbers, can be treated as random. In their own check, the Ministry discovered a small deficiency in the basic 10 per cent. sample; their figures have been rated up for publication.

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Each card related to a single spell of sickness terminating in 1951. Thus the data do not provide exact information on the incidence of spells in that year but the count of spells terminating is very similar to the incidence of new spells. Nor do the data provide information on the numbers of spells current at some time in 1951, i.e. the period prevalence of incapacity. Further, since benefit is not normally paid for the first three days of absence, the great majority of the spells lasted at least four days.

The details recorded on the card included: the claimant's date of birth, occupation and industry and, relating to the spell of absence, the month and year of the first medical certificate, the duration of the spell, and the diagnosis on the final certificate.

The occupation recorded on the card was that given by the man on his claim for benefit; the industry recorded was that shown on the last contribution card exchanged before he made his claim. Analysis by occupation was preferred because occupation was recorded at the time of sickness and because men in occupations classified as "coalmining" form a more homogeneous group than the men in the coalmining industry as a whole, which includes substantial numbers of craftsmen, administrative and professional men, clerks, etc.† Tabulations in considerable detail were, of course, available from the Census (General Register Office, 1956; General Registry Office, 1956) and could conveniently be used to form denominators for the calculation of incapacity rates by occupation.

To examine the sickness experience of the coalminers it would have been desirable to compare it with a "control" group in which the occupations were similar in physical demands to those of coalmining. There are of course few such occupations, but we compiled a group of occupations which we felt demand a level of physical exertion which, although not as high as that of mining, could fairly be described as "heavy". Further, we excluded from the remaining occupations some which were mainly sedentary. Thus four groups of occupations were obtained as follows:

Group C: coalmining occupations‡ (a population of 0·6 million);
H: heavy occupations, other than coalmining (3·6 million);
O: "other" occupations, i.e. all men in industrial, commercial, transport, etc. activities not included in one of the other three groups (7·5 million);
S: sedentary occupations (3·0 million).

Members of the armed forces were excluded because they are not covered by the sickness benefit scheme while they are still serving. Civil Servants and students were also excluded because information about their sickness, as derived from Ministry of Pensions and National Insurance figures, is necessarily incomplete. The principal occupations included in Groups H and S are given in Appendix I. It must be emphasized that the grouping by heaviness of occupation is to some extent arbitrary, and that Group O in particular is markedly heterogeneous.

As a preliminary, an examination was made of the distributions of spells according to their duration, in terms of days of incapacity, and based on a 6-day working week.

† A few discrepancies were found between industry and occupation, but they probably reflected no more than movement into and out of the particular industries in the period between the exchange of cards and the start of the spell of incapacity. In these cases the cards were allocated according to occupation because this represented the latest position before the start of the spell.

‡ No sub-division of coalmining occupations was possible. The Ministry of Pensions and National Insurance did not regard their classification by occupation as reliable because it had to be made on the basis of the information given by the claimant, which was often too vague to allow close classification. The convention adopted for Census purposes that persons described simply as "coalminers" should be classified as "hewers and getters" was followed and the proportion of such cases was known to be high.

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This showed that absences of whole weeks (i.e. of 6, 12, 18, ... days' incapacity) and of one day more than a whole week (i.e. of 7, 13, 19, ... days) were much more frequent than absences of other durations; Fig. 1 shows a typical distribution. A common way of classifying absences by length, based on a 6-day week, is: up to 6 days; 7 to 12 days; 13 to 18 days; .... This splits the peaks of the distributions and puts spells of just about a week (i.e. of 5, 6 and 7 days), say into two classes. In our study, spells were classified by length so that the peaks were not split in this way. The classes

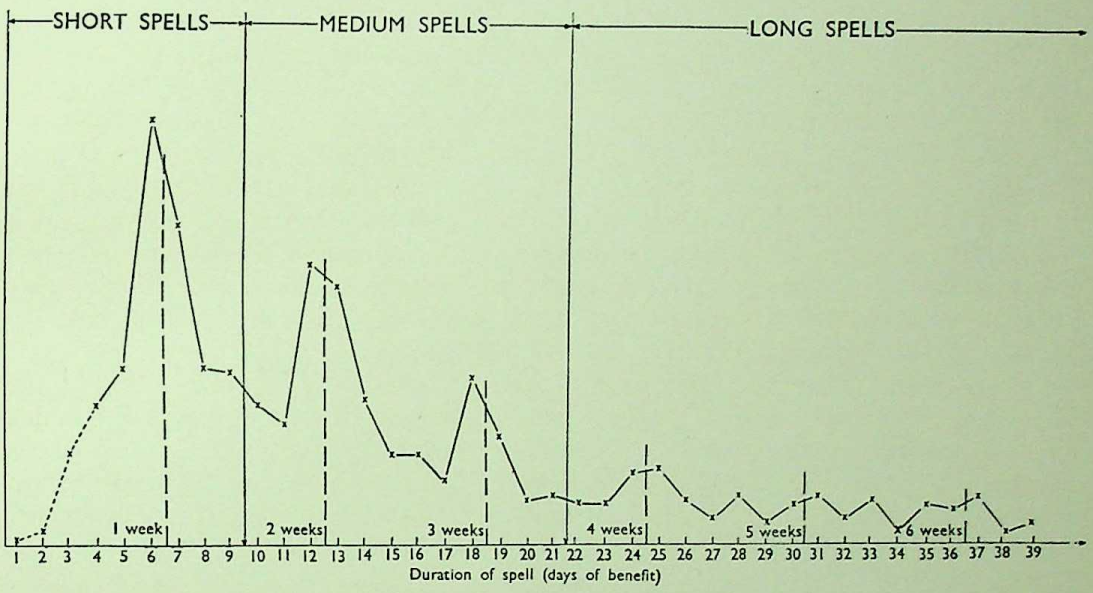

FIG. 1. A typical distribution of spells of incapacity by duration.

chosen were: under $1\frac{1}{2}$ weeks, i.e. less than 10 working days (short spells); $1\frac{1}{2}$ to $3\frac{1}{2}$ weeks, i.e. 10 to 21 days (medium spells) and over $3\frac{1}{2}$ weeks, i.e. over 21 days (long spells).

Further, although the incapacity rates increased with age up to 64, from there on they dropped markedly. Several obvious reasons for this drop suggest themselves and so, in what follows, only material relating to men of ages 15 to 64 has been considered.

Certain causes of incapacity were selected (arbitrarily) for more detailed study. The composition of the disease groups is given in Appendix II.

3. RESULTS OF THE STUDY

Fig. 2 shows the incapacity rates for each occupational group for all causes of sickness by age, differentiating by length of spell. For all spells, irrespective of duration, the rate of incapacity was higher for coalminers than for men in other heavy occupations; heavy occupations had higher rates than those for "other" occupations, while sedentary workers had the lowest rates. These relativities held for all age groups and for each length of spell, although for the longest spells (over $3\frac{1}{2}$ weeks) the differences were much less marked.

In making comparisons between occupational groups, some allowance should be made for the differing age structures. A "Standardized Incapacity Ratio" (S.I.R.) has been calculated (by the "new" method described by Liddell (1960) and discussed

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briefly in Appendix III) for each of the selected groups of diseases for all four occupational groups. The ratios are quoted in Table 3 for all recorded spells and for long spells (i.e. those of over 3½ weeks' duration),† together with their standard deviations (also discussed in Appendix III). In this table, daggers indicate S.I.Rs which differ from 100 by more than could be expected by chance fluctuations of the numbers on which they are based (significance level: 5 per cent.). Since the group of "other" occupations contains just over half the total population, the rates of this group are

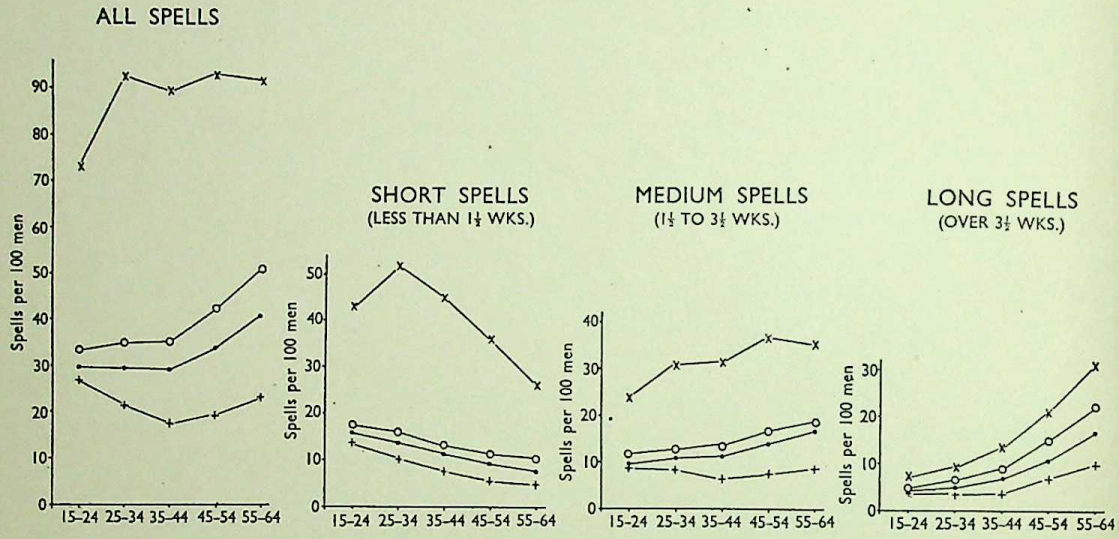

FIG. 2. Incapacity rates by occupation, age and length of spell, 1951, based on spells of incapacity terminating in 1951. No allowance has been made for errors due to inaccurate occupational description.

×———× Coalmining occupations
●———● "Other" occupations
○———○ Other heavy occupations
+———+ Sedentary occupations

usually close to average; for all spells of the common diseases, however, the numbers on which the S.I.Rs are based are so large that even minor departures from expectation appear significant in the purely statistical sense. For all spells, the S.I.R. for coalminers appears with a dagger in all the selected disease groups except one, tuberculosis. In almost every case the S.I.Rs rank in the order: coalmining, other heavy, "other", sedentary.

Allowance has also to be made for errors due to inaccurate occupational description. The only data available for direct assessment of these errors were those obtained by Heasman *et al.* (1958) from which it has already been shown (Section 1) that there was "promotion" into coalmining on claims for sickness benefit. Allowing for men whose occupations may well have been (but were not definitely) correctly classified as coalmining, the best estimate of "promotion" is 17 per cent. (compared with the previous upper estimate of 20 per cent.). However, this is based on only 311 claims for benefit and cannot, by itself, be considered very reliable.

† The corresponding results for short and medium spells are not given because in those diseases where there was a substantial proportion of the shorter spells the relationships were very similar to those for all spells. Further, the figures for all spells of certified incapacity are already truncated by the exclusion of most spells of less than four days' incapacity which, for London Transport (1956) workers in 1951, amounted to roughly as many as the certified spells.

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TABLE 3

Standardized incapacity ratios (based on spells of incapacity terminated in 1951)

(All occupations = 100)

The figures in brackets are the standard deviations of the ratios

N.B. No allowance has been made for errors due to inaccurate occupational description

Disease groups	*Long spells (over 21 days)*				*All recorded spells*			
	Coalmining	*Other heavy*	*"Other"*	*Sedentary*	*Coalmining*	*Other heavy*	*"Other"*	*Sedentary*
All diseases	181† (4)	123† (2)	97† (1)	64† (2)	264† (3)	113† (1)	95† (1)	63† (1)
Respiratory								
Common cold	297† (91)	139 (30)	88 (17)	47† (17)	442† (27)	101 (6)	87† (4)	57† (4)
Influenza	157† (22)	121† (9)	103 (6)	56† (8)	244† (8)	103 (2)	98 (1)	72† (2)
Bronchitis	172† (31)	145† (12)	98 (7)	39† (9)	264† (16)	128† (4)	94† (3)	49† (3)
Rheumatism								
Muscular rheumatism	126 (25)	155† (18)	93 (9)	46† (13)	311† (18)	137† (5)	91† (3)	33† (3)
Arthritis	143† (18)	127 (23)	116 (16)	19† (7)	221† (16)	114 (15)	112 (11)	21† (5)
Gastric								
Gastritis and duodenitis	294† (61)	140† (17)	87 (9)	43† (11)	382† (26)	126† (6)	86† (4)	45† (4)
Peptic ulcer	223† (14)	132† (13)	93 (7)	57† (9)	257† (10)	127† (9)	92 (5)	54† (6)
Disorders of the function of the stomach	235† (22)	110 (23)	116 (16)	26† (11)	368† (13)	112 (10)	97 (6)	34† (6)
Skin								
Skin diseases	287† (13)	132† (12)	88 (7)	49† (8)	260† (5)	122† (4)	95 (3)	50† (3)
Nervous								
Psychoses and psycho-neuroses	149† (11)	99 (12)	94 (8)	113 (15)	220† (8)	101 (7)	96 (5)	87 (8)
Organic nervous diseases	162† (22)	104 (22)	103 (16)	77 (19)	181† (15)	118 (16)	92 (10)	88 (15)
Others								
Diseases of the genito-urinary system	146† (16)	95 (16)	100 (12)	101 (20)	161† (9)	108 (10)	95 (7)	91 (10)
Appendicitis	122 (12)	98 (14)	104 (10)	93 (15)	128† (11)	94 (13)	101 (9)	103 (15)
Diseases of heart and arteries	102 (11)	99 (16)	103 (11)	92 (18)	134† (10)	107 (13)	98 (8)	89 (14)
Neoplasms	171† (34)	99 (32)	78 (18)	112 (40)	170† (24)	124 (26)	96 (16)	74 (24)
Hernia	119 (12)	118 (15)	111 (11)	51† (10)	150† (12)	105 (13)	109 (10)	50† (9)
Tuberculosis	79 (11)	96 (17)	107 (12)	98 (19)	95 (12)	98 (16)	109 (12)	90 (17)

† Indicates that the S.I.R. differs significantly (5 per cent. level) from 100.

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If there were a group of "reference diseases"—i.e. conditions (i) which are not occupational in origin, (ii) in which the diagnosis is unequivocal, and (iii) which cause disability for any occupation for a substantial period—an indirect estimate of "promotion" could be obtained in terms of the rates of incapacity from this group of diseases. There are few conditions which satisfy all three criteria. However, it is

TABLE 4

Comparisons of occupations in terms of the standardized incapacity ratios of Table 3

Disease groups	*Long spells (over 21 days)*			*All recorded spells*		
	Occupations for which S.I.R. is significantly:		*Occupational gradient*	*Occupations for which S.I.R. is significantly:*		*Occupational gradient*
	High	*Low*		*High*	*Low*	
All diseases . . .	C† H	O S	√	C† H	O S	√
Respiratory						
Common cold . .	—	S	√	C†	O S	√
Influenza . . .	H	S	√	C†	S	√
Bronchitis	H	S	√	C† H	O S	√
Rheumatism						
Muscular rheumatism .	H	S	X	C† H	O S	√
Arthritis . . .	—	S	X	C†	S	√
Gastric						
Gastritis and duodenitis .	C† H	S	√	C† H	O S	√
Peptic ulcer . . .	C† H	S	√	C† H	S	√
Disorders of the function of the stomach . .	C†	S	X	C†	S	√
Skin						
Skin diseases . . .	C† H	S	√	C† H	S	√
Nervous						
Psychoses and psychoneuroses . . .	C	—	X	C†	—	√
Others						
Hernia	—	S	X	C	S	X
Tuberculosis . . .	—	C	X	—	—	X

C indicates that S.I.R. for Coalmining occupations differs significantly‡ from 117.
H indicates that S.I.R. for Heavy, non-mining
O indicates that S.I.R. for Other
S indicates that S.I.R. for Sedentary
} occupations differs significantly‡ from 100.

N.B.—In each case of an entry S, the S.I.R. for sedentary occupations is also significantly‡ less than 90.

† indicates that S.I.R. for Coalmining is significantly‡ higher than for men in other heavy occupations (even after allowing for "promotion").

√ indicates the gradient C > H > O > S (after allowing for "promotion").

X indicates any other gradient.

‡ At the 5 per cent. level.

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generally accepted that with appendicitis, diseases of the genito-urinary tract, diseases of the heart and arteries, neoplasms and organic nervous diseases the criteria are met in large part. The S.I.Rs for long spells due to all these "reference diseases", together with their standard deviations, are:

Occupational group	*S.I.R.*	*Standard deviation*
C: Coalmining	129	6
H: Heavy, other than coalmining . . .	100	7
O: "Other"	102	5
S: Sedentary	90	8

The length of disability caused even by these "reference diseases" may depend on the strain of the occupation but it is considered that such an effect is probably small. Thus, most of the excess quoted above for coalmining (i.e. 29 per cent. ± 12 per cent.) is likely to be because of "promotion". The degree of "promotion" can therefore be estimated indirectly as lying somewhere in the interval from 41 per cent. to rather less than 17 per cent. As the direct estimate of 17 per cent. is within this interval, it can perhaps be accepted with greater confidence than before as of the correct order of magnitude.

For occupational groups H and O, the S.I.Rs for long spells of the "reference diseases" are very close to 100. For the more sedentary occupations (group S), the slightly low S.I.R. of 90 may arise in part because of shorter periods of recuperation needed, but in any case it differs quite insignificantly from 100. This suggests that inaccuracies of recording occupations in these groups either are negligible for the purposes of this study or tend to cancel out.

We have therefore prepared Table 4, indicating which S.I.Rs for the other selected diseases differ from "expectation", i.e. from 117 for coalminers and from 100 for the remaining occupational groups. This table also shows for which diseases the ranking of the occupational groups, even after allowing for the "promotion" effect, is in the order: coalmining, other heavy, "other", sedentary; and for which the coalminers' S.I.R., again after allowing for "promotion", is significantly higher than that for other heavy occupations.

These results are examined in the discussion.

4. Extension of Study to a Later Year

Norman (1958) has suggested that studies of sickness absence must be based on more than one year's experience because of variation between years due almost entirely to respiratory diseases. The year of 1951 was, in this sense, unfortunate in that there was a major epidemic of influenza in that year. We were not able to obtain material for a number of years in succession, because the Ministry of Pensions and National Insurance do not make occupational analyses each year as a matter of routine. However, the Ministry did kindly provide us with reproduced punched cards for spells of incapacity in their statistical year 1955–56. The cards received related to all spells current at any time during the year and consisted of a 5 per cent. sample of spells for coalminers and a 0·5 per cent. sample of spells for other occupations but, to allow comparison with the 1951 material, spells which did not terminate in the year were excluded from further study. The information punched on the cards was substantially the same as for 1951, except that industry was not recorded.

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Accurate rates of incapacity could not be calculated because population figures are not available by occupation for any year except that of a Census. However, an estimate was made of the population of men in coalmining occupations by adjusting the 1951 Census figures in the light of known changes between 1951 and 1955–56 in the population of men employed by the National Coal Board, which embraces the vast majority of occupied miners. These estimates by age must be regarded as tentative and so rates based on them can only be accepted as guides to the true rates.

Incapacity rates for coalminers in 1955–56, obtained in this way, have, however, been compared with the corresponding rates for 1951 and with the incapacity rates§ for men in all occupations published by the Ministry of Pensions and National Insurance (1959). The pattern which emerged for 1955–56 was closely similar to that for 1951, except that the disparity in incapacity rates between coalminers and others appeared even greater in 1955–56 than in 1951. The increase was virtually confined to short spells and the patterns were maintained for each group of diseases.

5. Discussion

5.1. *General*

The main statistical evidence about coalminers' morbidity can be summarized as in Table 5, each figure being a ratio of the particular rate for coalminers to that for all occupations.

There are five possible reasons for the high figures for coalminers' sickness apart from the "promotion" effect. First, they could arise if there were extra incentives for miners to obtain certificates and claim benefit. Secondly, they could indicate a true

TABLE 5

Indices of coalminers' morbidity

(All occupations = 100)

	1951	1955–56
Incapacity from sickness, excluding industrial injuries and prescribed occupational diseases (S.I.Rs adjusted to allow for 17 per cent. "promotion")		
All recorded spells for which benefit was claimed	226	282
Spells of more than 21 days	155	162
Spells of more than 21 days for "reference diseases"	110	102
Consultations with medical practitioners for all causes of sickness		
	1951†	1955–56‡
Consultation rate	175	231
Patients consulting in a year	—	138

† *Source: Survey of Sickness* (Logan and Brooke, 1957). Note that this figure relates to men in mining and quarrying occupations; see Table 1.

‡ *Source: Morbidity Statistics from General Practice* (Logan, 1960).

Note: "Promotion" has to be allowed for where numerators and denominators come from different sources in which the accuracy of recording occupation differs. The methods of obtaining rates based on consultations eliminated "promotion", although the samples purporting to be of coalminers may have been diluted by some men incorrectly describing themselves as such.

§ These published figures for all occupations relate to spells starting in the period rather than those terminating and are thus on a slightly different basis.

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state of differing morbidity. Thirdly, they may reflect differences between occupations in the fitness required for work. Fourthly, they may be due to malingering among miners. Fifthly, they may be due to the miners' social environment. None of these reasons is likely to be the sole cause of the differences and all may act to some extent.

In the following paragraphs, these possible explanations are discussed in the light of the material already presented and of such other evidence bearing on the subject as is available. As background it should be mentioned that it is generally accepted among physiologists that mining is among the heaviest of human occupations, as well as being one of the most dangerous. The men often have long, underground journeys to their work places, sometimes involving stooping or crawling. The work itself has usually to be carried out in cramping surroundings with abnormal temperatures and ventilation. The enforced patterns of meals underground are also considered to have major effects on health and morale. It is probable that, with the recent rapid spread of coal-face mechanization and of man-riding facilities in British collieries, the effort of work is decreasing, but other problems, such as the difficulty of keeping pace with machines, may be arising. In any case, these changes have been made with increasing tempo mainly since 1951.

5.2. *Incentives to obtain Certificates*

Although there was no sick-pay scheme for miners before 1958, there was one extra incentive in 1951 for them to obtain medical certificates of incapacity and an additional incentive in 1955–56. In the earlier period, the National Coal Board paid a proportion of an attendance bonus to a man absent from work owing to certificated injury or prescribed disease if he had worked on all the other days of the week and by 1955–56 this arrangement had been extended to cover absence due to certificated sickness. The high consultation rate in 1951 was probably largely due to consultations on account of accidents, etc. and the apparent rise between 1951 and 1955–56 in the consultation ratio (175 to 231) may possibly have been due to the additional incentive to obtain a certificate introduced between the periods. However, these figures may not really be comparable because of their different sources and methods of collection. Indeed comparisons between the two years may be seriously affected by the major influenza epidemic of 1951 and by the changes in the national rates of sickness benefit introduced in April 1955.

5.3. *Excess Morbidity*

From Table 4 it can be seen that, for long spells, the coalminers' S.I.Rs are significantly higher than 117 only for skin diseases, gastritis and duodenitis, peptic ulcer, disorders of the functions of the stomach, and psychoses and psychoneuroses, of those included in the table. Of these five groups of conditions, three also show a marked occupational gradient, with high S.I.Rs for other heavy occupations and the lowest S.I.Rs in sedentary occupations. This suggests that there may be an occupational element in their causation. Again, skin conditions may well be exacerbated by the coalminers' working environment, while gastric diseases may be more common among coalminers because of the patterns of eating enforced on them by underground work. A possible explanation of the high rates for psychoses and psychoneuroses lies in the environment and danger of the work: there is some evidence that miners have less emotional stability than men in other occupations; see for example Heron and Braithwaite (1953), Lion (1960). Thus it would appear feasible that miners suffer a true excess morbidity from all these conditions, which may perhaps all be to some

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extent expressions of psychological disturbance, as suggested by, for example, Halliday (1948).

The particularly low rate of long spells of incapacity among miners due to tuberculosis may be because miners found to have tuberculosis commonly leave the industry, at least until they are cured.

Three further pieces of evidence have to be considered. First, Liddell and McLintock (1960) have shown that airborne bacterial flora in coalmines are more numerous than in any other work places where measurements have been taken; the number of bacteria depended on the number of men the air had passed. Only a small proportion of the bacteria were pathogenic but these came mainly from the naso-pharynx. The measurements were taken in the summer and autumn when the common respiratory infections are least prevalent, and so it is at least possible that in the winter pathogenic bacteria in inspired air would be common. Thus, it is not unlikely that miners have an enhanced chance of respiratory infection. The particularly high S.I.Rs (all spells) for the respiratory diseases among miners may arise from a true excess of these conditions, due to the increased chance of infection at work. Whatever the reason, it seems clear from the second piece of evidence (Higgins and Cochrane, 1961) that miners and ex-miners have more chronic non-specific respiratory disease than other men living in the same areas.

Thirdly, the "corrected" Standardized Mortality Ratio for all coalminers in 1955 was found (Heasman *et al.*, 1958) to be very close to the average for all men. This is based on all men of ages 15 to 64 dying either while still in a coalmining occupation or having retired from coalmining without having taken up other employment; it does not relate to ex-miners who had taken on another occupation. Despite this drawback, it seems to indicate that coalminers are unlikely to have a very great excess morbidity of serious sickness.

5.4. *Difference in Incapacity*

If the same illness incapacitates a man for work in mining more than for any other occupation, miners would tend to be absent from work for relatively minor ailments that would allow other men to attend work; a miner would also often have to extend an absence beyond three days for an ailment that would allow another man to return to work within this time. Thus miners would have a higher rate of the shorter spells of sickness absence drawing benefit and would need to consult their medical practitioners to obtain the necessary certificates, and so have a higher consultation rate. However, a condition that would keep a non-miner absent for as long as 3½ weeks would be expected to keep a miner off work for at least as long, while the miner's rate of such long spells would be slightly increased by the longer recuperation necessary for conditions causing absence among other workers of a little less than 3½ weeks.

The hypothesis that the true morbidity of miners is fairly close to average but that the heavier demands of mining lead to more incapacity could thus explain all the figures given in Table 5.

This argument would also be expected to apply, with less force, to other heavy occupations and to be reversed for sedentary occupations. It would suggest a marked occupational gradient for short spells and hence for all spells, since for most conditions short spells are the majority and dominate. Table 4 shows that for all recorded spells such a gradient exists for every condition included, except hernia and tuberculosis. The lack of a gradient for hernia is probably due to occupational hernias

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attracting injury benefit (see e.g. Ministry of Pensions and National Insurance, 1960) whereas those considered here are those attracting sickness benefit. An explanation of a comparatively low rate for tuberculosis among miners has already been given and may also be applicable for some other occupations.

5.5. *Malingering*

All the material available is compatible with the hypothesis of the paragraphs above. Nevertheless, malingering (for want of a better word) cannot be discounted on this evidence either. However, to account for the occupational gradient there would also have to be a marked degree of malingering among men in heavy occupations other than mining. A reliable study of this subject is impossible without clinical examination and even then would be extremely difficult because of the problems of the definition and assessment of disability.

Some evidence is, however, available from a number of surveys of dermatitis and beat-knee (two of the most common occupational diseases of coalmining): clinical examinations of men working at certain collieries showed that those suffering from the particular condition studied included many more than were absent from work and drawing benefit (see for example Matthews, 1959).

It should also be borne in mind that all the spells of incapacity, leading to the coalminers' S.I.Rs (adjusted for "promotion") of 226 and 282, were of sickness certified by a medical practitioner. It is, of course, possible that the heavier demands of mining lead to more trivial causes being advanced for certification and a tendency among doctors towards lax certification. On the other hand, if doctors do have different standards for different occupations, it could be because they demand the highest standards of fitness for mining.

5.6. *Social Environment*

Jones (1959) has shown that in 1955–56 rates of incapacity through sickness were substantially higher in Wales than in the rest of Great Britain and were higher in Scotland and the northern part of England than in southern England. Only part of these differences seems to be explicable by the geographical distribution of industries; the rest may be due to "social environment", i.e. such factors as climate, housing and migration. The areas with above average incapacity rates include the majority of the coalfields and so it is possible that part of the excess in incapacity rates among miners is due to their social environment. We were not able to examine this possibility on the present material. However, further statistical studies, in which the "promotion" effect will be accurately eliminated, are being undertaken in conjunction with the Ministry of Pensions and National Insurance and this matter will be examined then.

6. Conclusion

Published figures suggest, at first sight, that coalmining is a very unhealthy industry or that there is much malingering among miners. There can be little doubt that the hazards to the miners' health are much greater than normal and that they lead to very high rates of incapacity because of injury and the prescribed occupational diseases. On other sickness, however, there appears to be nothing in the evidence to contradict the hypothesis that the true morbidity of coalminers is close to average, except for certain conditions, and that most of the reported excess can be accounted for in terms of the exceptional physical demands of mining.

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There is, nevertheless, a clear need for additional evidence so that the various hypotheses—and those discussed in this paper are not exhaustive—can be more fully explored. This should become available as a result of the further studies, mentioned above, which are integrated with a major statistical enquiry by the Ministry of Pensions and National Insurance (1961, para. 356), in cooperation with other interested Government Departments, the Medical Research Council, etc. A special sample of insured persons, set up in June 1961, to comprise 5 per cent. of employed men and 2½ per cent. of employed insured women in Great Britain, will be classified by area of residence and occupation, as obtained in detail from each person's employer. Incapacity for work in the following year among the people in the sample will be analysed to give, for the first time as far as the author is aware, reliable data on the incidence of incapacity and the average number of days of incapacity for work to allow comparisons between all major occupational groups.

ACKNOWLEDGEMENTS

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APPENDIX I

Principal Occupations contained in Occupational Groups H and S

Group H: Predominantly heavy occupations other than coal mining:

Fishermen;
Agricultural labourers, foresters and woodmen;
Mining and quarrying occupations other than coal mining;
Makers of bricks and pottery;
Makers of coalgas and coke;
Furnacemen, foundry workers, smiths, sheet metal workers, platers, riveters, welders, shipwrights and steel erectors;
Workers in building and contracting;
Firemen and stokers, porters, coal carmen and dock labourers;
Other labourers and unskilled workers.

Group S: Predominantly sedentary occupations:

Administrative and executive officers of local authorities, directors and managers of industrial undertakings;
Railway officials, ship owners and managers, bus and tramway managers, garage proprietors, etc.;
Proprietors, managers, agents, buyers, etc., of wholesale, retail and advertising businesses;
Persons employed in finance and insurance;
Doctors, lawyers, teachers, scientists, draughtsmen, etc.;
Proprietors and managers of hotels, restaurants, boarding houses and inns;
Clerks and typists.

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APPENDIX II

Classification of Diseases

Disease groups used in this study	*Codes in the W.H.O. international classification of diseases*
Respiratory	
Common cold	470
Influenza	481
Bronchitis	501
Rheumatism	
Muscular rheumatism	726
Arthritis	722–725
Gastric	
Gastritis and duodenitis	543
Peptic ulcer	540–542
Disorders of the function of the stomach	544
Skin	
Skin diseases	690–716
Nervous	
Psychoses and psycho-neuroses	300–318
Organic nervous diseases	330–362, 364–365, 367–369
Others	
Diseases of the genito-urinary system	590–617
Appendicitis	550–553
Diseases of heart and arteries	400–456
Neoplasms	140–239
Hernia	560–561
Tuberculosis	001–019

APPENDIX III

Standardized Incapacity Ratios

In this paper, comparisons between occupational groups, taking account of their differing age distributions, have been made in terms of Standardized Incapacity Ratios. The method of standardization used is that described by Liddell (1960); it is discussed briefly below, with a note on standard deviations. The notation used is as follows:

	All Males	*Particular occupational group*
Population	$P_i = \alpha_i P$	$p_i = \beta_i p$
Incapacity rate, due to a specific disease (number of spells per man)	R_i	r_i

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where capitals are used for All Males and lower case letters for the particular occupational group and where the subscript i indicates the ith age group. The total populations are P and p and the αs and βs indicate the proportions of the populations in the various age groups, so that $S(\alpha_i) = 1$ and $S(\beta_i) = 1$.

The Standardized Incapacity Ratio is defined as

$$\text{S.I.R.} = S(\alpha_i r_i/R_i),$$

and so is seen to be a weighted average of the ratios of the incapacity rates, r_i/R_i. The conventional methods of standardization, direct and indirect, also amount to obtaining weighted averages of these ratios, but the weights are considered to be less appropriate. In the direct method, the weights are proportional to the number of spells of incapacity for All Males, in the various age groups; thus they depend on the age gradient of the incidence of the specific disease under consideration. In the indirect method, which is computationally easier, the weights are proportional to the numbers of spells of incapacity expected in the particular occupational group were the rates for All Males to apply to that group; thus, again, the weights depend on the age gradient of the disease and, in addition, they differ with the differing age structures of the occupational groups. In the method adopted in this paper, the weights are proportional to the populations of All Males in the various age groups and so are independent both of the disease and the occupational group.

The standard deviations of the S.I.Rs can be found on the (usual) assumptions that (a) the number of spells of incapacity in each age group of the particular occupational group is distributed with variance equal to mean and independently of the number of spells in the other age groups, and (b) the other terms involved (p_i, P_i and R_i) are based on such large numbers as to have effectively zero variance. Then

$$\text{s.d. (S.I.R.)} = p^{-\frac{1}{2}}\{S(\alpha_i^2 r_i/\beta_i R_i^2)\}^{\frac{1}{2}}.$$

To ease the computation, the following approximation was used.

$$s' = p^{-\frac{1}{2}}\{S(1/R_i)(\alpha_i r_i/R_i)\}^{\frac{1}{2}}.$$

The errors involved in this approximation are small, but the "standard deviations" quoted in Table 3 of the paper may be misleading in that they take account only of the variation in the numbers of spells and not of the often major variation in the ratios r_i/R_i from one age group to another.

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DISCUSSION ON MR LIDDELL'S PAPER

Dr HEADY: It would be difficult to find anyone better qualified both by ability and by virtue of his job than Mr Liddell to talk about this particular subject. Any other person must be something of an outsider and less well acquainted with the material. Mr Liddell has done his work with his usual proficiency and skill, including the use of his own patent incapacity ratio which all workers in this field will welcome as an improvement of technique.

He has chosen a subject which must interest all those who work in social medicine and in many other fields. Historically, the apparently higher mortality of coal miners has attracted the attention of workers since the days of Farr. This evening we are treated to a refinement. Mr Liddell brushes aside the acknowledged dangers of the occupation—some

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eight times the average risk of being killed on the job, of being involved in an industrial accident or of suffering from prescribed occupational disease—and concentrates on the residual excess of some $2\frac{1}{2}$: 1 of other, more normal, sorts of illness as measured by spells of certified incapacity. This difference he then explains away, if I understand him aright, on the grounds that it is consistent with a hypothesis that "most of the reported excess can be accounted for in terms of the exceptional physical demands of mining".

We are, by now, accustomed to shocks of this sort. Some three years ago we were told by Mr Liddell and his colleagues that, in spite of their dangerous work, "the death rate for all miners was unlikely to be greater than the national rate for all males" and that the occupational mortality data published by the General Register Office were misleading in that respect. One feels that one cannot allow any further crumbling of the edifice without at least some sort of a struggle.

The way in which the occupational mortality ratios are calculated, based on the last recorded occupation before death, does not, as Mr Liddell has said, take any account of the mortality of ex-miners who take another job. It is therefore still possible that mining does lead to relatively early death—but it is not possible to say how much earlier without enquiry. One must also take into account Mr Liddell's other point—that a particularly arduous occupation can be performed only by fit men—and, hence, that if these particularly fit men die at an average rate the occupation must be more lethal than the average. So that, in respect of mortality, we can hang on to some of our illusions.

Mr Liddell's present thesis, as I see it, depends on two main points: first, a discussion of the "promotion" effect based on incapacity ratios for long spells due to a group of "reference" diseases. The use of his estimate of this promotion effect reduces the number of diseases in which coal miners have a significant excess to a relatively small number which he is able to say "may perhaps all be to some extent expressions of psychological disturbance". They may be, but I would rather talk about this here than in the bar of the "local" in a mining town. Secondly, the fact that the main difference between miners and others lies in the short and medium spells fits the hypothesis.

I feel that the weakest link in the argument is the use of his group of "reference diseases". No doubt Mr Liddell has been well advised on this point, but can it really be accepted that the diagnosis of diseases of the heart and arteries is unequivocal; and how do we know they are not occupational?—does this not beg the question? The choice of the particular diseases in this group is obviously crucial and can hardly be said to be beyond argument. The figure of 17 per cent. taken from the previous study is also not beyond criticism. It is based on the number of men whose certificates said they were in coal mining occupations and who were found not to be. It takes no account of the unknown, and presumably unknowable, number who were in coalmining occupations and whose certificates said they were not. It seems, therefore, to be near an upper limit of the likely effect. It would be interesting, too, to know what the incapacity ratio for coalmining for long spells of absence due to reference diseases was in 1955–56. Can one assume from Table 5 that it was $102+17$ per cent. of $102 = 119$? If so, what is its standard deviation, and is not this as good a basis for estimation as the 129 ± 12 deduced on p. 11? In other words, I feel that the use of the figure of 117 in Table 4 is rather arbitrary and from it rather large conclusions follow.

As for the fact that the main difference lies in short and medium spells, it should not be forgotten that medium spells last anything from 10 days to $3\frac{1}{2}$ weeks, which is not a short time, and that there is a considerable disparity between miners and other heavy occupations in medium spells.

Finally, I should like to suggest that Mr Liddell may have dismissed the role of social factors in these differences too lightly. Admittedly there is very little evidence to go on, but such as there is points, I think, to a poorer environment for miners than for men in most other occupations—outside the pit. The standardized mortality ratio for miners' wives around the 1951 census was considerably greater than that of miners themselves. Presumably all the errors of classification which reduce the miners' own ratio to normality

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apply equally to their wives, and therefore the mortality of miners' wives remains in excess of that of the wives of men in other occupations. This is usually taken to suggest that the home environment of the miners themselves is not as good as that of other men.

The infant mortality of miners' children is, and—since figures were first available—always has been a very high one. This is not to be explained by classification errors in the assignment of deaths because recent data are based on the occupation of the father at the time of the birth. This can be taken therefore as another indication of a relatively poor home environment.

All this does not mean that I think Mr Liddell is wrong—and, indeed, he has not committed himself very definitely—but that there may be room for other interpretations of the data than the one he seems to favour. The studies in progress which he mentions should go a long way to answering some of the outstanding questions, and we look forward to hearing the results.

I take great pleasure in proposing a vote of thanks to Mr Liddell for his stimulating paper.

Dr P. A. B. RAFFLE: Like many of my colleagues in industry I think that there is much to be learned about the causation of disease by the study of occupational morbidity statistics. In fact, it is discouraging to many of us that so little is being done in this field. It is encouraging that Mr Liddell has tackled it.

The difficulty about using national data, as Mr Liddell has done, is the definition of the population at risk. This he has discussed in the paper. He has used an ingenious method of circumventing some of the difficulties. Unfortunately, it is a method which is applicable only to a few occupations, mainly those in the nationalized industries. Mr Liddell has high-lighted a feature which many doctors who are not working in industry tend to forget: not only may occupation affect health, but health also affects the capacity to work in different occupations to a different extent. When the sample study of the Ministry of Pensions and National Insurance is published there may be a grave danger of many false deductions being drawn about the effects of occupation on health if the other half of the story is not continually reiterated.

Mr Liddell has shown that coal miners have excess morbidity compared with the rest of the occupied population and he gives six possible reasons for this—the promotion effect; incentives; true excess morbidity; difference in capacity for work; malingering; and different social environment. I hope that he will forgive me if I join Dr Heady in some criticism of his choice of reference diseases which he has used in checking the promotion effect. For instance, his definition of one of the requirements of a reference disease is that "it should not be occupational in origin", yet he includes diseases of the heart and arteries as reference diseases. There is strong evidence that both morbidity and mortality from coronary thrombosis are lower in occupations in which there is much physical activity than in those where there is very little.

Using the Ministry of Pensions and National Insurance figures, some 17 per cent. of all the absences for all the reference diseases which he mentioned are due to coronary heart disease, and there may be as much as a two-fold difference in morbidity from coronary heart disease between the extra heavy occupations, such as mining, and the sedentary occupations, such as those of clerks. To that extent there may be an underestimation of the standardized incapacity ratio for coalminers compared with those in sedentary occupations.

In London Transport for the last 14 years we have been studying sickness–absence data by methods which avoid any difficulty of definition of occupation for the numerator and denominator. The promotion effect is therefore not a problem. We are fortunate in having two large groups of staff—bus drivers and male bus conductors—who have similar characteristics except for the work which they do. In examining the sickness–absence data for drivers and conductors in the light of the five possible reasons which Mr Liddell put forward for the difference between coalminers' morbidity and that of other groups, we

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can eliminate three straightaway—incentives, malingering and social environment. The pay and conditions of service, the sick pay arrangements, the hours of work, the social group to which they belong, the residential area in the sense that both drivers and conductors tend to live near their garages: all these are the same in both groups. There is no reason to suppose that with this similarity of background malingering, if it exists at all in its true sense, would be any different in the two groups.

With this type of background, taking all sickness–absence, bus drivers have a better experience than conductors. Conducting in London is much more strenuous than driving and, in spite of popular opinion, there is probably little difference in the stress involved in the two occupations. Passengers can be as awkward as the drivers of other vehicles.

Looking at specific diseases or groups of diseases, our data for drivers and conductors, and for other groups of staff in other occupations which make different physical demands on the men, broadly confirm Mr Liddell's occupational gradients for all spells. In particular, Mr Cornwall and I have studied bronchitis, and have shown that the difference between occupational groups in frequency and duration of attacks was probably due, with one exception which I shall mention in a moment, to the effect of the disease on the capacity to work rather than to the occupation being an aetiological factor in the disease. There is also evidence of a true difference in morbidity between bus drivers and conductors, just as there is between miners and those in other occupations in Mr Liddell's paper. Conductors have a markedly worse experience from bronchitis than have drivers. This is likely to be explained by the extra physical demand which is placed on conductors. It is also possibly linked with the finding that conductors have more absence from the common cold than drivers, especially in their younger years, which is possibly due to close contact with the public. Bronchitis is a common complication of the common cold.

Broadly, therefore, our data in London Transport, which are free of some of the snags of Mr Liddell's data and do not require the special technique which he has used, support his hypothesis that some at least of the excess of morbidity in a heavy occupation such as mining is due to the exceptional physical demands of the job rather than to the direct effect of the occupation on health.

It gives me great pleasure to second the vote of thanks to Mr Liddell for a most interesting paper.

The vote of thanks was put to the meeting and carried unanimously.

Mr A. H. A. Wynn: Mr Liddell has made a most valuable contribution to a difficult subject. I accept his main hypothesis about the effect of sickness on the capacity to work, but is not the effect of selection probably much more far-reaching than he has suggested? He has referred to selection as the explanation of the low rate of long spells of incapacity due to tuberculosis. Do we know from the medical examination of new entrants—and every entrant to mining has a medical examination—how far they are representative of the population of the mining districts? In those districts the hardships of mining are well understood, and there must be some negative selection against mining as an occupation.

Most coalminers are recruited as juveniles or young adults. If the heavier demands of mining lead to more and longer spells of sickness, it seems likely that these will lead to more wastage of men less able to stand up to the difficult environment, so that the surviving populations in the older age groups are increasingly selected and therefore unrepresentative of the population as a whole. The surviving population will also be increasingly scarred by the environment. We know that to be the case. The "true morbidity" of miners may therefore be close to the average only because it is the morbidity of a surviving population especially adapted to the mining environment.

But I also find difficulty about this concept of a " true morbidity". In some cases there is an objective measure of sickness, but in others, for example with a bad headache, backache or psychological disturbance, a man is often only as bad as he feels, and I doubt whether we can ever arrive at an objective measure of morbidity.

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The same comment may be true of malingering. I believe that to a large extent this is a subjective concept, because it depends on personal intentions, and intentions can never be the subject of scientific measurement or determination. Only in extreme cases, where, for example, a man on sick leave engages in playing football, can malingering be clearly identified.

I hope that Mr Liddell will continue his studies, and that he will find some way of discriminating between the morbidity associated with the different mining occupations. It is not true that all mining is very strenuous. I should say that it is so for only a minority of miners. Moreover, mining as an occupation is changing very rapidly. The most important growing occupation is that of fitters, mechanics and electricians and is not exceptionally strenuous. It is therefore important to distinguish between the morbidity and the mortality, and the accident incidence of the different mining occupations.

Mr B. STEVENS: I should like to add my congratulations to Mr Liddell for his paper, and to take up a few points, largely on social and economic matters. The first concerns the "promotion" factors. He is quite right to point out that doctors' certificates are often misleading in this respect, and not only in the coalmining industry. There is a great deal of inaccuracy in their compilation but this can work two ways. It is true that if a craftsman works on the surface or in a central workshop it is ridiculous to take his morbidity as being the same type of experience as that of an underground worker, but some of these craftsmen work underground. With the growth of machine mining an increasing number of them are subject to the same type of risk from disease as any other underground worker.

Secondly, and probably more important, a large number of men on the surface who do not now do what we would call ordinary mining jobs call themselves miners as a badge of honour. Many men curse the industry in which they work while they are still there, but "miner" is regarded as a term of pride. Nonetheless, the fact that their occupations at the time of recording the morbidity may be something other than hewer or cutter may not mean that they have not had up to 30 years in an occupation underground which was subject to extra risk of certain types of illness.

On the question of whether incapacity tends to increase if the National Insurance benefit rates are raised I have looked at the three occasions in the last few years on which benefit rates have been increased and for which there are comparable figures—1952, 1955 and 1958. On one of these occasions, it is true, in the year when the benefit was increased there was a slight increase in the average weekly intake of new claims, but on the other two there was a decrease compared with the previous year. I doubt whether the small increases of benefit would have an effect, and certainly the figures do not appear to suggest it.

The question of emotional stability is also regarded as of some relevance, Bearing in mind the selection factors, most of the men who choose to take up a career in mining are probably somewhat stronger emotionally in calibre than other people: when they enter the mines most men have some idea of what to expect, particularly when they are brought up in the area. Taking the mining population at large, it may be that those who are predisposed to emotional instability will show a greater sense of it because of their environment than similar people in other types of industry.

Mr Liddell also raised the question of malingering. I think that he was very fair to the miner but, again, possibly the miners are in an industry where this problem is not met to the same extent. It is easier to observe malingering amongst miners for the simple reason that they live together, and many live within sight or within reach of a colliery office. If a manager can see a man papering his front room or digging his garden, he can say whether the man is malingering.

There are a surprising number of checks each year by the Ministry on incapacity claims. I believe that there are no fewer than 600,000 examinations by R.M.O.s of sickness benefit claims alone each year and no fewer than 100,000 checks on injury benefit. The Ministry do their best to prevent false claims being made. In the period studied there was

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no national sickness scheme in operation in the industry, although some men had modest private arrangements. Taking into account the scales of sickness benefit in force after 1948, and the fact that the men tend to marry young and have their families early, there are strong financial incentives for men to keep at work as long as possible or to get back to work after a spell as quickly as possible.

Morbidity as a result of the industry's environment may be considerably greater than the morbidity within the industry at any one time. I meet almost every week men who left the coalmines in the 1920s, 1930s and 1940s. One of them told me he watched his friends and his father's generation falling sick and becoming incapacitated and he decided that the time had come to get out of the industry. Many of these men are working elsewhere. The morbidity which they suffer today is recorded under other occupations, but it may be caused at least in part by their previous experience in this industry.

I do not think that anyone can say today, as they could 20 or 30 years ago, that the social environment of miners is one which would predispose them towards higher morbidity. The miner tends to be at least as well fed and well housed as most members of the industrial working class. His wages are a little higher. There is one geographical factor of interest: someone who lives on a mountainside and has some incapacity affecting respiration would find it more difficult to get about and might lie up for a longer period until fit to go back to work. But the number of these cases is very small.

Mr J. Thomas: There are two points which I want to bring up about this excellent study. It is known that there is an extremely high rate of voluntary absenteeism in coal mining. This probably results in more miner illnesses being uncertified. As a consequence, the sickness rates may be underestimated.

I should like to stress that although the mortality of miners is found to be near the national average, this may be because the study did not include ex-miners.

Mr C. J. Cornwall: I found the results which Mr Liddell obtained from the National Insurance data particularly interesting since they represent a continuation of a pattern of very long standing. The National Insurance figures as a source of data for rates of claim and sickness benefits were preceded by the figures provided by the Friendly Societies. The high claim rate for miners seems to have been very well known to active and expert people in the Friendly Society field throughout very nearly the whole of the nineteenth century. The climax of the use made of Friendly Societies' data was the report by Sir Alfred Watson on the experience of the Manchester Unity Independent Order of Oddfellows in the years 1893–97. Watson analysed a very large volume of data, quite exhaustively and in a large number of ways. He analysed the figures by urban and rural sub-groups within geographical areas and by occupation, and within each of those resulting sub-groups figures were presented showing claim rates both by age and by duration of sickness.

An interesting point in relation to Mr Liddell's results is that the mining occupation, which represented about 6 per cent. of the whole experience, had claim rates measured in weeks of sickness which were remarkably consistent—at about 60 or 70 per cent. higher than the experience of the whole Society in each age group, in each urban and rural sub-group of each geographical area and in total. That was the case both for the first six months of the duration of sickness claims and for the whole duration of sickness claims.

The group of miners was drawn so as to include all miners but to exclude quarrymen. No doubt the great majority of the miners were coalminers, and because the exposed-to-risk data and the claim data came from the same source there was no problem comparable with the promotional effect.

Watson also extracted what are now called the period prevalence rates, although he may not have known this term in his day. If he had known it, he might not have liked it. He presented a table showing these rates, which indicate clearly that the overall excess in miners' claims can largely be attributed to the number of claims. A further table shows the average duration of claims related to the numbers making a claim in a year. That adds

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slightly to the total volume, but the significant factor lies in the prevalence rates, which were remarkably consistent at about 60 per cent. above the whole Society's experience for each five-year age group between 25 and 55. There were slightly lower ratios for the youngest and oldest ages. It is worth looking at the five possible factors which Mr Liddell has mentioned in relation to the thought that what we are looking at is not an isolated phenomenon but one of very long standing.

I do not believe that there was any great incentive for a miner to be sick in the 1890s. There may be now, and that may account for an increase in the ratios in the 1950s compared with the 1890s. It is interesting to note that the ratio of 160 which I mentioned for all claims is roughly the same as appears in Table 5 for spells of more than 21 days. The 1950 rates for all claims appear to be considerably higher than the figure of about 160.

Social environment may well influence the overall results, but Watson showed that at least in the 1890s it was not of itself sufficient to explain the whole difference because, within each geographical area and within each urban and rural sub-group, the difference between miners and others existed.

That leaves the remaining three possible causes. It seems to me almost impossible to attribute the appropriate weight to each of these. One must decide which is likely to be the more stable factor causing this continuity in the excess over a long period. Bearing in mind the difference in patterns of incapacity by diagnosis, which might have been expected over the years to change the ratio, and the different economic factors which would be expected to have an effect on malingering, I would look to differences in incapacity as being probably the most important continuing factor. On the other hand, we may be observing the net result of various conflicting trends, some upwards and some downwards. One particular trend which may be worth attention is that in the population as a whole during this century there has been a marked growth in the habit of smoking.

One thing that we can feel almost certain about is that miners do not smoke while at work. Perhaps Mr Liddell can tell us whether there are any data indicating the extent to which they smoke off duty. If their total consumption of cigarettes is less than that for the general population, then, other things being equal, it might be a factor in their favour in relation to sickness. A *prima facie* case has been made that smoking is a contributory factor in the development of a wide range of diseases, such as cancer of the lung, respiratory conditions, coronary heart disease, certain digestive disorders, and the reactivation of tuberculosis in later life, and it may be that if some standardization according to smoking habits were made, the ratios recorded here would have to be adjusted upwards.

Mr C. M. STEWART: I, too, intended to refer to Watson's investigation but the previous speaker dealt with it at some length so I shall restrict myself to one further point.

Watson, like Mr Liddell, arranged his occupations in four main groups, and he, too, discovered that the sickness of miners was the highest. He formed a second group of other heavy occupations, such as quarrymen, iron and steel and chemical workers where sickness was somewhat lower, and two other groups where sickness was lower still—in just the same way as Mr Liddell. A good deal of the paper was therefore very familiar.

I was surprised at the absence from the paper of the word pneumoconiosis. I certainly thought that this disease was a great problem in the mining industry. No doubt Mr Liddell will give us some explanation of its omission.

It is true that it would not appear in a number of the tables in the paper. In the first table, pneumoconiosis does not attract injury benefit and would not be included in the spells of certified incapacity due to industrial accidents and the very small number of spells of certified incapacity due to prescribed occupational diseases. Pneumoconiosis, for the major part, is not a completely incapacitating disease; the majority of those receiving benefit suffer a loss of faculty, but are able to carry on in their ordinary occupations.

What struck me was that here we have an industry in which persons are suffering from pneumoconiosis although they may not be severely disabled by it. Through having

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contracted the disease may they not be more prone to sickness, possibly various coughs and colds, influenza and tuberculosis, which appear in the various tables? The lack of reference to pneumoconiosis in any part of the paper causes me concern. I believe that there are about 50,000 persons disabled by the disease to a greater or lesser extent—by which I mean persons receiving some kind of benefit, either under the Industrial Injuries Acts or under the Workmen's Compensation Acts or those Acts dealing with persons who fall between those two stools. Not all will still be of working age. Nevertheless, this forms a fairly sizeable group. Is there any possibility from the data available of seeing what proportion of those for whom he has recorded sickness were suffering from pneumoconiosis? There is no suggestion in the paper that this has been looked at. In further research, the first thing I would do would be to see what part, if any, is played by pneumoconiosis.

Pneumoconiosis is not standard throughout all divisions of the National Coal Board. I do not suppose that the Ministry of Pensions and National Insurance data will help here, but the sickness experience in the different divisions of the National Coal Board, such as the southern divisions (especially Wales) and Scotland, where there is a fairly large incidence of pneumoconiosis, might throw some light on the subject.

Mr Liddell points out that the 1951 data included only those spells of sickness which terminated during the year. In his 1955–56 investigations he had data of all spells of sickness, including those which were not terminated, but he eliminated the latter from the investigation in order to make the data comparable with the 1951 data. I should like to go back to the 1955–56 data and examine the spells which did not terminate to find out, for example, whether a large proportion of those people suffered from pneumoconiosis or were disabled from accidents at work.

Mr Liddell has dealt with the number of spells of sickness rather than the duration of sickness. As actuaries, some of us are faced with problems of compensating sickness absence by means of weekly payments and much more attention needs to be paid to the longer spells of illness. For example, one spell of sickness not terminated might have lasted for two years—100 weeks—and would therefore be 200 times more important in terms of absence from work than a single spell of three days. Perhaps actuaries look at the matter too much from one angle, but it occurs to me that the omission from the investigation of spells not terminated means that we are losing a lot of sickness.

Mr H. W. Haycocks: I understand that the "all sickness" rate by age in the 1893–97 experience of the Manchester Unity is more or less the same as recent National Health experience. It would seem from what has been said tonight that there is more or less the same excess of miners' sickness. These are extraordinary facts considering that over 60 years have elapsed.

I should like to comment on the method of standardization which the author has used. I remember many years ago being given an examination question in which I was asked to comment on an index number very much like the one used in the paper, I believe that Mr Liddell mentions it in an earlier paper; instead of the standard population for weights, the occupational population was used. I came across this index again years later in a publication *Vital Statistics Rates in the United States*, 1900–1940, which also gave a very interesting account of the various methods available. The author of this publication was opposed to Mr Liddell's method on the ground that it was not so much an index of death rates as an index of populations. I do not want to stress the point, I am no lover of index numbers. It seems to me that their main and safest use is simply one of illustration: one chooses that type of index number that best illustrates the results of the detailed analysis.

In some investigations we are more interested in ages where there are high absolute rates than in where there are very low ones. For example, in Friendly Society work, we are very much more interested in variation where the number of claims is high than where the number is very low. For that reason we should want to use for illustrative purposes the traditional type of index, the ratio of actual to expected sickness, rather than the index which Mr Liddell has used.

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Mr LIDDELL replied briefly at the meeting and subsequently as follows:

As so much of the argument of the paper depends on the validity of the assessment of "promotion", which was (quite understandably) called in question, I shall first show, from an examination inspired by the criticism, that the estimate I used was, if anything, conservative and that in any case little depends on the exact value used. I shall then reply to the remaining comments on methods. Next I shall discuss the other issues raised—all are interesting and helpful but not every one calls for comment—and I shall finish with some remarks about the studies that we already have in hand of the health of the miner in 1961–62.

"Promotion" is a phenomenon which may be more or less confined to the mining industry, but it is important to realize that there it is to be expected. There is every reason for a man who works underground (or who is now a surface worker in the mining industry but who spent many years underground) to call himself a "miner", wearing the "badge of honour" as Mr Stevens, speaking from his experience in the National Union of Mineworkers, describes it. Men who are miners in every sense, except that, for strict Census purposes, their occupations are not classified as coalmining occupations, are a substantial minority (about 20 per cent.); and we know that a high proportion of men in the mining industry use the term "miner" or "N.C.B." without any attempt to encourage accurate statistical classification by distinguishing their specific occupations, whether mining occupations for Census purposes or not. Certainly, as Dr Heady points out, the study by Heasman *et al.* (1958) took no account of men actually in mining occupations who, when claiming sickness benefit, said that they were not in such occupations, but it is difficult to see why a miner should quote any other occupation for himself. Thus the direct assessment of 17 per cent. "promotion" would not appear to be overestimated to any great extent from this cause.

As to the indirect estimate from "reference diseases" I must accept the criticism by Dr Heady and Dr Raffle over diseases of the heart and arteries. Dr Raffle states that coronary thrombosis is less in occupations where there is much physical activity; if this is so, the inclusion of heart diseases as a "reference disease", would have led to an underestimate of "promotion". In fact, he is right as far as the data for 1951 are concerned, but wrong, albeit slightly, for 1955–56: the S.I.Rs. (long spells) for all the "reference diseases" other than diseases of the heart and arteries among coalminers, in these two statistical years, are found to be 137 and 116; their standard deviations are rather larger than before because the numbers of spells of incapacity on which the ratios are based have been reduced.

Thus, the available estimators of "promotion" into the mining industry are:

	1951	*1955–56*
Direct	117	—
Indirect		
"Reference diseases" as originally defined . . .	129	119†
"Reference diseases" but excluding diseases of the heart and arteries	137	116

† Dr Heady is correct in assuming this can be used as another estimator.

Of these, 117 is almost the lowest and it was this fairly conservative estimate that was used in the paper.

Further, the conclusions drawn after allowance has been made for "promotion" are very little affected by the particular estimate used. This is because all but one of the diseases listed in Table 4 fall into one or other of two classes. In the first class (common cold, influenza, bronchitis, muscular rheumatism, arthritis, hernia and tuberculosis) the S.I.Rs. (long spells) for coalminers would not appear significantly high (at the 5 per cent. level) even using an estimate of "promotion" as low as 15 per cent. At the other extreme, there is the second class (gastritis and duodenitis, peptic ulcer, disorders of the function of

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the stomach and skin diseases) in which the S.I.Rs. (long spells) for coalminers always appear significantly high, even if "promotion" were assessed as great as 70 per cent. Indeed, it is only with psychoses and psychoneuroses (of the diseases included in this study) that the estimate of "promotion", within the range 15 per cent. to 70 per cent., could affect a conclusion.

I agree with Mr Haycocks in his general dislike of index numbers, although it was pleasant to incur Dr Heady's approval. It seems to me that the index numbers I used are acceptable in their context here, because they are used for illustrative purposes and because it would have been virtually impossible to quote age-specific rates. In any case, for several of the diseases considered, the age-specific incapacity ratios were very similar for each age-group, so that any of the indices, based on any acceptable method of standardization, would have led to a similar result.

It is interesting that the "inverse" method of age-standardization, introduced to so many of the medical statisticians of this country by Mr Kerridge (1958), had long been known to actuaries. However, I do not understand why it should be considered as leading only to an index of populations, for it provides, without doubt, a weighted average of age-specific incapacity ratios. Nevertheless, as I emphasized in my earlier paper—and, indeed, intended to make the main conclusion—the drawbacks of using any single index are often major and can never be entirely overcome.

In reply to Mr Stewart's criticism of the exclusion from the 1955–56 study of spells not terminating in the year, I must remind him that this was intended only as a validation of the study on 1951 data. Although consideration of days of incapacity might have been rewarding, it was not considered worth the extra effort involved, in particular in having to use tabulators rather than sorters in the card-processing.

It was most encouraging to find my main conclusion, that most of the differences in sickness absence between occupations are largely explained by differences in physical demand, supported from the other studies discussed by Dr Raffle, Mr Cornwall, Mr Stewart and Mr Haycocks. As the last named points out, the consistently high rate of sickness among miners over a period of 60 years is quite remarkable and Mr Cornwall's assessment of the various potential causes of the difference in terms of stability over such a long period was most helpful. I was also glad to find no dissent from my suggestion that there was no evidence of malingering among miners. I thank Mr Wynn for his important comments on the concepts of malingering and of "true" morbidity.

The different views on social factors, expressed by Dr Heady and Mr Stevens, were very interesting. Although I am not quite as sanguine as Dr Heady in accepting the accuracy of the published figures of the mortality of miners' wives and infants, they do certainly require investigation. I did not examine the effects of social environment because this would not have been at all easy on the available data and not because I thought it unimportant, particularly in its long-term effects.

Several speakers referred to the problems of selection and survival of the mining population. I agree with Mr Wynn that the effects are far-reaching. We do not know the extent to which new entrants to coal mining are representative of the population of mining districts; although all new entrants are examined medically, we have been unable to obtain corresponding information about the rest of the population. It is interesting to reflect on whether the negative selection because of known hardships, suggested by Mr Wynn, is balanced in any way by the greater initial emotional stability among recruits, which Mr Stevens thinks likely. We know that there is much wastage, particularly among the youngest recruits, which is almost certainly, as Mr Wynn said, of the less fit, so that the "true" morbidity and mortality of miners may be close to average only because of these exclusions, as pointed out by Mr Wynn, Dr Heady and Mr Thomas, while Mr Thomas's comment that so-called voluntary absence may conceal even more sickness is also valid, although it is unlikely to conceal many long spells of sickness. Again, I think the points made by Mr Stevens on the effects of the underground environment on the health of ex-miners and of surface workers who once worked underground are of considerable

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importance. As to the assessment of the effects of selection on mortality, we are hoping to make some progress in a study, which is in hand, on deaths in 1961 which will be related not only to each man's last occupation but also to his occupation in 1952 (Liddell, 1961). It is perhaps worth pointing out that every occupational group will have corresponding, although smaller, problems of selection and that every population is, in one sense at least, a survivor population.

In the studies on incapacity for work which are being carried out in conjunction with the Ministry of Pensions and National Insurance, the most important improvement will be the accurate specification of populations and the elimination of any "promotion" effects. Secondly, we will be able to compare occupations within mining, which Mr Wynn rightly considers so necessary. However, I am afraid it will probably not prove practical to examine the effects of smoking or of pneumoconiosis on the general health of miners. Nevertheless, I can refer Mr Cornwall to the assessment of smoking habits, as part of the National Coal Board's Pneumoconiosis Field Research, based on a questionnaire to some 35,000 miners; the first results suggest that, although miners do not smoke when at work underground, their consumption of tobacco over the whole day is almost as great as among other men (Ashford *et al.*, 1961). Pneumoconiosis was not mentioned in the paper because the main concern was non-occupational sickness; such evidence as there is on the relationship between this condition and other respiratory conditions is inconclusive and, indeed, to some extent contradictory. Incidentally, the figures quoted by Mr Stewart are, of course, of pneumoconiotics in benefit and do not accurately reflect the prevalence of the disease itself; this is now obtained as a result of the Board's Periodic X-ray Scheme in which well over 250,000 men have already been X-rayed (National Coal Board, 1961).

Finally, I should like to thank all who took part in the discussion for their most valuable and stimulating contributions. I found it extremely encouraging to have my conclusions supported, at least in broad terms, by a doctor, a member of the National Union of Mineworkers, and my Board Member, as well as by some of my professional colleagues.

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As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Aczel, John Anthony, B.A., M.Sc.(Econ.)
Armstrong, Robert Harold Rowland, B.Sc.(Econ.)
Bates, James Arthur, B.A., Ph.D.
Baxter, Eric George, M.A.
Benedictus, Simon Ricardo, A.R.C.S., M.Sc.
Bennett, Blair Miller, A.B., M.A.
Brereton, Ralph, B.Sc.(Maths.)
Brillinger, David Ross, B.A., M.A., Ph.D.
Brinton, Hugh Pennock, A.B., M.A., Ph.D.
Calder, Allan Balfour, B.Sc., Ph.D.
Cohen, Alan Max, B.Sc., M.Sc.
Cone, James Frederick, B.Sc.(Maths.)
Constable, Kathleen Marie, M.A., B.Sc.
Cook, Charles Ronald, B.Com.
Crookes, Peter Charles Irwin, M.Sc.(Maths.), M.Sc.(Stats.)
Dew, Roger Eric, B.Sc.(Econ.)
Dobner, Frank William
Dunlop, Anthony Edward
Dunnett, Charles William, B.A., M.A., D.Sc.
Dunning, Donald Frederick

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EARNSHAW, Angus Wemyss, B.Sc.
ELLIOTT, John Frederick, M.A.
ETCHELLS, Alan, B.A., B.Sc.(Econ.)
FAGG, Joan Rosalie
FEATHERSTONE, Michael Sheriff, B.A.(Psychol.)
FELDMAN, Jacob J.
FINCH, Peter Derrick, B.A.
GAINSBRUGH, Martin R., A.B.
GART, John Jacob, Ph.D.
GILFORD, Dorothy Morrow, B.S., M.S.
GILFORD, Leon, A.B., A.M.
GILLARD, Donald Austen, B.A.
GREENBERG, Bernard George, D.Phil., B.Sc.
GRICE, Hugh Barron
HAIDER, Adnan M., M.Sc., B.Sc.
HALL, William Jackson, A.B., M.A., Ph.D.
HAWLEY, Francis James, B.Sc.(Hons. Maths.), M.Sc.
HEADS, John, M.A., M.Sc.
HENRY, Basil Gordon Compton
HOLE, Geoffrey John Charles, B.Sc.
HUNTER, Charles Finch
ITKIN, Arthur Gerald, B.A., M.S.
JOHNSTON, Edward Alexander, B.A.(Maths.)
KEILSON, Julian, B.S., M.A., Ph.D.
KERNER, Hans, B.Sc.
KOLATKAR, Gopalrao Madhavrao, B.Comm.
LEWIS, Thomas William, B.Sc.(Econ.)
LIEBERMAN, John Brian
LIMERICK, Randolph Mortimer
LOYNES, Robert Michael, B.A., Dip. Math. Stats.
MCKENZIE, Ransford Edison
MALIK, Azizul, M.A.
MERRETT, John Desmond, B.Sc.
OGILVIE, John Charles, B.Sc., M.A., Ph.D.
ORRETT, John Ralph Denning, M.A.
OWEN, Donald Phillip
PADDLE, Geoffrey Michael, B.Sc., Ph.D., D.I.C.
PALMER, Joseph
PRATT, Clemens William
QUREISHI, Abdus Salam, B.Sc., M.Sc.(Stats.)
RADLEY, Bruce Ernest
RAO, J. N. K., Ph.D., M.A.
RAY, George Frank, B.Sc.(Econ.)
REDFERN, Gordon, B.Sc.
RILEY, Donald Crockett, B.S., A.M.
ROGERSON, George William, B.Sc., B.A.(Hons.)
ROUND, Anthony Paul, B.A., Dip. Math. Stat.
SARGAN, John Denis, M.A.
SESHAN, Madhava Iyengar
SHERIDAN, Anthony David, B.Sc.(Econ.)
SILMAN, Audrey
SPROTT, David, B.A., M.A., Ph.D.
SUBRAHMANIAM, K., M.Sc.
TALLIS, George Michael, B.Sc., M.Sc., Ph.D.
VASSILIOU, George Vasos, Dipl.Econ., Dr.Econ.
VAUGHAN, Kenneth Valentine
WARD, Michael Peter, B.A.
WEBSTER, Lucy, B.A.
WILKINSON, Graham Neil, B.Sc., B.Sc.(Hons.), M.Sc.
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Sampling Inspection and Defence Specification DEF-131

By I. D. Hill†

War Office

[Read before the Royal Statistical Society on Wednesday, December 13th, 1961, Mr H. Tetley, C.B., Vice-President, in the Chair]

Summary

Defence Specification DEF-131, *Sampling Procedures and Tables for Inspection by Attributes*, has recently been published by Her Majesty's Stationery Office, and is based upon the American Military Standard 105. This paper discusses the features of the Dodge and Romig tables, the Army Service Forces tables, and the Columbia University Statistical Research Group tables, as well as the Military Standard 105 tables, which were considered before the decision was taken to use the latter document as a basis.

The differences between Military Standard 105 and DEF-131 are discussed in detail, with the reasons for the changes.

1. Introduction

I should like, if I may, to distinguish between a sampling scheme and a sampling plan, although it is always dangerous to try to force new definitions on to words which are already in use. According to Tippett (1958) "sampling plan" is merely the American for "sampling scheme", yet "plan" is a perfectly good English word, and there seems to me to be no good reason against following the American usage in this instance, whereas there is a need for a word to express the concept of what I wish to call a sampling scheme.

By a sampling plan, then, I shall mean the specification of the rules to be followed in sentencing any particular batch of articles. For instance, specification of single sampling together with a sample size and acceptance number would form a sampling plan; specification of sequential sampling together with values of the parameters known as the handicap, the penalty and the target would also form a sampling plan.

By a sampling scheme, I shall mean an overall strategy specifying the way in which sampling plans are to be used. Thus it may be said that Dodge and Romig's LTPD tables, Dodge and Romig's AOQL tables, and the Military Standard 105 tables form three separate sampling schemes, while containing several hundred sampling plans.

In this paper I shall be discussing and comparing various sampling schemes.

Initially it should be noted that the study of sampling plans may be thought of as a purely mathematical exercise. When a plan has been specified, its implications, in such things as operating characteristic curve, average sample size curve, etc., may be calculated. This is purely a matter of mathematics and there is then no more to be said. If two statisticians were to disagree about the implications of a sampling plan it would be necessary only to check their mathematics to find out who was right and who was wrong. A sampling plan, in itself, is merely something which exists; it cannot be said to be a good thing or a bad thing, until it is incorporated in a sampling scheme.

The study of sampling schemes, on the other hand, is not solely a mathematical question. It is a matter of art, opinion, aesthetic sense and compromise as well as of

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science and mathematics, and it is this that gives the subject much of its fascination. If two statisticians disagree about the merits of a sampling scheme it is not possible, in any simple way, to check who is right and who is wrong. There are bound to be differences between any two people who think about the subject hard enough.

As a result, it is desirable that those who are responsible for the publication of a sampling scheme should also publish some statement of the reasons behind the decisions that have been taken—reasons which are not always obvious from a study of the sampling scheme itself. This paper has been written to satisfy this requirement in the case of Defence Specification DEF-131.

2. The Origin of DEF-131

Defence Specification DEF-131, *Sampling Procedures and Tables for Inspection by Attributes*, was produced by the "Acceptance Sampling by Attributes" sub-committee of the Engineering Standards Co-ordinating Committee of the Ministry of Defence. This sub-committee was set up "to examine the Services' need for common methods of sampling by attributes, taking into account the procedures made mandatory on the U.S. Armed Services by MIL-STD-105A" and "to prepare, if considered necessary, a Joint Services Guide on these procedures for the use of Inspectorates and others concerned".

In addition to DEF-131, this sub-committee has also produced Defence Guide DG-7, entitled *Sampling Inspection. A Guide to the Use of Defence Specification DEF-131*, which is designed to be intelligible to the inspector without statistical knowledge.

In setting about its task the sub-committee decided that in addition to considering MIL-STD-105A it would be advisable to consider some of the tables that preceded it, so that comparisons could be made, and it will be necessary to follow the same procedure now if the decisions reached are to be adequately explained.

3. Dodge and Romig Schemes

In their pioneering work of the 1920s and 1930s Dodge and Romig introduced the concepts of Lot Tolerance Percent Defective (LTPD) and Average Outgoing Quality Limit (AOQL), which later formed the basis of their Sampling Inspection Tables, (1959), first published in 1941. It is a measure of the soundness of their thinking in the early days of the subject that their concepts and tables are still in use today, but these concepts are applicable principally to the manufacturer inspecting his own product, and either accepting on the sampling results, or rectifying the batch and then accepting, which was what Dodge and Romig's tables were primarily designed for, and difficulties arise if they are used for the rather different case of a government inspectorate's acceptance procedure, which may often involve outright rejection rather than rectification.

In the following paragraphs the word "rejection" will be used, since we are considering what would happen if Dodge and Romig's tables were used for acceptance or rejection, rather than for acceptance or rectification. The comments made should not be taken as necessarily valid when the Dodge and Romig tables are used as originally intended.

3.1. *Lot Tolerance Percent Defective*

The LTPD scheme assumes the designation of a numerical value of lot tolerance, defined as "some chosen value of limiting percent defective in a lot". It should be

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noted that this definition refers to the quality required in the product and is not a description of the sampling plan, or of a point on its operating characteristic, (O.C.), curve. It is only when a "consumer's risk" is chosen to go with the LTPD that a point on the O.C. curve is obtained. This consumer's risk could take any value between 0 and 1, but it would be unusual to use the term LTPD if a consumer's risk of more than 0·5 were chosen. The fact that Dodge and Romig, in their tables, took a fixed consumer's risk of 0·1 has led to the common notion that LTPD is defined as the quality for which the sampling plan gives a probability of acceptance of 0·1. This changes the concept from a requirement in the product to a description of the sampling plan, and is certainly opposed to the original intention.

The difficulty with the LTPD concept is that the value chosen for the LTPD is usually the value that the consumer considers to be a border-line between what is acceptable to him and what is not. If, however, a low value of consumer's risk is then chosen (usually 0·1 or less) it leads to the situation where the quality offered may be considerably better than this border-line and yet a large proportion of the batches offered may be rejected. If the production and inspection system is to operate reasonably well, this forces the producer to offer a quality much better than the LTPD. This is implicit in Dodge and Romig's tables since the process average cannot be more than half the LTPD if an entry is to be found in the tables at all.

This system leads then, in general, to the producer making, and the consumer receiving, a quality considerably better than is really necessary, and the price of course must reflect this.

It may be argued that this means only that people use such a scheme without a full knowledge of what is involved. After all, whatever sampling plan is chosen, by whatever method, the resulting O.C. curve must have some quality corresponding to any chosen value of consumer's risk, and the same plan could have been chosen by seeking to attain this point on the O.C. curve rather than the other way round. This is undoubtedly true, but unfortunately a full knowledge of what is involved is, all too often, not possessed by those responsible for selecting a plan, and the use of the LTPD criterion does seem to mislead people into asking for a much tighter quality than is really required.

I do not deny—I have no wish to deny—that the situation sometimes arises where the primary requirement is a high chance of rejection, if quality as bad as some fixed value should be offered, but I do strongly suggest that this situation is the exception rather than the rule. In general, what the producer needs to know is what quality he must make so as to have a high proportion of his output accepted. What the consumer needs to know is what the producer's process average is likely to be, for it is the process average rather than any arbitrary point on the O.C. curve that primarily determines the quality the consumer is going to get.

Now, in normal situations, the process average corresponds to a high point on the O.C. curve; it has to, if the producer is going to make a profit. So it is really a high point rather than a low point which is the primary concern of both producer and consumer. This does not stop attention from being paid to a low point also, as a secondary consideration, but tables based upon the LTPD approach are not really suitable for this.

3.2. *Average Outgoing Quality Limit*

The AOQL approach does not work in terms of fixed probabilities on the O.C. curve but instead seeks to guarantee the average outgoing quality. If all rejected

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batches are sorted 100 per cent. and accepted after rectification, the average outgoing quality will be better than the incoming quality because of the rectification taking place, and for any given sampling plan there is a limiting value to the average outgoing quality, whatever may happen to the incoming quality, because if the incoming quality worsens beyond a certain point, the extra amount of rectification necessary is more than sufficient to compensate. It should be noted, however, that to achieve this result it is required that all rejected batches have to be 100 per cent. sorted, and that this 100 per cent. sorting has to be perfectly done. Now it is well known that in practice 100 per cent. sorting is unlikely to be perfectly done—indeed this is often given as a reason for using sampling methods. It is sometimes claimed that sampling is less likely to suffer from faulty inspection than is 100 per cent. inspection, and there may be some circumstances in which this is true, but quite often, to the actual inspectors, there is no difference between 100 per cent. and sampling except that fewer of them are employed for sampling and in such cases I can see no reason why the faults observed in 100 per cent. inspection should not also occur in sampling inspection.

In stressing, therefore, that the AOQL concept requires perfection in the inspection operation, I am not claiming that other methods do not require this. It is merely that the lack of such perfection seems to matter more in the case of AOQL, as the following example indicates.

Let the sample size be n, the acceptance number be c, and the submitted proportion defective be p. Let r be the probability that a defective article is called defective when inspected. Then, if we imagine that the inspection is perfect, we believe that the O.C. curve is given by

$$P = \sum_{k=0}^{c} \frac{n!}{k!(n-k)!} p^k (1-p)^{n-k},$$

whereas in fact it is given by

$$P' = \sum_{k=0}^{c} \frac{n!}{k!(n-k)!} (pr)^k (1-pr)^{n-k}.$$

In other words, the curve is shifted to the right, so that the distance of each point from the vertical axis is increased in the ratio $1/r$. Thus if $r = 0{\cdot}9$, the proportion defective at which a given probability of acceptance occurs is increased by approximately 11 per cent., as is shown in Fig. 1 for the case $n = 1{,}000$, $c = 3$. Whether this is highly important depends upon the circumstances of the particular case. (Experience suggests that a value of $r = 0{\cdot}9$ is not unreasonable for visual inspection. It is unlikely that r would be constant for all values of p, but to assume it to be constant is not likely to be seriously misleading for present purposes.)

If we imagine that the inspection is perfect, we believe that the AOQ curve is given by

$$\text{AOQ} = pP,$$

whereas in fact it is given by

$$\begin{aligned} \text{AOQ}' &= pP' + (1-r)p(1-P') \\ &= p\{1-r(1-P')\} \end{aligned}$$

Again for the case $n = 1{,}000, c = 3, r = 0{\cdot}9$, the result is shown in Fig. 2 (which is plotted on a square-root scale on each axis, so as to show the left-hand end better). The "maximum" on the curve is 0·00225, i.e. 16 per cent. higher than the assumed AOQL of 0·00194. Again, whether this is highly important depends upon the circumstances of the particular case, but the discrepancy is half as much again as the 11 per

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cent. observed with the O.C. curve. In addition, however, the curve now has a "minimum" point after the "maximum" and then rises continually until the AOQ reaches $1-r$ when $p = 1{\cdot}0$.

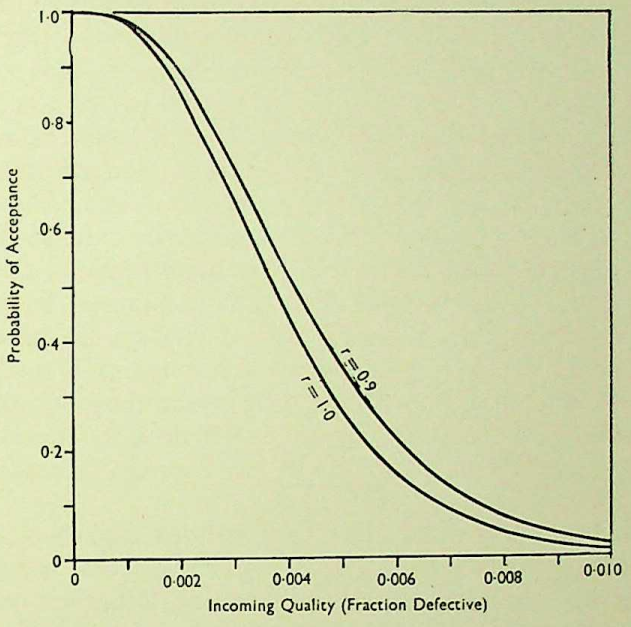

FIG. 1. Operating characteristic curves, $n = 1{,}000$, $c = 3$, $r = 1{\cdot}0$ and $n = 1{,}000$, $c = 3$, $r = 0{\cdot}9$ (where n is sample size, c is acceptance number, and r is probability that a defective article is called defective when inspected).

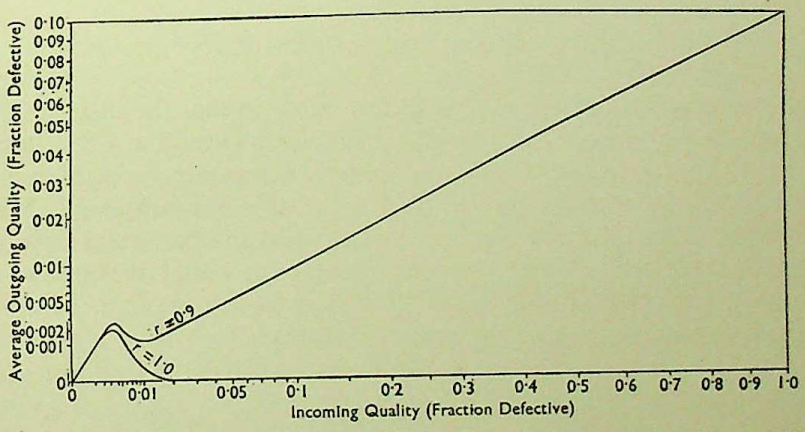

FIG. 2. Average outgoing quality curves, $n = 1{,}000$, $c = 3$, $r = 1{\cdot}0$ and $n = 1{,}000$, $c = 3$, $r = 0{\cdot}9$ (where n is sample size, c is acceptance number, and r is probability that a defective article is called defective when inspected).

I am assuming that defectives found are repaired rather than replaced, for replacement might involve an additional hazard in that some of the items used for replacement might mistakenly be defective. (It should be noted, however, that repairing also has its hazards.)

It might be argued that the right-hand end of the curve is relevant only when all batches are being rejected anyway, so that to attain at least an approximation to the

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stated AOQL it is necessary merely to add the rule that some batches must be accepted on the sampling results, otherwise inspection must cease pending process improvement. This would be so if we could assume that the process was in statistical control, so that we had a binomial distribution of defectives, but I find difficulty in regarding an AOQL which requires the process to be in control as a genuine AOQL at all. As an example of the sort of situation we might have in the absence of control, suppose that, on the average, of every five batches, four come from a process giving

TABLE 1

Probability of acceptance when the submitted quality is equal to the AOQL, assuming single sampling and the Poisson distribution of number of defectives in the sample

Acceptance number	*Probability of acceptance*
0	0·692
1	0·794
2	0·840
3	0·867
4	0·885
5	0·898
10	0·932
15	0·947
20	0·955

$p = 0{\cdot}002$ and the other one from a process giving $p = 0{\cdot}2$, then the batches from the first process will have an AOQ of 0·0018, those from the second process an AOQ of 0·02, and the overall AOQ will be 0·00544, or 2·8 times the assumed AOQL. Yet 71 per cent. of the submitted batches would be accepted on the sampling results. It is true that to get this result I have used rather an exaggerated case, namely a large sample size and a small acceptance number to give a very tight assumed AOQL, a value of r which indicates rather slack working, and incoming batches of two very different qualities. The figure of 2·8 times the AOQL should therefore not be taken too seriously, but the principle is the same in other cases. The fact that, in the absence of perfect inspection, the AOQL is not the absolute guarantee it is often thought to be is worth noting.

In deciding on the form that DEF-131 should take, the use of AOQL was decided against not so much upon the above argument as upon the fact that the concept of AOQL requires that all rejected batches shall be 100 per cent. sorted. In practice this often is done, but it was wished to have tables which were equally applicable whether it is or not.

One possible suggestion would be to adopt the AOQL simply as a means of indexing the tables, remembering that if a batch is offered of a quality equal to the AOQL the chance of acceptance is fairly high, but for the small acceptance numbers which are used most often in practice the chance is not really high enough for this to be a satisfactory approach, as is shown in Table 1.

This table shows that the use of an AOQL will, in general, force the producer to offer a quality a good deal better than the AOQL value, although not to such an extent as in the LTPD approach. To use the Dodge and Romig tables for AOQL

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it is necessary for the process average to be at least as good as the AOQL. If this is so, then the consumer is getting the quality he desires simply from this fact and one is rather left wondering what all the fuss is about, in setting up a sampling scheme to ensure what is happening anyway.

An additional point, from a government inspectorate's point of view, against the adoption of the Dodge and Romig tables as such (although not necessarily against the adoption of the LTPD or AOQL concepts) is that these tables are based upon the semi-economic approach, of minimizing the total quantity of inspection, within the constraint imposed by the chosen LTPD or AOQL criterion—a policy which may be thoroughly justified for a manufacturer inspecting his own product, but which can lead to undesirable consequences for a government inspectorate, such as having two manufacturers for a given item, and accepting a higher proportion of the production of the worse manufacturer than of the better one, a situation which I have described elsewhere (Hill, 1960).

Both the LTPD and the AOQL concepts are capable of giving good protection against poor quality, provided they are used efficiently, but both do so at the cost of rejecting a good deal of satisfactory production.

4. United States Army Schemes

In 1942 the U.S. Army started to use the Army Ordnance Tables, which with some modifications became the Army Service Forces, or ASF, Tables of 1944. These tables introduced the concept of Acceptable Quality Level—a useful concept but a poor choice of name since the initials AQL have inevitably been confused with AOQL. AQL was defined as "the maximum per cent defective which can be considered satisfactory as a process average", a definition which clearly refers to the quality required in the product and is not a description of the sampling plan to be used. This definition is very similar to that of LTPD, except that LTPD refers to a limiting quality for an individual batch, AQL to a limiting quality for the process average. The tables, however, make very different uses of them.

Whereas Dodge and Romig's LTPD tables give a high chance of rejection for any batch of a quality worse than the LTPD, the ASF tables give a high chance of acceptance for any batch of a quality better than the AQL. The producer, not the consumer, is given the benefit of the doubt introduced by the sampling variability. On the face of it, this sounds a bad thing from the consumer's point of view, but it must be remembered that the quality the consumer gets does not depend solely, or even mainly, upon the O.C. curve of the sampling plan, but upon the process average of the submitted product. It is therefore part of the system that a watch must be kept on the sampling results to see whether the process average is as good as the AQL. (Calculations for this purpose are discussed in more detail in Section 8.5.) If the process average is as good as the AQL, then the consumer is getting the quality he requires. If, however, at any time the process average is significantly worse than the AQL, tightened inspection must be installed to protect the consumer. In the ASF tables tightened inspection consists of changing to a sampling plan with an AOQL not greater than the AQL, and the limit for deciding whether the process average is significantly worse than the AQL is based upon "3 sigma" limits of the binomial distribution. It should be noted that this use of the AOQL, only when tightened inspection is called for, does not ensure that the true AOQ, over a succession of periods of normal and tightened inspection, is necessarily better than the AQL, even if perfect inspection is assumed.

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An interesting form of reduced inspection is also included, for use when the quality record is good, which employs double sampling using a first sample one-fifth the size of the first sample for normal inspection double sampling, but a second sample, where required, which makes the total sample up to the total of the two samples for normal inspection.

The values of AQL are given as ranges and not as point-values. The chance of acceptance at the AQL for normal inspection varies from one plan to another without any clear pattern in the variation, and differs for defects classified as major and those classified as minor. For a major defect with an AQL at the top of a range the chance of acceptance varies between about 0·95 and 0·99; for a minor defect with an AQL at the bottom of a range the chance of acceptance varies from about 0·999 to well over 0·999999.

The Dodge and Romig semi-economic principle is abandoned in these tables, and something is needed to replace it since a single sampling plan has two parameters and the specification of an AQL and a probability of acceptance at the AQL cannot, on its own, determine these. The ASF tables resolve the problem by giving a purely arbitrary fixed relationship between batch size and sample size, such that as batch size increases sample size also increases but the ratio of sample size to batch size decreases. There is much to be said on intuitive grounds in favour of taking larger samples from larger batches, since there is more at stake where a large batch is concerned so it is more important to do the right thing with it, and from a large batch a large sample can be afforded. In addition, sampling plans designed on an economic or semi-economic theory always seem to have this feature (e.g. Dodge and Romig, 1959; Horsnell, 1957; Hald, 1960). But, of course, there is much to be said against making such a feature fixed and arbitrary if something better could be devised. With the ASF tables it was said that, at the time they were produced, simplicity of operation was the vital factor and that the arbitrariness could be accepted in order to attain this.

A further arbitrary feature of the ASF tables is in the double sampling tables where the rules are introduced that the first sample is to be two-thirds of the sample size of the equivalent single sampling plan, that the second sample is to be twice the first, and (as in Dodge and Romig) that the first and second rejection numbers are to be identical. These rules reduce the number of parameters at choice from five to two, thus making the task of fixing the parameters for a particular case much simpler, but they have some undesirable features as will be shown later in this paper.

5. The United States Navy Scheme

The U.S. Navy adopted sampling procedures in 1945, basing their tables upon the work of the Columbia University Statistical Research Group which was published as the book *Sampling Inspection* (Freeman *et al.*, 1948). These tables are usually known as the SRG tables.

They follow the ASF tables in many ways, namely, indexing by AQL, use of normal, tightened and reduced inspection, and the sample size determined by the batch size. In the details, however, there are considerable differences.

One of the main differences is that the definition of AQL is changed to "The percentage of defective items in an inspection lot such that the sampling plan will result in the acceptance of 95 per cent. of submitted inspection lots containing that percentage of defective items". This change is most regrettable, and has led to much confusion. There is, of course, no reason why the SRG tables should not be indexed

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in terms of the 0·95 chance of acceptance, but why could not a different name have been chosen? The concept of AQL is entirely changed, from a requirement in the product to a description of the sampling plan. As an example of the sort of confusion that this produces, consider a change from normal to tightened inspection. In the ASF tables the sampling plan is tightened, but the AQL remains the same; in the SRG tables, to tighten the sampling plan it is necessary to change the AQL.

As in the ASF tables the AQLs are given in the form of ranges, although there is, of course, only one point in the range, in each case, which will exactly satisfy the definition of AQL.

In the ASF tables the relationship between batch size and sample size is fixed. In the SRG tables a choice is given between five "Inspection Levels". Each inspection level gives a different arbitrary relationship between batch size and sample size, although all follow the same pattern of increasing the sample size as the batch size increases, but reducing the ratio of sample size to batch size.

It should be noted that these inspection levels are not the same thing as the severity of inspection as given by the use of normal or tightened plans. The inspection level is defined in advance, and fixes the sample size for a given batch size; the use of normal or tightened inspection then fixes the acceptance number.

The SRG tables do not give much help with the question of how to choose an inspection level. Level III is said to give the "normal amount" of inspection, but other levels may be fixed to give from a half to twice this amount. In choosing, it is said that factors for consideration are "(a) the risk that can be run of accepting inspection lots whose percent defective is worse than the AQL. If a small risk is desired, the inspection level must be high; (b) availability of inspectors and inspection facilities. If these are limited, the amount of inspection must be small and the inspection level consequently low."

In the ASF tables tightened inspection is provided by using a plan with an AOQL not greater than the AQL. In practice it was found that the consumer's protection lay not so much in the value of this AOQL as in the producer's desire to avoid tightened inspection and his efforts to keep his process average at least as good as the AQL for this reason. The SRG tables drop the AOQL basis of tightened inspection and, instead, use the acceptance and rejection numbers given for normal inspection of a tighter AQL value. The switch is made by using the column in the tables two to the left of the column for normal inspection. This does not have a very uniform result—the AQL (in the SRG sense) may be changed to about two-thirds of its initial value, or it may be as little as a fifth of its initial value, or somewhere between these limits. Where acceptance numbers are large, the change can be very severe indeed—a normal acceptance number of 24, for example, can be tightened to 8. In addition, the upper limit for the process average, the exceeding of which leads to the adoption of tightened inspection, is reduced from "3 sigma" to "1·96 sigma".

For reduced inspection the SRG tables keep the AQL fixed but change the inspection level to give a smaller sample size. This has the disadvantage that the chance of acceptance at a quality better than the AQL (where the production presumably is if reduced inspection is in use) is reduced. This penalizes the producer for submitting such good quality.

A disadvantage of the SRG tables that became clear with use is that for a given AQL range different sample sizes are sometimes given with the same acceptance number. This means that the O.C. curve for the smaller sample size will lie above the curve for the larger size throughout its entire length, and that a producer can

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always do better, whatever his quality, by reducing his batch size to give the smaller sample size.

A welcome feature of the SRG tables is the fact that multiple sampling is given in addition to single and double sampling. (In these tables it is called sequential sampling—a name which, since the time when these tables were compiled, has generally been reserved for the case where the successive samples are of only one item each.)

The SRG tables are often considered outmoded nowadays, because other things have come along since their day, but it should be emphasized that although they may be outmoded as a sampling scheme they never can be outmoded as a collection of sampling plans (except that the double plans are inefficient because of the arbitrary constraints put upon them) and their O.C. curves. The book *Sampling Inspection* will always be worth having for this collection, and also for a great deal of the text, namely all that part where sampling plans are being discussed as distinct from the particular sampling scheme put forward.

After the war the American armed services were combined for certain purposes under the Department of Defense, which, in 1949, adopted the SRG tables under the new name Joint Army Navy Standard 105 (abbreviated JAN-STD-105). This was superseded in 1950 by Military Standard 105A (MIL-STD-105A), which in turn was superseded in 1958 by MIL-STD-105B.

6. Military Standard 105

The differences between MIL-STD-105A and MIL-STD-105B are very slight, consisting mainly of changes in layout, alterations to the wording of the text to make the document more suitable for use other than by the American Government, and the incorporation into 105B of the Appendix to 105A (issued in 1955). It will be convenient therefore to use the name MIL-STD-105 to include both versions.

These tables continue to employ the system of AQL, inspection level, and normal, tightened and reduced inspection.

AQL is again re-defined. It seems that the U.S. Army and the U.S. Navy could not agree on what the meaning should be, and as a compromise a very nebulous definition was chosen which does not appear to have any precise meaning. The AQL is said to be "a nominal value . . . specified for a given group of defects of a product". Although this does not tell us much, an examination of the tables themselves makes it clear that the SRG concept of the AQL, as the quality giving a 0·95 chance of acceptance, has been dropped and the AQL has returned to being considered as a requirement in the product rather than a description of the sampling plan.

There are good reasons for abandoning the rule that batches of a quality equal to the AQL should have a 0·95 (or any other fixed) chance of acceptance, since this fixes the producer's risk for all sample sizes and leaves the consumer's risk to take up all the variation introduced by different sample sizes. This means that where the sample size is small in relation to the specified AQL, the risks taken by the consumer are unduly large; it would be more reasonable to require the producer to bear some of the extra risk. On the other hand, where the sample size is large in relation to the specified AQL, such plans are needlessly severe to the producer; the chance of acceptance at the AQL can be increased considerably while still giving the consumer good protection.

In MIL-STD-105 the chance of acceptance at the AQL varies from about 0·80 for the smallest sample sizes to over 0·99 for the largest. This shares the risks more

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equitably between the producer and the consumer, and has the additional advantage of giving the producer an incentive to supply large batches, since sample size depends upon batch size, and large batches therefore give him a higher chance of acceptance provided his quality is good enough.

The values of AQL, which in the ASF and SRG tables were given in the form of ranges, are now given instead as a series of point values forming an approximate geometric progression with a common ratio of the fifth root of ten. This forms a useful set of values, but the progression is broken at one point where the value 0·035 per cent. is used and 0·025 per cent. and 0·040 per cent. are omitted. A table is given for converting AQLs other than the given values to one of the given ones.

The maximum AQL was 5 per cent. defective in the ASF tables and 8·5 per cent. defective in the SRG tables, but in MIL-STD-105 values right up to 1,000 are included. This is explained by the fact that these tables are designed to be used either for "percent defective" or for "defects per hundred units", and whereas 1,000 per cent. defective is impossible, 1,000 defects per hundred units (an average of 10 defects per unit) is quite possible and, where the unit is something large and the defects are of minor importance, might be a perfectly reasonable quality level.

Tightened inspection is performed in these tables by changing to the normal plan for the next tighter value of AQL. In general, this has the effect of reducing the acceptance number while keeping the sample size the same. The only exceptions to this occur where the normal acceptance number is 0 or 1; in these cases tightening consists of keeping the acceptance number the same and increasing the sample size. Over most of the table the MIL-STD-105 method does not give such a sharp degree of tightening as does the SRG method, but the amount of tightening is very variable. For example, the producer might hardly notice if the consumer tightened the acceptance number from 5 to 4 (as happens, for example, with AQL 1·0 and code letter M), whereas the effect of tightening the acceptance number from 184 to 124 (AQL 10·0 and code letter Q) might well be catastrophic. This method of tightening was devised more on the grounds of administrative convenience than of strict logic, but it should again be remembered that the fact that there is any tightening at all, and that the decision to tighten is based upon the quality of production, is far more important than the exact degree of tightening. To make sense of this tightening method it is necessary to look at it in terms of the guarantee to the producer, that most of his batches will be accepted, while on normal inspection, provided his quality is at least as good as the AQL. When on tightened inspection he loses this guarantee, but keeps an assurance that most batches will be accepted provided his quality is at least as good as two-thirds of the AQL (approximately). Within the limits of this guarantee the consumer may tighten as much as he wishes.

The upper limit to the estimate of process average, which is used for deciding when tightened inspection should be applied, returns to the ASF value of "3 sigma". Presumably the SRG "1·96 sigma" was found to lead to resorting needlessly to tightened inspection too frequently.

Reduced inspection is also included, but in this scheme it is done by means of a separate table, not by changing the inspection level. This table is designed so that the producer does not lose his high chance of acceptance when reduced inspection is introduced. However, there are a few entries in the table where this aim is not achieved. In all the schemes being discussed it should be noted that reduced inspection is optional, as distinct from tightened inspection which is an essential part of the scheme; also that tightened and reduced inspection are not really opposites as they are often

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thought to be—the opposite of tightened inspection would be loosened inspection, i.e. the same sample size with a higher acceptance number, whereas the opposite of reduced inspection would be increased inspection, i.e. a larger sample size.

MIL-STD-105 follows the SRG tables in giving corresponding single, double and multiple plans. For the double sampling plans the same arbitrary rules are adhered to, as introduced in the ASF tables, and continued through the SRG tables. Only single sampling is given for reduced inspection.

These tables also follow the SRG tables in giving a choice of inspection levels by means of which the sample size is tied to the batch size. The number of levels is cut from five to three, but the appendix introduces eight additional levels for use when "relatively small sample sizes are desirable or are necessitated by some aspects of inspection", provided certain conditions are met.

The tables overcome one of the disadvantages of the SRG tables. For a given AQL, no more than one sample size is available for any given acceptance number, which means that the O.C. curves for two plans for the same AQL always cross at some point along their length. The only exception to this is in the reduced sampling table, where the point is not of such importance, since the main object of this table is to keep a watch on the process average rather than to accept or reject individual batches. No more than one batch can ever be rejected anyway during a run of reduced inspection, since one rejection causes the re-introduction of normal inspection.

7. Defence Specification DEF-131. Main Principles

When the "Acceptance Sampling" sub-committee of the E.S.C.C. set about their task, they had to decide, after looking at all these American tables, whether to adopt the concept of LTPD, AOQL, or AQL, or whether to design some other type of scheme. It was decided to concentrate at first upon the case where a sequence of batches is to be offered for acceptance at the time of production, and that the case of the odd batch on its own, or of the inspection of stores at some time later than the time of production, would be considered separately at a later date. For this purpose it was decided that the type of scheme wanted was that which would give the best probability of the consumer getting the quality he desired at a price he was prepared to pay. It was decided that in attaining this aim the most essential requirement is that the quality of production must be right. Given that the quality of production is good enough, the quality received by the consumer must be good enough, whereas if the quality of production is not good enough it might be possible to get the right quality eventually by sorting but, even where this is possible, the price must be excessive. The prime aim of the scheme should therefore be to give the producer the incentive to produce an acceptable product.

This idea, of the economic incentive aspect of sampling inspection, I have developed elsewhere (Hill, 1960), and it should be noted that the basis is entirely different from the type of economic scheme that is based upon a complete knowledge of prior distributions and costs. It is not the aim here to reach an optimum from the cost point of view within the confines of a fixed prior distribution. It is the aim that the sampling scheme adopted should influence the prior distribution.

From this point of view the economic optimum type of scheme fails to meet the requirements, for, apart from the difficulties of estimating what the prior distribution is at any one time, it cannot be expected that this distribution will remain fixed, nor can it be expected that two manufacturers of the same article will give the same prior distribution. If they do not, what then? Are we to apply the same sampling plan

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to both? If we do, it cannot be optimum for both and the theory is compromised. If we do not, the differences between the optimum plans will often be grossly unfair as between one manufacturer and the other. It might be possible to overcome some of the difficulties by building an economic incentive into such a scheme, by arranging for differential payments in such a way that a producer whose product was good enough to enable the sample size of the optimum plan to be reduced should receive some of the benefit of this by being paid a higher price for his output. Such a device, however, would cause difficulties with the writing of contracts, and would call for a considerable degree of statistical knowledge among the manufacturers if they were to understand what was going on, and why.

There is everything to be said in favour of an experiment along these lines, in a suitable case, to investigate the workings of the economic optimum type of scheme in practice, and to demonstrate whether the difficulties are as great as they may appear to be. In the absence of such an experiment, I remain to be convinced that the problem, of what sampling inspection should seek to do, is really soluble along the lines of the economic optimum.

The AQL concept, together with the tightened inspection device, as used in MIL-STD-105, while it has several arbitrary features, does seem to me, and seemed also to the "Acceptance Sampling" sub-committee, to be designed along the right lines to act as the economic incentive that is desired. It is assumed that the producer wishes to make a profit, and that he is unable to do so unless a high proportion of his batches is accepted. This forces him to produce a quality of the order of the AQL, or better.

Two points should be noted: the first is that this approach assumes that a high proportion of rejected batches is uncomfortable for the producer. In practice this nearly always is so, but a watch needs to be kept for the rare case when it is not. An example is to be found in conditions of raw-material shortage, where a producer may be allowed supplies of the raw material concerned solely because he is producing for the Government. There may then arise the case where a rejected batch can be sold to the general population for a higher price than the Government would have paid. In such circumstances any form of inspection is likely to get into trouble unless special provisions have been written into the contract to allow for the situation.

The second is that indexing the plans by AQL does not prevent the consumer from choosing a plan by means of an LTPD approach (choosing a low point on the O.C. curve) on the occasion when it is really necessary to do so. But it does ensure that, having done so, he does not quote this quality, with a low chance of acceptance, to the producer as the border-line of the quality he requires. He has to translate the chosen plan into the terms of the tables and tell the producer the AQL, which is what the producer really needs to know.

Having decided to adopt an AQL approach, the next point the committee had to examine was whether to adopt the MIL-STD-105 scheme as it stood, or whether to design a new scheme following similar principles. (There would have been little point in returning to the ASF or SRG tables, which had been superseded by MIL-STD-105, except for some very good reason.)

One point in favour of adopting something similar to MIL-STD-105 was provided by the Joint Service Specification K1001 on Electronic Valves, issued by the Ministry of Supply (1958). Appendix XI of this specification deals with acceptance sampling, and for inspection by attributes quotes most of the text and tables from MIL-STD-105A, but with a few minor amendments. This specification had been in use in this country

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for a number of years, and there was experience available of its successful operation.

Within the framework of an AQL scheme it would be possible to include something on the lines of deferred sentencing (Hill *et al.*, 1959), or, more generally, some other form of serial sampling (Cox, 1960). It was decided that while deferred sentencing could be very useful on suitable occasions, the mechanism of deferring sentence would be too much of a nuisance if widely applied, and should not be recommended for a general specification. Considering serial schemes that only look backwards, at batches of the recent past, and not forwards, at batches of the near future, and thus avoid having to defer sentence, it was noted that MIL-STD-105 is already a scheme of this type in so far as the normal, tightened and reduced inspection introduces a serial element. This is not necessarily the most efficient type of serial element that could be introduced, but experience did exist of its operation and it was thought safer to continue with a scheme of a known type, hoping to introduce experiments with other forms of serialism at a later date, and eventually modify the document if something more successful was devised.

It was decided that considerable advantages would arise from international standardization in this matter between the United Kingdom and the United States, that it would therefore be a good idea to follow the lead of MIL-STD-105 except where there was a clear advantage in making a change, and that where a change was made an attempt should be made to keep the document compatible with MIL-STD-105.

8. Differences between DEF-131 and MIL-STD-105

Some of the differences between DEF-131 and MIL-STD-105 are merely minor changes in the wording or the spelling (e.g. "gauging" instead of "gaging"), or are necessary changes to the text because of changes in the tables. These minor changes will not be listed here, but the more important changes will now be discussed in detail. The summary tables of DEF-131, and a typical page from the extended tables, are shown for reference, in an appendix, at the end of this paper.

8.1. *The Authority*

MIL-STD-105A laid down that many of the functions should be performed by "the Government". This wording is not consistent with British military contracts. The standard contract conditions state that "the Authority means the person who in the contract is stated to be the Authority" and the contract itself then starts by naming the Authority as, for example, The Secretary of State for War, or The Minister of Aviation. This wording has therefore been adopted for DEF-131.

MIL-STD-105B has largely succeeded in removing the Government from the text, so as to make the document more suitable than formerly for use by private industry, but this has been at the cost of a certain amount of vagueness as to who is to perform some of the functions.

The DEF-131 wording has the advantage of being explicit, while still leaving the way open for anyone to use the document simply by stating in the relevant contract that he is the Authority, for the purposes of that contract.

During the preparation of DEF-131 there was much discussion of the possibility of being more explicit, and stating which functions were to be performed by the Design Authority, Inspection Authority, etc., but this would have led to contractual difficulties, and also there was some disagreement as to which functions should be

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performed by which branch. It was therefore decided to leave the Authority undivided and let the problem of delegating the functions be settled individually from one case to another.

8.2. *Lots and Batches*

A collection of articles submitted by a supplier for inspection is generally called a "lot" in the United States; in this country the term "batch" is more usual, and has been used in DEF-131. This led to considerable discussion on the committee since "lot" is frequently used in the electronics industry, whereas in ammunition manufacture "lot" is used for a different purpose and would have caused confusion. Eventually, "batch" won the day.

8.3. *Acceptable Quality Level*

It was decided to adopt the AQL as the quality required in the product, and not as describing any fixed point on the O.C. curve, but a more meaningful definition than that given in MIL-STD-105 was considered desirable. The American Society for Quality Control, in its ASQC Standard A2-1957, defined AQL as "The maximum percent defective (or the maximum number of defects per 100 units) which can be considered satisfactory as a process average", that is to say, an exact return to the ASF definition of 1944, except for the additional words in brackets to allow for defects inspection as well as defectives inspection.

This definition is used in DEF-131, with the addition of the word "true" in front of "process average" to indicate that the population parameter and not the sample statistic is being referred to.

AQL values greater than 10·0 (percent defective or defects per hundred units) have been omitted from the tables. It was thought that these would be used too rarely to be worth inclusion. On the other hand, an additional value of 0·010 (percent defective or defects per hundred units) has been included as this had been particularly requested.

Table I in MIL-STD-105 is used for converting AQLs which do not fall exactly on a value given in the tables to a value which does. This table originated for the purpose of using the standard with specifications which had been written for use with the ASF or SRG tables. It has now largely served its purpose in that nowadays AQLs usually are specified as one of the values given in the tables, but if, for a particular purpose, someone wishes to specify an AQL not in the tables, it seems wrong to defeat his wishes by using some other value instead. As given in MIL-STD-105, the table appears to be mandatory, and might lead to cases where an inspector carried out a policy quite contrary to the requirements. For example (deliberately taking an extreme case for illustrative purposes), suppose an AQL is given as 2·80 per cent. and the estimated process average is 3·50 per cent., judged from 22,500 sample units. The estimated process average is significantly greater than the AQL, and tightened inspection should be required. Yet Table I in MIL-STD-105 would tell the inspector to act as if the AQL had been 4 per cent. and he would find that he could change to reduced inspection if he wished. Such a situation is clearly unsatisfactory.

DEF-131 therefore omits this table and instead calls the fifteen given values of AQL "Preferred AQLs", stating that if "an AQL be specified other than a preferred AQL, the tables . . . are not applicable". In general, I believe that this will lead people to choose one of the preferred values, but where they decide that they must have some other value, a special plan will have to be devised.

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8.4. *Code Letters and Inspection Levels*

The code letters, by which the tables are indexed in addition to the indexing by AQL, really do no more than indicate the sample size, and if only normal inspection and single sampling were to be used it would be possible to index by sample size instead of by code letter. However, when tightened and reduced inspection, and double, multiple and sequential sampling are introduced, code letters (or some equivalent device) become necessary.

The code letters in DEF-131 have precisely the same relation to the sample sizes as in MIL-STD-105, except that three additional letters (R, S and T) are introduced, which give larger sample sizes than are available in MIL-STD-105, but come into use only with the tightest AQLs.

Inspection levels I, II and III are identical in the two documents, except that, in DEF-131, letter R is introduced in level III for the largest batch sizes. Since this letter had been included in the tables, it seemed logical to use it here. A good deal of thought went into the decision to copy these inspection levels. The sort of pattern they follow seems intuitively to be a fairly sensible one, there are advantages in taking larger samples when batch sizes are large enough to allow it, experience of using them was apparently favourable, and it was not wished to change anything merely for the sake of change without a good reason. And yet they are quite arbitrary. The question really came down to this: is it possible to find an alternative scheme, which will serve the same purposes, and which can be laid down in advance of the start of production, which is not arbitrary? Having failed to find such an alternative, the decision to adopt the inspection levels of MIL-STD-105 followed.

An addition in DEF-131 is inspection level IV which gives sample sizes two to three times as large as level III. It is thought that this will be useful for the tightest AQLs.

DEF-131 also gives inspection levels IA, IB and IC which follow level I for small batch sizes but are cut short at relatively small sample sizes for the larger batches. These "may be used where relatively small sample sizes are necessary and large sampling risks can be tolerated". They have been adopted as an alternative to the "Special procedures for small-sample inspection" of MIL-STD-105, which were thought to be too cumbersome. Levels IA, IB and IC have been used in the electronics industry in this country for some years, appearing in K.1001, and have been found useful.

DEF-131 itself does not give any advice on how to choose an inspection level. The forthcoming Defence Guide DG-7 suggests that "having set the AQL as the quality required as an average, the inspection level should be set by considering what quality should have a high chance of rejection if an occasional batch of that quality should happen to be offered". An examination of the O.C. curves for the given AQL should then enable a level to be chosen that will give the desired protection for the batch size expected. This is very similar to an LTPD approach, except that it is being considered as a second choice to be made, after the selection of the AQL, and not as the primary choice.

8.5. *Process Average*

In the various American tables, it is not always clear whether the words "process average" refer to the population parameter or the sample statistic, except in MIL-STD-105B where the wording is "estimated process average". In DEF-131 the

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two concepts are distinguished by the names "true process average" and "estimated process average".

Both the ASF and the SRG tables used an estimate of process average as a criterion for switching between normal, tightened and reduced inspection but neither gives any clear instructions on how often the calculation is to be made. The SRG tables say that the frequency "can only be determined by experience", which implies that no routine and regular computation is expected. Each of these tables favours a

TABLE 2

Probability that the results of five successive samples call for tightening of inspection when submitted quality equals the AQL

Code letter	*Size of combined samples*	AQL													
		·010	·015	·035	·065	·10	·15	·25	·40	·65	1·0	1·5	2·5	4·0	6·5	10·0
A	10															·013
B	15														·014	
C	25													·017		
D	35												·011			·006
E	50											·007			·005	·003
F	75										·007			·010	·003	·003
G	125									·010			·004	·005	·003	·003
H	175								·006			·006	·005	·006	·003	·002
I	250							·026			·004	·005	·005	·003	·005	·003
J	375						·020			·004	·005	·005	·004	·003	·002	·003
K	550					·019			·007	·004	·004	·005	·004	·004	·004	·003
L	750				·014			·013	·012	·004	·005	·006	·003	·004	·004	·003
M	1,125			·008			·008	·008	·007	·004	·003	·005	·003	·003	·003	·002
N	1,500		·022			·004	·008	·005	·004	·006	·003	·003	·003	·003	·002	·002
O	2,250	·022			·017	·008	·025	·005	·005	·005	·003	·003	·004	·003	·002	·002
P	3,750			·011	·013	·005	·005	·004	·006	·004	·004	·004	·003	·002	·003	·002
Q	7,500		·006	·006	·005	·005	·003	·003	·004	·002	·002	·002	·002	·002	·002	·002
R	11,250	·006	·008	·007												
S	16,500	·007	·004													
T	25,000	·014														

computation embracing at least 20 batches if possible, but again neither requires any fixed routine. MIL-STD-105A requires the process average to be estimated from the results of 10 batches (unless otherwise specified) and it is usual to do this regularly on a moving average basis, although it is not specifically stated that this is what is required. MIL-STD-105B changes the basis to 5 batches and also requires the results to be weighted by the batch sizes, where these differ from one batch to another. In each case the process average limits are based upon a multiple of the standard deviation of the Poisson distribution.

In considering this question for DEF-131, it was at first questioned whether a multiple of the standard deviation was the right basis, since multiples of the standard deviation usually imply that an assumption of normality is being made and that a

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constant probability is being aimed at. In the major portion of these tables, however, the normal approximation is poor, and if constant probabilities are required it would be better to derive them straight from Poisson tables. Secondly, however, it was questioned whether constant probabilities were really the right thing to aim at, the argument being very similar to the argument against a fixed probability of acceptance at the AQL, namely that where sample sizes are small (in relation to the AQL specified) the total risks must be greater and it is not reasonable to let the consumer suffer all the extra risk; the producer should bear a share of it.

TABLE 3

Probability that the results of five successive samples call for tightening of inspection when submitted quality equals twice the AQL

Code letter	*Size of combined samples*	AQL													
		·010	·015	·035	·065	·10	·15	·25	·40	·65	1·0	1·5	2·5	4·0	6·5	10·0
A	10															·121
B	15														·120	
C	25													·135		
D	35												·096			·255
E	50											·063			·195	·289
F	75										·064			·250	·265	·431
G	125									·082			·175	·298	·460	·706
H	175								·054			·161	·265	·430	·590	·773
I	250							·132			·133	·224	·427	·530	·807	·935
J	375						·105			·121	·224	·339	·507	·733	·910	·990
K	550					·100			·156	·185	·311	·483	·708	·903	·990	·999
L	750				·076			·177	·256	·275	·432	·651	·836	·973	·999	1·000
M	1,125			·046			·126	·206	·294	·394	·569	·815	·960	·998	1·000	1·000
N	1,500		·076			·084	·169	·224	·318	·575	·733	·901	·990	1·000	1·000	1·000
O	2,250	·076			·172	·169	·442	·339	·531	·750	·901	·984	1·000	1·000	1·000	1·000
P	3,750			·126	·220	·224	·339	·507	·792	·933	·993	1·000	1·000	1·000	1·000	1·000
Q	7,500		·078	·161	·275	·432	·569	·836	·973	·999	1·000	1·000	1·000	1·000	1·000	1·000
R	11,250	·078	·126	·268												
S	16,500	·117	·128													
T	25,000	·238														

When calculations were made on the basis of the tables in MIL-STD-105 it was found that these two arguments cancelled each other. The effect of using a constant multiple of the standard deviation, even though the normal approximation is poor, leads to a variability in the probabilities which provides the required more equitable sharing of the risks between producer and consumer. This is shown in Tables 2 and 3, which are calculated on the five-batch basis. Table 2 shows the probability that the estimated process average exceeds the upper limit when calculated from the results of five given batches using single sampling when the submitted quality equals the AQL; Table 3 shows the corresponding figures when the submitted quality equals

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twice the AQL. Both these tables exhibit a certain amount of "lumpiness"—this is partly because a binomial variable is discrete, not continuous, and partly because the table of process average limits proceeds in steps rather than giving all the possible values, to make it easier to use—but the general pattern does show that the producer is required to bear more risk of tightening being installed by chance when the discrimination is not very good, i.e. along the top diagonals of the table, whereas the risk falls to a low level in the bottom right-hand corner of the table where the discrimination is good enough to allow it to do so.

Some of the chances of installing tightened inspection towards the top of Table 3 do not look as large as one would wish them to be, but it must be remembered that these limits are not applied just once, but continuously on a moving average basis. In this sense Tables 2 and 3 are not wholly adequate, based as they are upon static rather than dynamic considerations, but they do show the main point they are intended to show. As a result of a study of tables of this sort, it was decided to adopt the "3 sigma" limits of MIL-STD-105 (with suitable extensions and deletions to allow for the extensions and deletions in the range of AQLs and code letters), but it remained to be decided over how many batches to calculate the moving averages. For this purpose a dynamic approach is essential, since a static approach would merely show that for distinguishing between one fixed quality and another, the larger the sample size the better, which would appear to imply that the number of batches considered should be as large as possible. But in practice we do not want to distinguish between fixed qualities, since we can be sure that the quality submitted is very unlikely to remain fixed; what we require is that the estimated process average shall respond quickly to a change to a quality worse than the AQL, and if we take too long a series of batches for estimating process average the old results will outweigh the new ones and the response to a change will be diminished.

To get the information for a decision on this point, some "Monte Carlo" trials were carried out in which random numbers were used to simulate the results of inspection, the true process average being changed every 25 batches or so from the AQL to twice the AQL and back again, the switch between normal and tightened inspection being noted when the estimated process average was based upon 2, 3, 4, 5, 7 and 10 batches respectively.

The technique of antithetic variates was used (Hammersley and Mauldon, 1956), i.e. each set of random numbers was used twice in such a way as to get two valid answers which would be negatively correlated. Then if one answer is overestimating, the other is likely to be underestimating. Thus, for example, for the binomial distribution $n = 150, p = 0{\cdot}01$ the usual variate would be:

Random number	*Number of defectives*
0000–2214	0
2215–5569	1
5570–8094	2
8095–9352	3
9353–9819	4
9820–9957	5
9958–9991	6
9992–9998	7
9999	8

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the variate antithetic to this would be:

Random number	*Number of defectives*
7785–9999	0
4430–7784	1
1905–4429	2
0647–1904	3
0180–0646	4
0042–0179	5
0008–0041	6
0001–0007	7
0000	8

A study of the results led to the conclusion that a 5-batch basis was the best of those considered and this was accordingly adopted.

The idea of weighting the results by batch size was rejected as adding too much complication without giving any great advantage in return. Since this decision was taken, Maisel (1960) has shown that weighting by batch size gives, if anything, a less precise estimate, in addition to being more complicated.

MIL-STD-105B requires that "when double or multiple sampling" is used, "only data from the first sample from each inspection lot will be used in computing the estimated process average". The SRG tables also require this to be done; MIL-STD-105A does not mention the point, but it has normally been the method used in practice to consider only first sample results.

The reason for this rule is to ensure an unbiased estimate. It is arguable, in general, whether lack of bias is a property that should always be aimed at, and in preparing DEF-131 it was argued that a biased estimate using all the information might be preferable. For sampling inspection purposes, an estimate of process average as such is not required. What is required is a rule for taking action to change the sampling plan when necessary. A further Monte Carlo study established that the biased estimate, using all the information, appeared to give a better action rule than the unbiased estimate, and it was therefore adopted.

While a 5-batch moving average gives reasonable results for switching between normal and tightened inspection, it is by no means claimed that it is the best possible mechanism. There certainly exists scope for either experimental or theoretical studies of improved methods. In particular, it should be possible to adopt the recent control chart methods based upon geometric moving averages (Roberts, 1959) or upon cumulative sums (Page, 1954; Barnard, 1959; Ewan and Kemp, 1960; Page, 1961) to this purpose with beneficial effects.

8.6. *Sample Sizes and Acceptance Numbers for Normal Inspection, Single Sampling*

The sample sizes given in MIL-STD-105 follow almost the same sequence as did those of the ASF and SRG tables, but with some extension at the lower end. The sequence is a fairly sensible one following an approximate geometric progression with a common ratio of the sixth root of 10, rounded off to "round numbers" but with an odd jump at the top end from 750 to 1,500, whereas the progression would call for an additional value between these two. There would be advantages in changing this sequence to one with a common ratio of the fifth root of 10 instead of the sixth root, since this ratio is used for the sequence of AQLs. However, it was decided in

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this instance that the advantage of standardization of the documents was more important and the sample size sequence of MIL-STD-105 was adopted for DEF-131, but with three additional larger sample sizes to allow extra flexibility with the tightest AQLs.

Since the definition of AQL does not specify a particular probability of acceptance, the fixing of the sequences of AQLs and sample sizes does not fix the values of the acceptance numbers. It was decided that what was required was a good chance of acceptance at the AQL for normal inspection, and that, for a given AQL value, the chance of acceptance should improve if the sample size were increased. These properties are, in general, possessed by the table in MIL-STD-105, although certain discrepancies are unavoidable since the acceptance numbers must be integers. It was therefore decided to adopt the American table—some improvements might be possible but would be only of marginal importance, so the advantages of standardization outweigh them. There would be severe disadvantages in having two tables available at the same time giving different acceptance numbers for the same AQL and sample size. Even though we know that an AQL is a requirement in the product, and not a description of a sampling plan, and that any sampling plan can be used for any AQL if that is what you want for the particular circumstances, discrepancies between one scheme and another should be avoided unless good reasons can be put forward.

8.7. *Tightened Inspection*

A comparison of some of the normal inspection plans with the corresponding tightened plans in MIL-STD-105 shows that tightened inspection does not always appear to be as tight as might be thought necessary. However, the fact was considered that the tightening had been more severe in the SRG tables, and the change appeared to be deliberate. Presumably as a result of experience it had been found that less

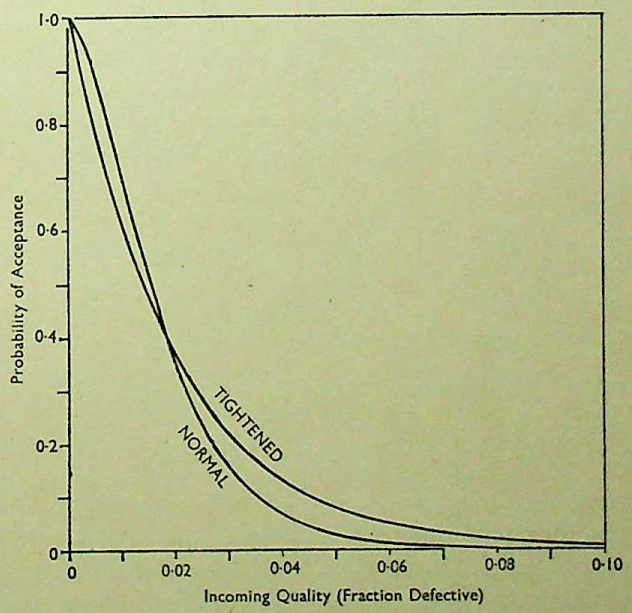

FIG. 3. Operating characteristic curves for normal and tightened inspection for AQL 0·40 and code letter J in MIL-STD-105.

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severity was preferable. It was therefore thought wise to follow the American tables on this point, except for the correction of two anomalies.

The first anomaly is that in MIL-STD-105 an upper limit for the estimated process average is given for an AQL of 0·015 and the text states that "Tightened inspection shall be instituted when the estimated process average . . . is greater than the upper limit", yet no tightened plans are given for this AQL. In DEF-131 the additional AQL of 0·010 provides tightened plans for 0·015 and tightened plans for 0·010 are given also.

Secondly, there are certain circumstances where, if the MIL-STD-105 scheme is adopted, the tightened plan has a considerably smaller sample size than the normal plan. This is where the arrows in the table come into play and point downwards for normal inspection but upwards for tightened inspection. For example, for an AQL of 0·40 and code letter J, the normal plan has a sample size of 110 while the tightened plan has only 50. The acceptance number is also reduced, but, as will be seen from the O.C. curves in Fig. 3, this is not sufficient to compensate for the reduction of sample size; over a considerable range of percentages defective the so-called tightened plan is actually more lenient than the normal plan.

It may be that the authors of MIL-STD-105 expected the user of the tables to find the normal plan first, following arrows where necessary, and then move one place to the left for the tightened plan, but such a procedure is at variance with the text. In DEF-131 the double arrow symbol (up for normal, down for tightened) is introduced to overcome the difficulty.

8.8. *Double Sampling*

The MIL-STD-105 double sampling table incorporates the three arbitrary rules inherited from the ASF and SRG tables, and already mentioned in Section 5 above. Hamaker and van Strik (1953, 1955) have shown that these plans are inefficient in that virtually the same O.C. curves can be obtained with smaller sample sizes. They recommend a system in which the first sample is $\frac{2}{5}$, and the second sample is $\frac{4}{5}$, of the equivalent single sample size, instead of $\frac{2}{3}$ and $\frac{4}{3}$ as in MIL-STD-105.

If we denote the acceptance and rejection numbers for single sampling by a and r respectively, and for double sampling by a_1 and r_1 for the first sample, and by a_2 and r_2 for the combined samples, they recommend in their 1953 paper taking

$$a_2 = 1{\cdot}15a,$$

$$r_1 = r_2 \quad (= a_2 + 1),$$

and

$$a_1 = \left\{\sqrt{\left(\frac{a_2+2}{3}\right)} - 0{\cdot}30\right\}^2 - 1.$$

This last relationship shows an almost linear connection between a_1 and a_2, and might as well be expressed as

$$a_1 = 0{\cdot}306a_2 - 1{\cdot}03,$$

which is a much simpler expression to use.

In the 1955 paper they recommend abandoning the $r_1 = r_2$ rule in addition. This rule was not too unreasonable in the Dodge and Romig tables where it was always assumed that rejection meant 100 per cent. inspection by the same inspectors, although even in those circumstances to be able to go straight ahead with 100 per cent. inspection would be preferable to the trouble of drawing a second random sample when the

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verdict is virtually certain to be a rejection. In the case of a government inspectorate examining submitted batches and returning them to the supplier if rejected, it just does not make sense to keep the $r_1 = r_2$ rule. To take the extreme case from MIL-STD-105 (AQL = 10·0, code letter Q), a first sample of 1,000 is required and if it shows 347 defectives a further random sample of 2,000 must be drawn to see if one more defective is found!

For picking the first sample rejection numbers for the DEF-131 plans, various possible rules were used experimentally, and finally the one adopted was that if the quality offered was such that the expectation in the two samples combined was equal to the second acceptance number there should be not more than one chance in ten of rejecting on the first sample. Calculations were made to find in each case the lowest rejection number to satisfy this condition.

The DEF-131 double plans are derived from the Hamaker and van Strik formulae together with this additional rule. Figs. 4 and 5 show in a typical case that the

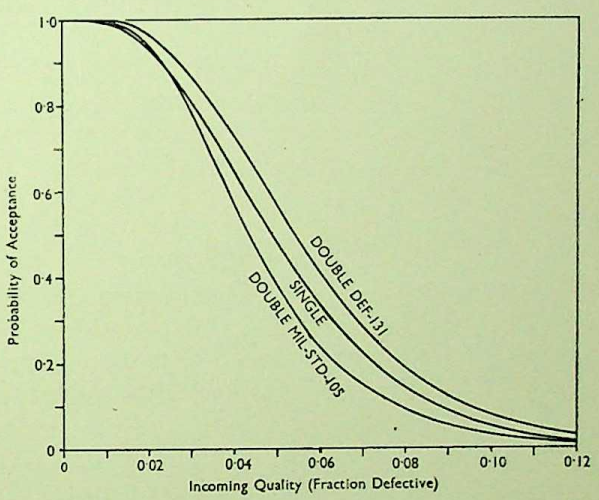

FIG. 4. Operating characteristic curves for AQL 1·5 and code letter J for single and double sampling.

match of the O.C. curve to the single sampling O.C. curve is just as good as is that of the MIL-STD-105 plan, whereas the average sample size curve is a considerable improvement.

The Hamaker and van Strik formulae do not, however, produce a satisfactory equivalent plan to a single sampling plan with an acceptance number of 1. The tightest double plan possible with sample sizes of $\frac{2}{5}$ and $\frac{4}{5}$ of the single sample size shows an O.C. curve which is much too lenient in the higher percentages defective. There are three possible courses available, namely (i) to accept this discrepancy in the O.C. curves; or (ii) to give only single sampling for this case; or (iii) to use double plans with different sample sizes. The third of these possibilities has been adopted for DEF-131. If a first sample is taken equal to $\frac{2}{3}$ of the single sample size (with $a_1 = 0, r_1 = 2$) and a second sample equal to $\frac{2}{3}$ of the first sample (with $a_2 = 1, r_2 = 2$), the O.C. curve is matched very well and the average sample size curve is satisfactory. If this principle were adopted precisely, it would lead to an anomalous situation in places, since it would be found that this plan would require a larger first sample than

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the Hamaker and van Strik plan for the next code letter. The plans actually used have been modified, so as to avoid this situation, by reducing the first sample size where necessary to be no bigger than the first sample for the next code letter and increasing the second sample to keep the total sample size the same. This has caused a slight loss in the accuracy of matching, but it is considered that the gain in consistency makes this worth while.

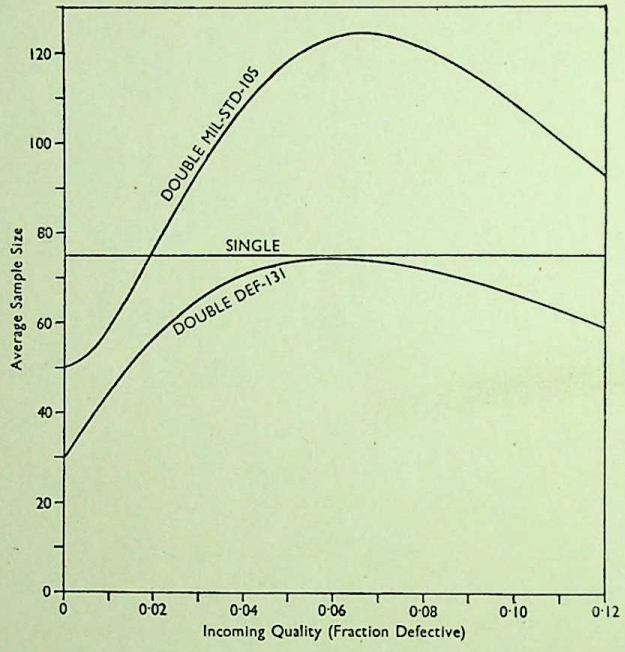

FIG. 5. Average sample size curves for AQL 1·5 and code letter J for single and double sampling.

8.9. *Multiple and Sequential Sampling*

It seems probable that multiple sampling plans would not generally be used, because of their additional complication compared with single and double sampling, except when it is necessary to keep the average sample size as small as possible (for a given O.C. curve). When this is necessary, greater advantages in average sample size can be obtained by using sequential sampling, so it was decided to give sequential parameters in DEF-131, together with rules for constructing a multiple plan from a sequential one if desired.

Wald's (1947) procedure for deriving a sequential plan gave three parameters. This three-parameter set can be reduced to a two-parameter set by choosing the two risks α and β to be equal, or, using Barnard's (1946) notation, by choosing $H = H'$, that is to say the Target to be twice the Handicap. Hamaker (1953) has argued that plans which depart to any large degree from this two-parameter set are highly inefficient, whereas those which depart in only a small degree "are in no way of marked advantage" compared with using a two-parameter plan. It should be noted that Hamaker was using his own definition of efficiency which is not really fair to Wald's theory, but if we are concerned with the entire length of the O.C. curve rather than exclusively with two points upon it, then Hamaker's conclusion does seem to be justified, and furthermore the O.C. curves of the two-parameter set are of very much the same shape as single-sampling O.C. curves, so they are really what we need if we desire plans to match single-sampling plans.

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For the two-parameter set Wald's equations give

$$h_1 = h_2 = \frac{\log\left(\frac{1-\alpha}{\alpha}\right)}{\log\frac{p_2(1-p_1)}{p_1(1-p_2)}},$$

$$s = \frac{\log\left(\frac{1-p_1}{1-p_2}\right)}{\log\frac{p_2(1-p_1)}{p_1(1-p_2)}},$$

where the probability of acceptance is to be $1-\alpha$ for proportion defective p_1 and α for proportion defective p_2, and when $p = s$, the probability of acceptance is 0·5.

Converting to Barnard's notation, we have

$$h_1 = h_2 = \frac{H}{b+1} \quad \text{and} \quad s = \frac{1}{b+1}$$

and hence

$$\frac{H}{b} = \frac{\log\left(\frac{1-\alpha}{\alpha}\right)}{\log\frac{p_2}{p_1}},$$

and when $p = 1/(b+1)$, the probability of acceptance is 0·5. These last two relationships can be used to fit a sequential plan to a single-sampling plan, a choice being necessary for α. For the DEF-131 tables $\alpha = 0{\cdot}1$ was chosen.

If the single-sampling plan that it is desired to match has sample size n and acceptance number c, we have $P(\text{acc}) = \frac{1}{2}$ when $np = \frac{1}{2}\chi^2_{2(c+1),0{\cdot}5}$ (using the Poisson approximation). Hence, we require

$$\frac{1}{b+1} = \frac{\frac{1}{2}\chi^2_{2(c+1),0{\cdot}5}}{n},$$

or

$$b = \frac{2n}{\chi^2_{2(c+1),0{\cdot}5}} - 1$$

$$= \frac{n}{c+0{\cdot}67} - 1, \quad \text{approximately.}$$

If p_1 and p_2 give $P(\text{acc}) = 0{\cdot}9$ and $P(\text{acc}) = 0{\cdot}1$ respectively, we have (again using the Poisson approximation)

$$np_1 = \tfrac{1}{2}\chi^2_{2(c+1),0{\cdot}9} \quad \text{and} \quad np_2 = \tfrac{1}{2}\chi^2_{2(c+1),0{\cdot}1}.$$

Letting

$$k = \frac{\log\left(\frac{1-\alpha}{\alpha}\right)}{\log\frac{p_2}{p_1}}$$

$$= \frac{\log 9}{\log\frac{\chi^2_{2(c+1),0{\cdot}9}}{\chi^2_{2(c+1),0{\cdot}1}}},$$

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we have $H = kb$, and k is dependent only upon c. Values of k are shown in Table 4.

There are two approximations involved in this method, namely the use of the Poisson distribution instead of the binomial, and the approximation used by Wald in deriving his equations. A further approximation arises by rounding off H and b to integer values. Where the derived H and b values were small, the exact O.C. curves were computed using the binomial formula, and Burman's (1946) exact formula for the O.C. curve of a sequential plan, and the H and b values were adjusted where it appeared desirable.

For $c = 0$ no true sequential plan is possible, but by taking $H = n, b = 2n$ the single-sampling plan can be expressed in the sequential parameters. This is worth

TABLE 4

$$\textit{Values of } k = \frac{\log 9}{\log \frac{\chi^2_{2(c+1),0\cdot9}}{\chi^2_{2(c+1),0\cdot1}}}$$

c	k	c	k	c	k	c	k
1	1·104	6	2·209	11	2·924	16	3·496
2	1·395	7	2·369	12	3·047	17	3·600
3	1·637	8	2·519	13	3·165	18	3·700
4	1·847	9	2·661	14	3·279	19	3·798
5	2·036	10	2·796	15	3·390	20	3·894

while since it may be wished to use sequential sampling with some sort of machine to keep the score, and to have values of the necessary parameters to feed into the machine in all cases would then be helpful.

The sequential plans are truncated at 1·5 times the equivalent single-sample size, with the rule that the batch is then accepted if the score is equal to or greater than the handicap, rejected otherwise. Since the average sample size of a sequential plan is less than the size of the equivalent single plan, a size of 1·5 times the equivalent single plan must be reached comparatively rarely and, even when it is reached, the acceptance rule will often lead to the same decision as would eventually have been taken by continuing. Intuitively, then, it would seem likely that this truncation rule would have little effect on the O.C. curve, and examination of the O.C. curves in a few typical cases indicates that this is indeed so.

The method given for deriving a multiple plan from a sequential ensures that all acceptance points of the sequential become acceptance points in the multiple, but there is only one rejection point for each $b+1$ items examined instead of b rejection points. It follows that the O.C. curve for the multiple plan cannot, at any point, be lower than for the sequential plan. It may be identical, or it may be higher, but where it is higher it will be only by a negligible amount since the chance of passing through a rejection point in the sequential but finally reaching an acceptance point is small.

8.10. *Reduced Inspection*

Reduced inspection is used only when the quality history of the product is good and there is reason to expect the quality to continue good. In these circumstances

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considerable savings in inspection costs can be made, and the sample sizes are reduced to one-fifth of the normal inspection single sample size (or to 2 where the normal sample size is less than 10). The protection to the consumer now lies more than ever in the good production, and not in the sorting ability of the sampling plan. In fact the main purpose of the reduced sampling plan is not to sort the batches, but to watch the process average, and call for the restoration of normal inspection if required. Reduced plans are given for single sampling only since these are the most efficient for watching the process average.

DEF-131 follows MIL-STD-105 in not presenting any O.C. curves for reduced inspection. This is because the portion of the curve for quality better than the AQL is normally practically indistinguishable from the normal inspection curve, whereas the portion for quality worse than the AQL is irrelevant if the quality is not in fact in this region. If it is in this region, then reduced inspection should not be in operation. It is true that the information might sometimes be wanted of the chance of rejecting a single bad batch included among a sequence of very good ones, when on reduced inspection, but it was decided that to include the curves for this purpose would probably mislead more people than it would help.

If the sample size is to be considerably reduced when the quality history is good, it is most important that the producer's risk should not be increased in the process, since this would mean penalizing the producer for good work. The MIL-STD-105 plans fail to meet this requirement in six cells of the table, and the acceptance numbers have been adjusted in these cells. Otherwise the DEF-131 table is similar to the MIL-STD-105 table.

8.11. *Layout of "Extended Tables"*

The master tables in MIL-STD-105, renamed summary tables in DEF-131, together with the text in each case, really give all the information in the documents other than the O.C. curves. In each case, however, there are given extended tables in addition. These are designed to display the O.C. curves, to show which curve corresponds to which sampling plan, and to allow easy comparison of the equivalent single, double and multiple or sequential plans.

In MIL-STD-105 a pair of pages is allotted to each code letter. This arrangement leads to possible confusion in seeing which O.C. curve corresponds to which plan, and also to possible mistakes in following the triangular symbols that direct the user from one page to another.

For DEF-131 it was decided that paper was cheaper than mistakes, and by giving one page to each combination of AQL and code letter, it becomes possible to set out everything needed for any given circumstances on a single page. It is true that this leads to a fatter document and to a certain amount of repetition (e.g. nearly every normal inspection plan is given again on another page as a tightened plan), but the gain in simplicity of use is considerable.

9. The ABC "Acceptance Sampling" Working Group

Under the American-British-Canadian (ABC) Standardization Programme copies of the draft of DEF-131 were sent to the U.S.A. and to Canada for comments. Among the American comments it was said that "as a major revision of the standard, the proposed specification does not cover all the areas where MIL-STD-105 could and should be improved. Therefore, the proposed specification falls short of the desired goal" and it was suggested that "the establishment of a working party to examine the

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proposal, together with MIL-STD-105, is considered likely tc produce a mutually satisfactory agreement".

In reply it was agreed that "this document does not cover all the areas where MIL-STD-105 could and should be improved. This is, at least partially, because we have adopted changes only where we consider the advantage to be substantial, so that if we were to use the Defence Specification and the Americans MIL-STD-105 the two would be compatible, although not identical. If there is to be the chance of a working party to consider revision, there are further changes we should wish to propose."

This paper is concerned with the development of DEF-131, and not with the subsequent ABC work, but the story of DEF-131 would be incomplete without just a note to say that it was decided, when the ABC Working Group was established, to proceed with the publication of DEF-131 as an interim document, but that it now appears likely that it is to have a relatively short life before being overtaken by new proposals.

ACKNOWLEDGEMENTS

I alone am responsible for this paper; but the production of Defence Specification DEF-131 and Defence Guide DG-7 has been the work of the "Acceptance Sampling by Attributes" sub-committee of the Engineering Standards Co-ordinating Committee. The other members of this sub-committee must bear equal responsibility for these documents (although "some are more equal than others") .

On this sub-committee, at one time and another, have served Mr R. H. Billingsley, Capt. S. Brown, Cdr K. F. Busbridge, Mr H. W. G. Deeks, Mr H. J. Elton, Col. G. W. H. Fellows, Mr P. F. Foss, Mr N. Griffin, Sqn. Ldr. A. T. Griffiths, Mr G. N. Harding, Mr H. W. Harverson, Mr K. Hopkinson, Miss P. M. E. Johnson, Maj. L. Jones, Mr T. J. Knott, Mr J. E. McLean, Mr F. Munroe, Col. R. V. Peters, Col. B. G. Ralfs, Mr W. R. Rushmer, Mr F. C. Stearn, Mr B. J. G. Stewart, Mr H. J. Stone, Mr R. J. Temple, and Mr T. Veitch. I should like to take this opportunity of thanking them all for the contributions they have made to the project.

I thank the Controller of Her Majesty's Stationery Office for permission to reproduce the tables in the appendix.

I should also like to acknowledge the services of Miss D. M. Parry who drew the O.C. curves in DEF-131, and of Mr G. Clark who drew the figures for this paper.

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APPENDIX

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Tables from Defence Specification DEF-131, reproduced by permission of the Controller of Her Majesty's Stationery Office

TABLE I

Code Letters

(Inspection Levels for General Use)

Batch Size	Inspection Levels			
	I	II	III	IV
2 to 8	A	A	C	D
9 to 15	A	B	D	E
16 to 25	B	C	E	F
26 to 40	B	D	F	H
41 to 65	C	E	G	I
66 to 110	D	F	H	J
111 to 180	E	G	I	K
181 to 300	F	H	J	L
301 to 500	G	I	K	M
501 to 800	H	J	L	N
801 to 1300	I	K	L	N
1301 to 3200	J	L	M	O
3201 to 8000	L	M	N	P
8001 to 22000	M	N	O	Q
22001 to 110000	N	O	P	R
110001 to 550000	O	P	Q	S
550001 and over	P	Q	R	T

TABLE II

Code Letters

(Additional Inspection Levels)

Batch Size	Inspection Levels		
	IC	IB	IA
2 to 15	A	A	A
16 to 40	B	B	B
41 to 110	C	C	C
111 to 300	D	D	D
301 to 800	E	E	E
801 to 1300	E	E	F
1301 to 3200	E	F	G
3201 to 8000	E	G	H
8001 and over	E	G	I

Tables I and II from Defence Specification DEF-131.

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10

TABLE

Single Sampling Summary Table

Code Letter	Sample Size	Acceptable Quality Levels															
				0.010		0.015		0.035		0.065		0.10		0.15		0.25	
		Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re
A	2																
B	3																
C	5																
D	7																
E	10																
F	15																
G	25																
H	35																
I	50															0	1
J	75													0	1	N⇑⇓	T
K	110											0	1	N⇑⇓	T	⇓	
L	150									0	1	N⇑⇓	T	⇓		1	2
M	225							0	1	N⇑⇓	T	⇓		1	2	2	3
N	300					0	1	N⇑⇓	T	⇓		1	2	2	3	3	4
O	450	⇓		0	1	N⇑⇓	T	⇓		1	2	2	3	3	4	4	5
P	750	0	1	N⇑⇓	T	⇓		1	2	2	3	3	4	4	5	6	7
Q	1500	⇓		⇓		1	2	2	3	3	4	5	6	7	8	9	10
R	2250			1	2	2	3	3	4	⇑		⇑		⇑		⇑	
S	3300	1	2	2	3	3	4	⇑									
T.	5000	2	3	3	4	⇑											
		0.010		0.015		0.035		0.065		0.10		0.15		0.25		0.40	
		Acceptable Quality Levels															

AQLs are in percent defective, or in defects-per-hundred units
Ac = Acceptance Number. Re = Rejection Number.
⇑ = Use first sampling plan above arrow.
⇓ = Use first sampling plan below arrow.
N⇑⇓T = Use first sampling plan above for normal inspection, first below for tightened
When sample size equals or exceeds batch size, do 100% inspection.

Table III from Defence

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11

III

(Normal and Tightened Inspection)

(Normal Inspection)

0.40		0.65		1.0		1.5		2.5		4.0		6.5		10.0		Code Letter
Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re	Ac	Re	
										↓		↓				
										0	1	0	1	0	1	A
												N⇑⇓T		↑		B
										↓		↓		↓		C
				↓		↓		↓		N⇑⇓T				1	2	D
				0	1	0	1	0	1	↓		1	2	2	3	E
		↓		N⇑⇓T		N⇑⇓T		N⇑⇓T		1	2	2	3	3	4	F
		0	1	↓		↓		↓		2	3	3	4	5	6	G
0	1	N⇑⇓T		1	2	1	2	1	2	3	4	5	6	7	8	H
N⇑⇓T		↓		2	3	2	3	2	3	4	5	6	7	9	10	I
↓		1	2	3	4	3	4	3	4	5	6	7	9	10	13	14	J
1	2	2	3	4	5	4	5	4	5	6	7	9	10	13	14	18	19	K
2	3	3	4	5	6	5	6	6	7	8	9	12	13	18	19	24	25	L
3	4	4	5	6	7	8	9	8	9	11	12	17	18	24	25	34	35	M
4	5	5	6	7	8	10	11	11	12	14	15	20	21	32	33	44	45	N
5	6	7	8	10	11	14	15	14	15	20	21	29	30	43	44	62	63	O
8	9	11	12	15	16	20	21	31	32	45	46	68	69	98	99	P
13	14	18	19	25	26	35	36	56	57	81	82	124	125	184	185	Q
↑		↑		↑		↑		↑		↑		↑		↑		R
																S
																T
0.65		1.0		1.5		2.5		4.0		6.5		10.0				

(Tightened Inspection)

inspection.

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12

TABLE

Double Sampling Summary, Table

Code Letter	Sample	Sample Size	Cumulative Sample Size	Sample Size	Cumulative Sample Size	Acceptable Quality: 0.010	0.015	0.035	0.065
						Ac Re	Ac Re	Ac Re	Ac Re
A						↓	↓	↓	↓
B						↓	↓	↓	↓
C						↓	↓	↓	↓
D	First	4	4			↓	↓	↓	↓
	Second	4	8			↓	↓	↓	↓
E	First	6	6	4	4	↓	↓	↓	↓
	Second	6	12	8	12	↓	↓	↓	↓
F	First	9	9	6	6	↓	↓	↓	↓
	Second	9	18	12	18	↓	↓	↓	↓
G	First	14	14	10	10	↓	↓	↓	↓
	Second	14	28	20	30	↓	↓	↓	↓
H	First	20	20	15	15	↓	↓	↓	↓
	Second	20	40	30	45	↓	↓	↓	↓
I	First	30	30	20	20	↓	↓	↓	↓
	Second	25	55	40	60	↓	↓	↓	↓
J	First	45	45	30	30	↓	↓	↓	↓
	Second	40	85	60	90	↓	↓	↓	↓
K	First	60	60	45	45	↓	↓	↓	↓
	Second	60	120	90	135	↓	↓	↓	↓
L	First	90	90	60	60	↓	↓	↓	*
	Second	75	165	120	180	↓	↓	↓	*
M	First	120	120	90	90	↓	↓	*	N⇑⇓T
	Second	120	240	180	270	↓	↓	*	N⇑⇓T
N	First	180	180	120	120	↓	↓	N⇑⇓T	↓
	Second	150	330	240	360	↓	↓	N⇑⇓T	↓
O	First	300	300	180	180	↓	*	↓	(0 2 / 1 2)
	Second	200	500	360	540	↓	*	↓	(0 2 / 1 2)
P	First	500	500	300	300	*	N⇑⇓T	↓	(0 2 / 1 2)
	Second	330	830	600	900	*	N⇑⇓T	↓	0 3 / 2 3
Q	First	900	900	600	600	↓	↓	(0 2 / 1 2)	0 3 / 2 3
	Second	750	1650	1200	1800	↓	↓	(0 2 / 1 2)	0 4 / 4 5
R	First	1300	1300	900	900	↓	(0 2 / 1 2)	0 3 / 2 3	0 4 / 4 5
	Second	1200	2500	1800	2700	↓	(0 2 / 1 2)	0 3 / 2 3	0 4 / 4 5
S	First	2000	2000	1320	1320	(0 2 / 1 2)	0 3 / 2 3	0 4 / 4 5	↑
	Second	1650	3650	2640	3960	(0 2 / 1 2)	0 3 / 2 3	0 4 / 4 5	↑
T	First			2000	2000	0 3 / 2 3	0 4 / 4 5	↑	↑
	Second			4000	6000	0 3 / 2 3	0 4 / 4 5	↑	↑
						0.010	0.015	0.035	0.065	0.10

Acceptable Quality

AQLs are in percent defective or in defects-per-hundred units
Ac = Acceptance Number. Re = Rejection Number.
* = Use corresponding single sampling plan. (Table III).
↑ = Use first sampling plan above arrow.
↓ = Use first sampling plan below arrow.
N⇑⇓T = Use first sampling plan above for normal inspection, first below for tightened inspection.
When Acceptance and Rejection numbers are in brackets, use sample sizes in brackets.
When Acceptance and Rejection numbers are not in brackets, use sample sizes not in brackets.
When sample size equals or exceeds batch size do 100% inspection.

Table IV from Defence

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13

IV

(Normal and Tightened Inspection)

Levels (Normal Inspection)

0.10	0.15	0.25	0.40	0.65	1.0	1.5	2.5	4.0	6.5	10.0	Code Letter
Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	
↓	↓	↓	↓	↓	↓	↓	↓	↓	↓	*	A
									*	↑	B
								*	N⇑↓T	↓	C
							*	N⇑↓T	↓	0 2 / 1 2	D
						*	N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	E
					*	N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	F
				*	N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	1 5 / 6 7	G
			*	N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	1 5 / 6 7	1 6 / 9 10	H
		*	N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	0 4 / 5 6	1 5 / 7 8	2 7 / 10 11	I
	*	N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	0 4 / 5 6	1 5 / 7 8	2 7 / 10 11	3 9 / 15 16	J
*	N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	0 4 / 5 6	1 5 / 7 8	2 6 / 9 10	3 8 / 14 15	5 11 / 21 22	K
N⇑↓T	↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	0 4 / 5 6	1 5 / 6 7	2 6 / 9 10	3 8 / 12 13	5 11 / 19 20	7 14 / 28 29	L
↓	0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	0 4 / 5 6	1 5 / 6 7	2 6 / 9 10	3 8 / 12 13	5 11 / 19 20	7 14 / 28 29	11 19 / 39 40	M
0 2 / 1 2	0 3 / 2 3	0 4 / 4 5	0 4 / 5 6	1 5 / 6 7	1 6 / 8 9	2 7 / 11 12	4 9 / 16 17	6 12 / 23 24	10 18 / 37 38	14 24 / 51 52	N
0 3 / 2 3	0 4 / 4 5	0 4 / 5 6	1 5 / 6 7	1 6 / 8 9	2 7 / 11 12	4 9 / 16 17	6 12 / 23 24	9 16 / 33 34	14 23 / 49 50	21 31 / 71 72	O
0 4 / 4 5	0 4 / 5 6	1 5 / 7 8	2 6 / 9 10	3 8 / 12 13	4 10 / 17 18	6 12 / 23 24	9 17 / 36 37	14 24 / 52 53	23 34 / 78 79	34 47 / 113 114	P
1 5 / 6 7	1 6 / 8 9	2 7 / 10 11	3 9 / 15 16	5 11 / 21 22	7 15 / 29 30	11 19 / 40 41	18 29 / 65 66	27 39 / 93 94	43 58 / 144 145	65 83 / 215 216	Q
↑	↑	↑	↑	↑	↑	↑	↑	↑	↑	↑	R
											S
											T
0.15	0.25	0.40	0.65	1.0	1.5	2.5	4.0	6.5	10.0		

Levels (Tightened Inspection)

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14

TABLE

Sequential Sampling Summary Table

Code Letter	Maximum Sample Size	Acceptable Quality Levels						
			0.010	0.015	0.035	0.065	0.10	0.15
		H b	H b	H b	H b	H b	H b	H b
A		↓	↓	↓	↓	↓	↓	↓
B		↓	↓	↓	↓	↓	↓	↓
C		↓	↓	↓	↓	↓	↓	↓
D		↓	↓	↓	↓	↓	↓	↓
E	15	↓	↓	↓	↓	↓	↓	↓
F	25	↓	↓	↓	↓	↓	↓	↓
G	40	↓	↓	↓	↓	↓	↓	↓
H	55	↓	↓	↓	↓	↓	↓	↓
I	75	↓	↓	↓	↓	↓	↓	↓
J	115	↓	↓	↓	↓	↓	↓	75 150
K	165	↓	↓	↓	↓	↓	110 220	N⇅ T
L	225	↓	↓	↓	↓	150 300	N⇅ T	↓
M	340	↓	↓	↓	225 450	N⇅ T	↓	147 133
N	450	↓	↓	300 600	N⇅ T	↓	197 178	155 111
O	675	↓	450 900	N⇅ T	↓	295 267	233 167	200 122
P	1125	750 1500	N⇅ T	↓	492 446	389 279	332 203	296 160
Q	2250	↓	↓	986 893	781 560	666 407	538 264	462 195
R	3375	↓	1479 1340	1172 840	1002 612	↑	↑	↑
S	4950	2169 1965	1720 1233	1470 898	↑	↑	↑	↑
T	7500	2607 1869	2228 1361	↑	↑	↑	↑	↑
		0.010	0.015	0.035	0.065	0.10	0.15	0.25
		Acceptable Quality Levels						

AQLs are in percent defective, or in defects-per-hundred units.
H = Handicap. b = Penalty.
The target is equal to twice the Handicap.
↑ = Use first sampling plan above arrow.
↓ = Use first sampling plan below arrow.
N⇅T = Use first sampling plan above for normal inspection, first below for tightened inspection.

For Multiple Sampling:-

The Gap (G) may be calculated as the smallest whole number that is not less than $\frac{2H}{b+1}$

The Acceptance Number at the maximum sample size may be calculated as the largest whole

Table V from Defence

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15

(Normal and Tightened Inspection)

(Normal Inspection)

0.25		0.40		0.65		.1.0		1.5		2.5		4.0		6.5		10.0		Code Letter
H	b	H	b	H	b	H	b	H	b	H	b	H	b	H	b	H	b	
												5	10	3	6	2	4	A
														N	T			B
																		C
										7	14	N	T					D
						15	30	10	20	N	T			7	6	5	3	E
				25	50	N	T					9	8	7	5	6	3	F
		35	70	N	T			22	20	15	14	13	9	10	6	8	4	G
50	100	N	T			32	29	25	18	17	12	15	9	10	5	9	4	H
N	T			49	44	38	27	31	19	21	13	18	10	15	7	11	4	I
97	88	72	65	56	40	47	29	42	23	28	15	22	10	19	7	16	5	J
116	83	77	55	65	40	57	31	51	25	33	15	30	12	24	8	19	5	K
133	81	98	60	87	47	79	39	63	25	40	16	35	12	29	8	21	5	L
175	95	116	63	106	52	90	38	75	27	53	18	43	12	34	8	30	6	M
245	111	159	78	137	58	115	41	98	30	62	19	55	14	39	8	34	6	N
410	154	217	86	184	63	159	47	137	35	82	21	65	14	51	9	41	6	O
		345	109	292	79	247	57	210	41	111	23	87	15	71	10	59	7	P
										161	25	132	17	105	11	81	7	Q
																		R
																		S
																		T
0.40		0.65		1.0		1.5		2.5		4.0		6.5		10.0				

(Tightened Inspection)

number that is not greater than $\frac{\text{Maximum Sample Size}}{b + 1}$

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16

TABLE

Single Sampling Summary Table

Code Letter	Sample Size	Acceptable Quality							
		0.010	0.015	0.035	0.065	0.10	0.15	0.25	0.40
		Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re
A-E	2	↓	↓	↓	↓	↓	↓	↓	↓
F	3								0 1
G	5							0 1	↑
H	7							↑	↓
I	10						0 1	↓	1 2
J	15					0 1	↑	↑	1 2
K	22				0 1	↑	↓	↓	↓
L	30			0 1	↑	↓	1 2	1 2	2 3
M	45		0 1	↑	↓	1 2	1 2	2 3	3 4
N	60	0 1	↑	↓	1 2	1 2	2 3	2 3	4 5
O	90	↑	↓	1 2	1 2	2 3	2 3	3 4	5 6
P	150	↓	1 2	1 2	2 3	3 4	4 5	5 6	6 7
Q	300	1 2	1 2	2 3	↑	↑	↑	↑	↑
R	450	1 2	↑	↑					
S	660	2 3							
T	1000	↑							

AQLs are in percent defective, or in defects-per-hundred units.
Ac = Acceptance Number. Re = Rejection Number.

↑ = Use first sampling plan above arrow.

↓ = Use first sampling plan below arrow.

When sample size equals or exceeds batch size, do 100% inspection.

Table VI from Defence

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17

V

(Reduced Inspection)

Levels							Code Letter
0.65	1.0	1.5	2.5	4.0	6.5	10.0	
Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	Ac Re	
↓	0 ↓ 1	0 ↑ 1	0 1 ↓	↓ 1 2	1 2 1 2	1 2 2 3	A-E F
0 1 ↑ ↓	↑ ↓ 1 2	↓ 1 2 1 2	1 2 1 2 2 3	1 2 2 3 2 3	2 3 3 4 3 4	3 4 3 4 4 5	G H I
1 2 1 2 2 3	1 2 2 3 2 3	2 3 2 3 3 4	2 3 3 4 4 5	3 4 4 5 5 6	4 5 5 6 7 8	6 7 8 9 11 12	J K L
2 3 3 4 4 5	3 4 4 5 5 6	4 5 5 6 6 7	5 6 6 7 9 10	7 8 9 10 11 12	10 11 12 13 14 15	13 14 15 16 18 19	M N O
5 6 8 9 ↑	7 8 11 12 ↑	9 10 13 14 ↑	11 12 17 18 ↑	14 15 22 23 ↑	19 20 32 33 ↑	26 27 48 49 ↑	P Q R S T

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18

TABLE

Upper Limits for

Number of sample units included in estimated process average	Acceptable					
	0.010	0.015	0.035	0.065	0.10	0.15
10 - 14	0.876	1.076	1.655	2.272	2.837	3.502
15 - 19	0.738	0.906	1.396	1.919	2.400	2.966
20 - 24	0.649	0.798	1.231	1.695	2.122	2.625
25 - 34	0.566	0.697	1.077	1.485	1.861	2.306
35 - 49	0.473	0.582	0.901	1.245	1.563	1.941
50 - 74	0.391	0.482	0.748	1.036	1.304	1.625
75 - 99	0.332	0.409	0.637	0.885	1.117	1.396
100 - 124	0.293	0.362	0.565	0.787	0.996	1.248
125 - 149	0.266	0.329	0.514	0.718	0.911	1.143
150 - 199	0.238	0.294	0.460	0.644	0.818	1.030
200 - 249	0.210	0.260	0.410	0.575	0.733	0.926
250 - 299	0.191	0.237	0.374	0.527	0.673	0.851
300 - 349	0.177	0.219	0.347	0.490	0.627	0.795
350 - 399	0.165	0.205	0.325	0.460	0.590	0.750
400 - 449	0.155	0.193	0.307	0.436	0.561	0.714
450 - 549	0.144	0.179	0.286	0.407	0.525	0.670
550 - 649	0.132	0.165	0.264	0.377	0.488	0.625
650 - 749	0.121	0.151	0.247	0.354	0.459	0.589
750 - 899	0.115	0.143	0.231	0.331	0.430	0.555
900 - 1099	0.105	0.131	0.213	0.307	0.400	0.518
1100 - 1299	0.097	0.121	0.197	0.286	0.374	0.485
1300 - 1499	0.090	0.113	0.185	0.270	0.354	0.461
1500 - 1699	0.085	0.107	0.175	0.256	0.337	0.440
1700 - 1899	0.081	0.102	0.167	0.245	0.324	0.424
1900 - 2249	0.076	0.096	0.158	0.233	0.308	0.405
2250 - 2749	0.070	0.089	0.147	0.218	0.290	0.383
2750 - 3499	0.064	0.081	0.136	0.202	0.270	0.358
3500 - 4999	0.056	0.071	0.121	0.182	0.246	0.328
5000 - 6999	0.048	0.062	0.108	0.164	0.222	0.300
7000 - 8999	0.043	0.056	0.098	0.151	0.206	0.280
9000 - 10999	0.040	0.052	0.091	0.142	0.195	0.266
11000 - 13499	0.037	0.048	0.085	0.133	0.185	0.254
13500 - 17499	0.034	0.044	0.080	0.127	0.176	0.243
17500 - 22499	0.031	0.041	0.075	0.119	0.167	0.232
22500 and over	0.027	0.036	0.067	0.109	0.155	0.217

AQLs and Upper Limits are in percent defective, or in defects-per-hundred units.

Table VII from Defence

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19

VI

Estimated Process Average

Quality Levels

0.25	0.40	0.65	1.0	1.5	2.5	4.0	6.5	10.0
4.575	5.866	7.609	9.62	12.03	16.02	20.97	27.85	35.98
3.883	4.993	6.497	8.24	10.34	13.86	18.28	24.44	31.83
3.444	4.437	5.790	7.36	9.27	12.49	16.53	22.27	29.19
3.032	3.916	5.103	6.52	8.27	11.23	15.05	20.58	27.47
2.562	3.328	4.383	5.63	7.17	9.82	13.26	18.30	24.64
2.155	2.810	3.722	4.81	6.17	8.52	11.62	16.21	22.05
1.858	2.434	3.243	4.22	5.44	7.59	10.43	14.70	20.17
1.667	2.193	2.935	3.83	4.97	6.98	9.67	13.73	18.96
1.532	2.021	2.716	3.56	4.64	6.55	9.13	13.03	18.11
1.386	1.836	2.481	3.27	4.28	6.09	8.54	12.29	17.18
1.251	1.666	2.264	3.00	3.95	5.67	8.00	11.60	16.33
1.155	1.545	2.110	2.81	3.72	5.36	7.62	11.12	15.73
1.083	1.453	1.993	2.67	3.54	5.13	7.33	10.75	15.27
1.025	1.380	1.900	2.55	3.40	4.95	7.10	10.45	14.90
0.978	1.321	1.824	2.46	3.28	4.80	6,91	10.21	14.60
0.921	1.249	1.732	2.34	3.14	4.62	6.68	9.92	14.24
0.863	1.175	1.638	2.23	3.00	4.44	6.45	9.62	13.87
0.817	1.117	1.564	2.13	2.89	4.29	6.27	9.39	13.59
0.772	1.061	1.492	2.04	2.78	4.15	6.09	9.16	13.30
0.724	1.000	1.415	1.95	2.66	4.00	5.90	8.92	13.00
0.683	0.948	1.348	1.87	2.56	3.87	5.73	8.71	12.74
0.651	0.907	1.296	1.80	2.48	3.77	5.60	8.54	12.54
0.625	0.874	1.255	1.75	2.42	3.69	5.50	8.41	12.37
0.604	0.847	1.220	1.71	2.37	3.59	5.41	8.30	12.24
0.579	0.817	1.181	1.66	2.31	3.54	5.32	8.18	12.08
0.550	0.779	1.134	1.60	2.23	3.45	5.20	8.03	11.90
0.518	0.739	1.083	1.54	2.16	3.35	5.07	7.87	11.70
0.480	0.691	1.021	1.46	2.06	3.23	4.92	7.67	11.46
0.444	0.645	0.962	1.39	1.97	3.11	4.77	7.49	11.22
0.418	0.612	0.920	1.34	1.91	3.03	4.67	7.36	11.06
0.400	0.590	0.892	1.30	1.87	2.97	4.60	7.27	10.95
0.384	0.570	0.866	1.27	1.83	2.92	4.54	7.18	10.85
0.371	0.552	0.844	1.24	1.80	2.88	4.48	7.11	10.76
0.356	0.534	0.821	1.21	1.76	2.84	4.42	7.04	10.64
0.337	0.510	0.790	1.17	1.71	2.77	4.35	6.93	10.52

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20

TABLE
Lower Limits for

Number of sample units included in estimated process average	Acceptable					
	0.010	0.015	0.035	0.065	0.10	0.15
Up to 99	*	*	*	*	*	*
100 - 124	*	*	*	*	*	*
125 - 149	*	*	*	*	*	*
150 - 199	*	*	*	*	*	*
200 - 249	*	*	*	*	*	*
250 - 299	*	*	*	*	*	*
300 - 349	*	*	*	*	*	*
350 - 399	*	*	*	*	*	*
400 - 449	*	*	*	*	*	*
450 - 549	*	*	*	*	*	*
550 - 649	*	*	*	*	*	*
650 - 749	*	*	*	*	*	*
750 - 899	*	*	*	*	*	*
900 - 1099	*	*	*	*	*	*
1100 - 1299	*	*	*	*	*	*
1300 - 1499	*	*	*	*	*	*
1500 - 1699	*	*	*	*	*	*
1700 - 1899	*	*	*	*	*	*
1900 - 2249	*	*	*	*	*	*
2250 - 2749	*	*	*	*	*	*
2750 - 3499	*	*	*	*	*	*
3500 - 4999	*	*	*	*	*	*
5000 - 6999	*	*	*	*	*	*
7000 - 8999	*	*	*	*	*	0.020
9000 - 10999	*	*	*	*	0.005	0.034
11000 - 13499	*	*	*	*	0.015	0.046
13500 - 17499	*	*	*	0.003	0.024	0.057
17500 - 22499	*	*	*	0.011	0.033	0.068
22500 and over	#	##	0.003	0.021	0.045	0.083

AQLs and Lower Limits are in percent defective, or in defects-per-hundred units.
* = Number of sample units included in estimated process average is insufficient for reduced
# = Reduced inspection is permissible for AQL 0.010 only if no defects have been observed in
= Reduced inspection is permissible for AQL 0.015 only if no defects have been observed in

Table VIII from Defence

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21

VIII.

Estimated Process Average

Quality Levels

0.25	0.40	0.65	1.0	1.5	2.5	4.0	6.5	10.0
*	*	*	*	*	*	*	*	*
*	*	*	*	*	*	*	*	1.04
*	*	*	*	*	*	*	*	1.89
*	*	*	*	*	*	*	0.71	2.82
*	*	*	*	*	*	*	1.40	3.67
*	*	*	*	*	*	0.38	1.88	4.27
*	*	*	*	*	*	0.67	2.25	4.73
*	*	*	*	*	0.05	0.90	2.55	5.10
*	*	*	*	*	0.20	1.09	2.79	5.40
*	*	*	*	*	0.38	1.32	3.08	5.76
*	*	*	*	*	0.56	1.55	3.38	6.13
*	*	*	*	0.11	0.71	1.73	3.61	6.41
*	*	*	*	0.22	0.85	1.91	3.84	6.70
*	*	*	0.05	0.34	1.00	2.10	4.08	7.00
*	*	*	0.13	0.44	1.13	2.27	4.29	7.26
*	*	0.004	0.20	0.52	1.23	2.40	4.46	7.46
*	*	0.045	0.25	0.58	1.31	2.50	4.59	7.63
*	*	0.080	0.29	0.63	1.41	2.59	4.70	7.76
*	*	0.119	0.34	0.69	1.46	2.68	4.82	7.92
*	0.021	0.166	0.40	0.77	1.55	2.80	4.97	8.10
*	0.061	0.217	0.46	0.84	1.65	2.93	5.13	8.30
0.020	0.109	0.279	0.54	0.94	1.77	3.08	5.33	8.54
0.056	0.155	0.338	0.61	1.03	1.89	3.23	5.51	8.78
0.082	0.188	0.380	0.66	1.09	1.97	3.33	5.64	8.94
0.100	0.210	0.408	0.70	1.13	2.03	3.40	5.73	9.05
0.116	0.230	0.434	0.73	1.17	2.08	3.46	5.82	9.15
0.129	0.248	0.456	0.76	1.20	2.12	3.52	5.89	9.24
0.144	0.266	0.479	0.79	1.24	2.16	3.58	5.96	9.36
0.163	0.290	0.510	0.83	1.29	2.23	3.65	6.07	9.48

inspection.
the last 90000 units inspected.
the last 60000 units inspected.

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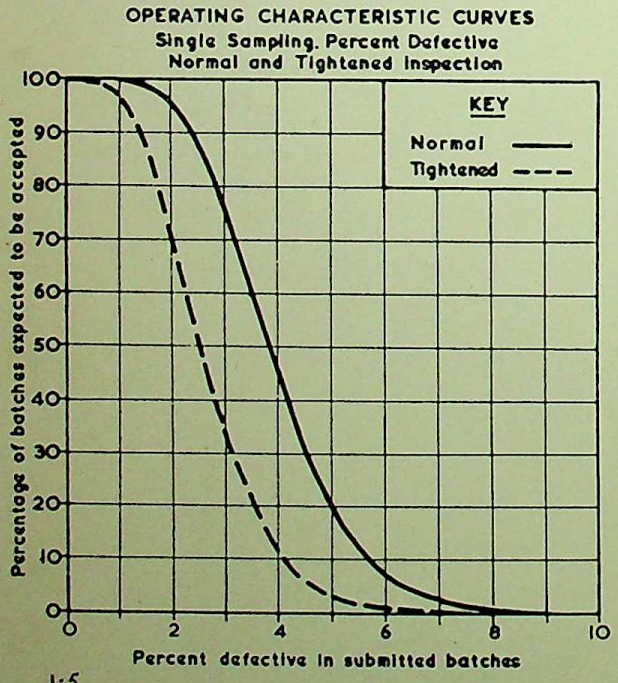
93

A Q L 1.5 Code Letter M	Normal Inspection	Tightened Inspection	Reduced Inspection
SINGLE SAMPLING			
Sample Size	225	225	45
Acceptance Number	8	5	4
Rejection Number	9	6	5
DOUBLE SAMPLING			
First Sample Size	90	90	
Acceptance Number	2	1	
Rejection Number	6	5	
Second Sample Size	180	180	
Combined Sample Size	270	270	
Acceptance Number	9	6	
Rejection Number	10	7	
SEQUENTIAL SAMPLING & MULTIPLE SAMPLING			
Handicap (H)	63	79	
Penalty (b)	25	39	
Target (2H)	126	158	
Gap (G)	5	4	
Maximum Sample Size	340	340	
Acceptance Number	13	8	
Rejection Number	14	9	

Inspection Level	Batch sizes
IC	-
IB	-
IA	-
I	8001 - 22000
II	3201 - 8000
III	1301 - 3200
IV	301 - 500



Number of Sample Units	Limits for Estimated Process Average	
	Upper	Lower
10 - 14	12.03	*
15 - 19	10.34	*
20 - 24	9.27	*
25 - 34	8.27	*
35 - 49	7.17	*
50 - 74	6.17	*
75 - 99	5.44	*
100 - 124	4.97	*
125 - 149	4.64	*
150 - 199	4.28	*
200 - 249	3.95	*
250 - 299	3.72	*
300 - 349	3.54	*
350 - 399	3.40	*
400 - 449	3.28	*
450 - 549	3.14	*
550 - 649	3.00	*
650 - 749	2.89	0.11
750 - 899	2.78	0.22
900 - 1099	2.66	0.34
1100 - 1299	2.56	0.44
1300 - 1499	2.48	0.52
1500 - 1699	2.42	0.58
1700 - 1899	2.37	0.63
1900 - 2249	2.31	0.69
2250 - 2749	2.23	0.77
2750 - 3499	2.16	0.84
3500 - 4999	2.06	0.94
5000 - 6999	1.97	1.03
7000 - 8999	1.91	1.09
9000 - 10999	1.87	1.13
11000 - 13499	1.83	1.17
13500 - 17499	1.80	1.20
17500 - 22499	1.76	1.24
22500 & over	1.71	1.29

A typical page from the "extended tables" of Defence Specification DEF-131.

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Discussion on Mr Hill's Paper

Dr G. Horsnell: It is over four years since the Society last discussed sampling inspection schemes at an Ordinary Meeting.

We are grateful to Mr Hill for providing us with another opportunity of airing this interesting topic. His paper marks the official recognition by Her Majesty's Government, in the form of the Ministry of Defence, of the part that sampling inspection should play in Government contracts in general. This recognition, although somewhat tardy (in fact, over ten years later than the Americans'), is encouraging, and Mr Hill, as Chairman of the sub-committee responsible for the production of DEF-131, and the other sub-committee members are to be congratulated on the part that they have played.

Throughout the paper there is the suggestion that an incentive to a producer to produce large batch sizes is desirable and it is certainly true of all published sampling schemes that large batches are more economical to deal with than small. This is as it should be but, if we think of batches in the Shewhart phrase as "rational sub-groups", we do not want a producer to submit larger batches by grouping several "rational sub-groups" together, and some safeguards against this should be introduced into the contract.

Little is said in the paper on how an inspection level should be chosen. It is often useful to think of an O.C. curve as being determined by two points, a percentage defective for which the O.C. curve has a high probability of acceptance, say 0·95, and a percentage defective with a low probability of acceptance, say 0·05. The ratio of these two percentages defective has proved a very useful indexing device and is directly related to inspection level at a given AQL. It would be interesting to look at the DEF-131 inspection levels from this point of view. Tables of single sampling plans using this type of index have been published, including one by myself in *Applied Statistics*, in 1954.

A minor comment on switching from normal to tightened inspection is that the tables seem to provide for switching to tightened inspection if sample results from less than five successive batches are accumulated. I wonder if this is a good idea? In any case, it would appear that some printing could have been saved, for switching limits are quoted for numbers of units less than the size of a single sample, as can be seen from the typical page from DEF-131 in the Appendix to Mr Hill's paper.

My chief comments are concerned with DEF-131 itself. It will be no surprise to Mr Hill when I say that I regard all published tables of sampling schemes with suspicion and especially those which pay no heed to economic aspects. The relationship between batch and sample size in DEF-131 is arbitrary and only governed empirically by economic considerations. It can, in fact, lead to some very odd results, as I will demonstrate.

Suppose that inspection is destructive and that we wish to operate a very stringent inspection plan on batches of 10,000 items, where the AQL is 1 per cent. defective. We might choose inspection level IV and from Table I we see that the code letter is Q. From Table III we see that the single sampling plan recommended has sample size $n = 1{,}500$, acceptance number 25 and rejection number 26. From DEF-131, or from probability tables, one finds that this plan gives a probability of 0·05 of accepting a single batch containing 2·33 per cent. defective. Now there will be a large number of single sampling plans each giving a probability of 0·05 of acceptance at 2·33 per cent. defective, and one can arrange them in order in the form of a table with, alongside the plans, the probability of accepting batches of AQL quality. As inspection is destructive, each batch accepted contains only $(N-n)$ items where N is the batch size and n the sample size. Hence, per batch offered at AQL quality, $(N-n)\,A(n, k, p)$ items are accepted where $A(n, k, p)$ is the probability of accepting at the AQL with the plan (n, k), k being the rejection number of the plan.

It will be seen from the final column that values of $(N-n)\,A(n, k, p)$ rise to a maximum of 8,632 for the scheme $n = 1{,}095$, $k = 18$, and for the DEF-131 scheme where $n = 1{,}500$ the value of $(N-n)\,A(n, k, p)$ has fallen to 8,447. It seems very undesirable that we should be told to sample an extra 400 items in order to accept nearly 200 satisfactory items per offered batch less than the maximum we could get.

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The same plan is proposed for a batch size of 20,000. Here, with a 1 per cent. AQL as before, the maximum single sample size that we need ever take is $n = 1{,}300$ and $k = 22$, giving the same protection at 2·33 per cent. as the DEF-131 plan but resulting in an average number of accepted items per submitted batch at the AQL of 18,437 against the DEF figure of 18,386, i.e. sampling 200 items less to produce on average 50 more accepted items.

Similar criticisms apply at AQLs as low as 0·1 and will apply more strongly at higher AQLs for inspection level IV. My general conclusion would be that inspection level IV

Single-sampling plans : AQL 1·0 *: probability of acceptance at* 2·33 = 0·05

Sample size n	*Acc.* $k-1$	*Re.* k	*Prob. of Acc. at AQL*	$(N-n)$	$(N-n)\,A(n, k, p)$
940	14	15	0·9445	9,060	8,558
990	15	16	—	9,010	—
1,045	16	17	0·9624	8,955	8,620
1,095	17	18	0·9692	8,905	8,632
1,145	18	19	0·9746	8,855	8,629
1,195	19	20	—	8,805	—
1,245	20	21	0·9831	8,755	8,605
1,300	21	22	0·9861	8,700	8,581
.	.	.	.	.	.
.	.	.	.	.	.
.	.	.	.	.	.
1,500	25	26	0·9938	8,500	8,447

should not be used for destructive inspection. It is interesting too that there are isolated places in the other inspection levels where the same type of criticisms apply. Such a case is code letter P inspection level III for a batch size of 25,000. Here a reduction in single sample size of 45 increases the number of items accepted per batch at AQL of 1 per cent. by 9.

If sampling is very much more expensive than the actual cost of the items, it may pay to sample fewer than the number that would secure a maximum return per submitted batch at the AQL. For example, in the table it would be better to sample 940 items and get an average of 8,560 back rather than 1,095 and get 8,630 back if the cost of sampling the extra 155 items were greater than the value of the extra 70 items won back, both schemes of course providing the same protection against bad quality. I have developed this approach to specifying acceptance sampling plans more fully elsewhere both for destructive and non-destructive inspection.

Whether this approach is accepted or not, it is instructive to work out the cost of any sampling plan that may be recommended and weigh this against the protection that is being provided by the plan and ask the question: "Is it really worth it?" In many cases, a quality surveillance plan of the deferred sentencing scheme type is all that is required.

Mr Hill has mentioned Dr Warner's and our joint paper on deferred sentencing schemes. I do not think that this has received the attention and started the research and application that it deserved. Perhaps if we rephrase the proposed schemes in the currently popular cumulative sum nomenclature, this may bring them more to general notice.

The schemes correspond to an extension of one-sided cumulative sum schemes where the reference value is zero. We imagine that batches are coming forward for inspection in sequence. In the example given in our paper we assumed that we could sample ten items from each batch and, where necessary, a further twenty. We may plot out the results

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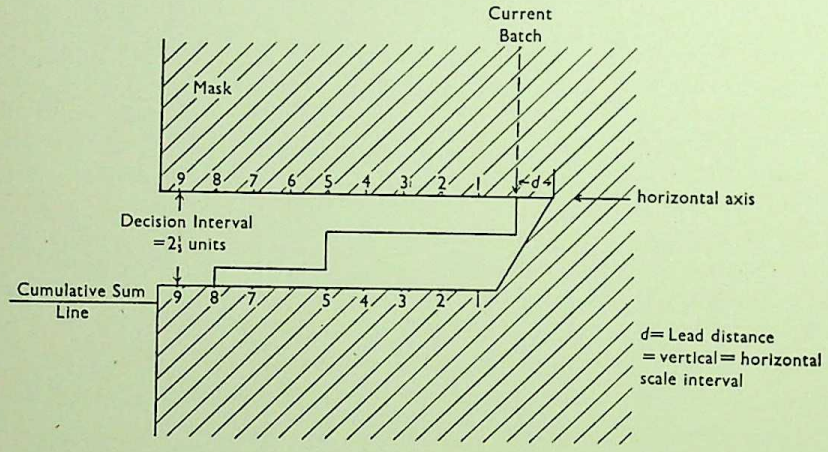
in the form of a cumulative sum chart, plotting the cumulative number of defectives vertically against batch number where the vertical and horizontal plotting intervals are equal. It is convenient to use a one-sided mask on the results, where the shape of the mask is determined by the properties required from the deferred sentencing plan. In our example, we required a plan that would accept batches of 1 per cent. defective quality on over 95 per cent. of occasions on which they are offered and would reject batches of quality worse than 1 per cent. defective as frequently as possible. We chose to look at a maximum of ten consecutive batches, starting with a batch that gave one or more defectives in its first sample. If a batch gave two or more defectives in its first sample, it was immediately scheduled for re-sampling. If it gave no defectives or one defective and the batch was included in a sequence of ten batches or fewer with a combined total of three or more defectives in their first samples, the whole sequence of ten or fewer batches, beginning and ending with a batch showing at least one defective in their first samples, was scheduled for re-sampling. If these conditions are replaced by a mask to be used on a cumulative sum chart of defectives, the shape of a suitable mask is as shown:



A deferred sentencing scheme mask.

In the diagram the mask vertex, having been put at one scale interval in front of the point plotted from the current batch, has resulted in the cumulative sum line disappearing below the mask at eight batches in front of the current batch. The action to be taken is then to re-sample these nine batches, i.e. the current batch and the eight preceding batches, for three defectives have been sampled from these nine consecutive batches. If the cumulative sum line disappears below the right-hand inclined edge of the mask, the single batch for which this happens is re-sampled (because the plan requires a single batch showing two or more defectives on the first sample to be re-sampled). When batches are re-sampled, the first and second sample results are combined and a batch is rejected if three or more defectives have been sampled from it or the batch occurs in a sequence of three or less with a combined total of three or more defectives in their first and second samples together. Such a sequence must begin and end with a batch with at least one defective in either its first sample or its second sample and every batch in the sequence is rejected. A suitable mask can easily be constructed. If the two masks are cut out of one sheet of card and the cumulative results from second samples are added to the cumulative sum from first samples, the operation of the plan is greatly simplified. If the cumulative sum line does not disappear below the edge of the first mask, the batches concerned are accepted and are not re-sampled, and if the combined cumulative sum line from the first and second samples does not disappear below the lower edge of the second mask, the re-sampled batches are accepted.

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In my opinion, the DEF-131 plans do represent an advance on MIL-STD-105 in a number of ways, notably in double and sequential sampling. I have much pleasure in proposing the vote of thanks to Mr Hill.

Dr G. B. WETHERILL: First I should like to refer to p. 42, Section 7, where Mr Hill says, "It is not the aim here to reach an optimum from the cost point of view within the confines of a fixed prior distribution". In the previous sentence he refers to economic schemes "based upon a complete knowledge of prior distributions and costs". The implication seems to be that all such schemes, based on minimizing costs in some way, are critically dependent on the parameters of the assumed model, and in fact so much so that rough knowledge of these quantities is useless. Some recent work on this, though far from complete, suggests that the opposite is true, and that the optimum of economic sampling schemes is very flat to most of the parameters of the model.

Dr Vagholkar (this *Journal*, B, **21**, 1959) gave a method of fitting what he called "equivalent" two-point binomial prior distributions to any prior distribution and, when tried out, this method gave sampling plans almost identical to those obtained using the original prior distribution. If this is so, then it follows that the form of prior distribution assumed is not critical.

Professor Anscombe (1958, 1961)† has used the minimax principle, and obtained sampling schemes which do not assume any detailed knowledge of the prior distribution. In the latter paper only one cost quantity has to be estimated, which is effectively the ratio cost of inspecting an item/cost of passing a bad item. Once this is known, the sampling plan can be written down. Professor Anscombe is able to show that, even if one had full knowledge of the prior distribution, the efficiency of the plan could be improved only by an amount usually negligible.

There is evidence to suggest, therefore, that "economic" schemes are not critically dependent on "full knowledge of the prior distribution". One quantity which needs to be estimated fairly well in such schemes is the break-even quality—but the sampling plans of DEF-131 are dependent on quantities rather similar to this.

For any given batch size, DEF-131 gives us several inspection levels, but there is only vague guidance of which to use. I should have thought that here some simple rough "economic" calculations would be of value either by guessing a prior distribution based on past experience, or by using the minimax principle and avoiding assumptions of the prior distribution.

My second point concerns the process average. Mr Hill rightly stresses that it is the process average rather than the sampling plan which governs the quality the consumer is going to get. However, in the paper the emphasis is on the sampling plan as being the important thing, and the process average is regarded merely as an indication of when to switch between normal, tightened and reduced inspection. I wonder if, in some situations at least, this emphasis is not misplaced. The cumulative sum chart could be used for the process average. When this indicated a drop in the average, the average (say of the past five batches) would be determined. Two possibilities are then open to us: (i) to reject a group of batches, (ii) to reduce the price being paid (or pay a "bonus" at other times), until we have evidence that the process average is acceptable. This comes very close to the "serial sampling" schemes discussed by Professors Barnard and Cox.

Clearly, there must be safeguards to ensure that individual batches below the acceptable level cannot slip through. The way to look at this would be by the *average run length* concept.

Finally, Mr Hill must have a wealth of experience of operating sampling schemes in different situations. I had hoped that we might benefit from this by a deeper analysis of how proposed models and schemes are or are not appropriate in practical cases. For

† ANSCOMBE, F. J. (1958), "Rectifying inspection of a continuous output", *J. Amer. statist. Ass.*, **53**, 702–720; (1961), "Rectifying inspection of lots", *J. Amer. statist. Ass.*, **56**, 807–823.

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instance, perhaps several sets of tables such as DEF-131 are really required when sampling inspection is to serve different purposes. Perhaps Mr Hill could be encouraged to continue his work on some other occasion.

I have much pleasure in seconding the vote of thanks.

The vote of thanks was put to the meeting and carried unanimously.

Professor BARNARD: I am glad to associate myself with the remarks of the Chairman about the value of Mr Hill's paper. I hope that in so doing by way of preface I may perhaps correct the balance of my remaining remarks, some of which may seem more critical.

The first point concerns the terms "sampling scheme" and "sampling plan". The distinction which Mr Hill draws is a very useful one. It was considered by Miss Garforth in a Diploma thesis she wrote a few years ago at Imperial College and we suggested that the term "sampling scheme" should be used, more or less as at present, to cover things like the Dodge–Romig scheme which is a collection of sampling plans; but a system whereby you choose the plan you want we called a system.

Another point is that the problem considered by the Committee which set up DEF-131, when a *sequence* of batches is offered for acceptance, is really a problem in control engineering. The sequence of batches represents a process which you wish to control, and I think an approach from the point of view of some of the steps involved in control engineering might have been more helpful. One particular thing which would have come out of this would have been a description of the scheme, not in terms of the O.C. curve, giving the probability of acceptance as the function of batch quality, but in terms of the run length, the average run length (ARL) being the average amount of production passed after the quality has deteriorated before the deterioration is picked up by the inspection. This quantity is of most interest to users of inspection schemes, as it represents the average amount of bad quality they are liable to get before they can spot it. Other points in favour of ARL are (i) that it shows better the effect of increased batch size—doubling the batch size, leaving all else the same, doubles the ARL, but the O.C. curve is, misleadingly, left unaltered; and (ii) that for certain types of highly efficient serial sampling schemes, the ARL is well defined, while the O.C. curve is not. Table 3 of the paper would, I think, show up much better in terms of ARL than it does now.

My major point has already been stressed by the preceding speakers. It is, that I feel it is a great pity that the Committee did not look more seriously at the economic aspects of inspection. It really is not good enough at this stage to dismiss so readily the tremendous amount of work which has been done on the economic side of sampling inspection schemes. The fact that the process curve is liable to change is a valid and very useful point made by Mr Hill in an earlier paper, but one feels that the Committees who drew up these schemes consisted largely of users rather than producers because not many of them suggest recognition of the difficulties in changing prior distributions in the way in which they are being required to do by the schemes proposed. It is much more difficult to change the conditions of production than one might think.

As to the difficulties which Mr Hill mentions in the third paragraph of Section 7, the differences between the economic schemes which should be applied to manufacturers who have different prior distributions, this surely could be readily got over by adopting a *vendor rating scheme* of the kind which is already in use by several large engineering organizations and which should be seriously considered for adoption by the Government. The producer of good average quality gets the benefit of such schemes.

Dr Wetherill has mentioned the break-even quality, and my own feeling is that if we are to concentrate on a single quality the break-even quality is the one to look at. The concept of the AQL being defined in the way it is in (8.3), as the maximum percentage defective which can be considered satisfactory, embodies to some extent an attitude to

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quality which is, I fear, all too common. I do not think one should look at the worst one can get away with, a quantity which in any case is much less stable than the prior distribution. If one is exposed to the cold winds of the Common Market one may find that the amount one can get away with is drastically reduced! No one would think of regularly producing break-even quality. We should always think in terms of producing regularly the best quality we can produce, and the higher the quality the better.

Dr. S. R. BROADBENT: In my limited experience of acceptance sampling in industry I have yet to come across an example of the full application of MIL-STD-105, starting from the choice of an inspection level and going on to use, when called for, tightened or reduced inspection. However, I know several cases where individual sampling plans are agreed by manufacturer and buyer. In these cases the all-important question of the true process average is left to normal commercial negotiation. For example, a buyer who draws from three suppliers all of whose deliveries are usually accepted by the sampling plan will still feel himself free to point out to the supplier with the worst process average what his position is and, if he thinks desirable, to stop drawing from him. Equally, he will feel free to take, if he is in great need of it, a batch which the plan says he should reject, although he may complain. It seems to me that this sort of operation is defensible on the grounds of simplicity and flexibility, but it is not explicitly allowed for in DEF-131. Does Mr Hill think the strict application of a full sampling scheme would always, commercially, be an improvement?

My experience also suggests that the choice of a plan through Tables I or II of DEF-131 may be not only arbitrary but misleading. Negotiation between buyer and supplier can often be adequately based on the more primitive notions of the consumer's and producer's risk points, coupled with the cost of inspection.

I am grateful to Mr Hill for distinguishing so clearly between the AQL and the producer's risk point, and if the tables are to be used in the way I have suggested it must always be remembered that the two may be different. In fact, at small sample sizes the producer is not likely to agree to an AQL which results in 20 per cent. of his batches with this quality being rejected. Once the top portion of the O.C. has been agreed, it is up to the buyer to determine the plan by comparing the costs of inspection with the resultant movements of the lower end of the O.C. This method of choice seems to me to be the alternative looked for in Section 8.4—it can be carried out if necessary "in advance of production" at least as realistically as the choice of an AQL. The method is outlined in the last paragraph of Section 8.4, although the cost of inspection is not there explicitly mentioned, and from sampling plan we seem to go to inspection level and back again. An advantage of this method is that any convenient set of plans may be used.

Incidentally, I agree with Mr Hill on the economic aspects of inspection and, in particular, that the gain in moving the lower end of the O.C. to the left may be unimportant. It would be interesting to try the plan of passing incoming batches through a black box which rejected 1 per cent. at random (this process would be kept secret from the supplier). The psychological effect would probably be very beneficial.

I am doubtful about the statement in Section 8.9 that "It seems probable that multiple sampling plans would not generally be used", i.e. compared with sequential plans. I know several examples of multiple plans, none of sequential. The reason is, of course, the inconvenience of sampling, however exceptionally, going on for a long time.

I think it would be useful to indicate the expected size of sample (for all except single-sampling plans) for various percentages of defective in the batch, perhaps one-half, one and two times the AQL. This table might appear in place of the extracts from Tables I and II on each page. The information would be useful in two ways, firstly in choosing between plans, where I have already indicated that the cost of inspection, i.e. size of sample, is an important factor, and secondly, in helping to choose between single sampling and other types, where smaller sample sizes may be offset by greater complexity of operation and uncertainty about the size of sample.

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Mr K. Hopkinson: I have had great pleasure in reading Mr Hill's paper. In the electronic valve side of our work we have been using the MIL-STD-105A single-sampling schemes for about ten years. We published the essential material of the MIL document as part of our K1001 basic valve specification and we have had a lot of experience in using these schemes. It is my responsibility to write specifications for electronic valves, and when we decided to use acceptance sampling techniques for the many different valve parameters which we wished to control I deemed it to be my responsibility to set acceptance limits, inspection levels and AQLs at one and the same time. I feel strongly that this should never be decided otherwise.

One difference between our K1001 use of the MIL-STD-105A scheme and the original was that we decided to limit the lot or batch to one week's production. This has led us into many difficulties, particularly for small-scale production.

We have usually tested:

Major electrical parameters, at inspection level II, 0·65 per cent. AQL;
Minor electrical parameters, at inspection level I, 1 per cent. AQL;
Incidental electrical parameters, at inspection level IC, 6·5 per cent. AQL.

In addition, we have used IA or IC inspection levels with 6·5 per cent. AQLs for environmental and life tests.

We have been severely criticized for using these very low inspection levels because if consideration is given to the consumer's risk end of the O.C. curve it is possible that a product having, for instance, a 48 per cent. per 1,000-hour life test failure rate may be accepted on a 6·5 per cent. AQL plan, particularly for small lot sizes. However, over the years we have found that manufacturers have had to work to a much tighter process average than the AQL in order to avoid batch rejections. Process averages of 0·9 per cent. have been quite common when 6·5 per cent. AQLs have been specified. Mr Hill has been right in emphasizing the importance of the process average.

Recent activities in America have shown the need for much lower failure rates and figures of 0·001 per cent. per 1,000 hours have been quoted. These "dynamic decimals" are receiving much attention, particularly for space research requirements, and the specification of the consumer's risk end of the O.C. curve is becoming more usual. These LPTD sampling schemes have had much publicity and we have been influenced to include one such scheme in our new semiconductor specification. These schemes tend to become economically impossible. For instance, we are being asked to specify that the manufacturers shall put some 4,000 devices on life test per batch and have only one failure.

More work, thought and investigation are necessary in this area, and we would strongly recommend a return to the use of process averages as an acceptance criterion.

In the semiconductor side of industry we are not at all sure that the accuracy of measurement of semiconductor parameters is sufficiently good to enable even the AQL schemes to be applied with reasonable confidence.

Considerable improvements must be made to the stabilization of test temperatures, etc., before the above levels of inspection can be meaningful, otherwise inaccuracies of measurement will completely swamp the significance of any sampling schemes and make the results valueless.

Dr P. G. Moore: The first point I examined in Mr Hill's paper was the amount of inspection which was required, as against the different batch sizes. Looking at Table I, inspection level IV, the percentages to be inspected are as follows:

Batch size	*Inspection* %
500	50
1,000	30
5,000	25
10,000	15
50,000	6·6

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These are all fairly large percentages and especially large for the smaller batch sizes. It is commonly believed that in industry the batch sizes one works with are extremely large, but I do not think this is necessarily so. For example, I give here a percentage breakdown of the batch size of a certain type of container made by one of my firm's factories in 1960:

Batch size	*% of orders*
under 1,000	23
1,000–	25
2,000–	12
3,000–	5
4,000–	3
5,000–	13
6,000–	5
7,000–	14

Roughly speaking, one-half of the batches made are under 2,000 in size and the amount of inspection involved could become quite large in following some of these plans. This is especially important in an industry where rectification of items is not possible, the testing is destructive, and the commodity concerned a relatively cheap one, so that the pressure is to keep the cost down by reducing the amount of inspection to an absolute minimum.

There is a move amongst consumers now to try to write specification clauses into contracts. These clauses usually tend to be MIL-STD-105A and probably will now be of the form of DEF-131. The one difference is that usually it is not possible to state categorically what is defective and what is not; it is usually done by specifying that some measurable character shall not fall below a certain point. There is a marked tendency to work in terms of minima all the time and not with the process average. I feel that in all specifications this idea of process average should be kept clearly in mind, although this is commonly not done. For example, a clause given by industry X states, "unless otherwise specified two boxes shall be taken for test, if either of the boxes does not comply with requirements ten additional boxes shall be selected and if any of these does not pass inspection the lot may be rejected". The requirement is that the bursting strength of the board must not exceed a certain limit. The Railway Clearing House tests have the same type of requirement, that the average of each individual sample shall be above a certain minima. Although there are marginal differences between the minima set by different organizations, these differences are much larger when translated into terms of process average. The difference between the industry and the Railway Clearing House specifications regarding the burst strength of similar grades of board is 35 lb. per sq. in. Translated into terms of process average the difference is 60 lb. per sq. in. on a normal figure of approximately 200.

One is often trying to achieve too much in small batches. If anything more intensive is done small sample tests may not always achieve the right end; manufacturers become more anxious to reduce variability in order to keep the item over the limit rather than to improve process average. One manufacturer may have a process average which is 25 per cent. lower than another, but because he has managed to reduce his variability he will meet the specification rather more easily. Yet this may not always be what the customer wants and it has led to schemes for trying to overcome the difficulties of combining batches by using cumulative sum charts, and so on.

One difficulty is to draw up the clause in the contract to cover this latter type of scheme which tends to be slightly complicated and not readily understood when put on paper. A simplified version of such a scheme is given by British Standard Specification No. 2095 which deals with industrial safety helmets. In this there is a clause stating that when a certain steel bolt is dropped on to the helmet making a dent, the diameter of the dent may not exceed 4·8 mm. and the average of the last ten dents may not exceed 4·45 mm. This type of procedure enables batches to be grouped together. I feel that to overcome some

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of the difficulties mentioned these more sophisticated methods must be put in fairly simple terms if they are to be got across in the actual contract, and have the maximum effect.

Dr K. A. J. CRAWFORD: I am not entirely convinced about the merits of including the extended tables discussed in Section 8.11. Firstly, anyone with sufficient statistical training to appreciate the finer differences between single, double and sequential sampling would, I think, be able to make these comparisons for himself from the summary tables. Secondly, if the person using the tables has already decided to use single, double or sequential sampling he would just as easily get what he wants directly from the summary tables. If the extended tables were eliminated, it would reduce the bulk by about four, which would bring the price down and make the tables more widely distributed.

The following contribution was received in writing after the meeting:

Professor A. HALD: I should like to add my congratulations to Mr Hill on his important and stimulating paper. The survey of previously published sampling schemes and the reasons for basing DEF-131 on MIL-STD-105 are useful starting-points for a discussion of some of the basic principles in this field.

It is interesting to notice the trend in the indexing or labelling of the sampling schemes during the forty years this subject has been discussed. The development may roughly be expressed by the following inequalities: LTPD > AOQL > $p_{0\cdot95}$ > AQL > 0. The labelling of the first developed plans indicated the protection obtained by the consumer whereas the latter systems indicate the quality required by him. From the economic incentive aspect of sampling inspection, advocated so strongly by Mr Hill, the indexing by AQL is preferable, and it seems natural to ask whether such an indexing could not be applied to the Dodge–Romig tables. The Dodge–Romig LTPD tables could be rearranged and relabelled promoting the process average to AQL and degrading the LTPD to $p_{0\cdot10}$. Naturally, to each AQL we shall then get a selection of plans corresponding to the various values of $p_{0\cdot10}$ but this is also so in DEF-131 with its seven inspection levels. To get a fair comparison of the two schemes in practice it would be necessary to present the relabelled Dodge–Romig tables in a similar streamlined layout as the MIL-STD-105 and DEF-131. In the Dodge–Romig tables as in DEF-131 the producer's risk decreases with increasing batch size.

Mr Hill criticizes the Dodge–Romig schemes—both in the present paper and in his 1960 paper—for leading to unreasonable results for the producer when tightened inspection is employed. This conclusion, however, again depends on keeping the LTPD fixed and letting the process average vary. It seems to me that it would be just as "natural" to keep the *ratio* between process average and LTPD fixed and let the process average (AQL) vary. Consider, for example, the following rule for introducing tightened inspection: If the estimated process average is significantly higher than the specified process average (AQL), then use the plan with the same $k = \bar{p}/p_t$ but with the next lower p_t. The effect of this rule will be very similar to the effect of the rule for tightened inspection in DEF-131.

As far as I can see, Mr Hill achieves the degree of economic incentive of the sampling scheme by *specifying the average quality required* by the consumer and by *alternating between normal and tightened inspection* thus letting the system react upon changes in the production process.

My remarks above show that the Dodge–Romig LTPD scheme can easily be applied according to Mr Hill's ideas. If the average quality produced is equal to the average quality required, the scheme has the further advantage of leading to the minimum amount of inspection and offering a specified protection against accepting batches of unsatisfactory quality.

In a forthcoming paper I prove that the Dodge–Romig LTPD scheme has the following asymptotic properties: (1) the sample size is proportional to the logarithm of lot size, (2) $c/n - p_t$ is inversely proportional to $\sqrt{n}$, (3) the producer's risk is inversely proportional

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to lot size, and (4) the average amount of inspection for lots of process average quality apart from sampling inspection is independent of lot size. When comparing the Dodge–Romig scheme with other schemes it is useful to keep these simple and attractive properties in mind.

One of the reasons for my rather detailed defence of the Dodge–Romig scheme is that I think a similar defence will be possible for the more fully specified economic schemes based on prior distributions. It is quite possible to start from a hypothetical prior distribution (AQL) required by the consumer and corresponding to the price the consumer is willing to pay. If the distribution of submitted lots is poorer than required, the result will be excessive rejections and the introduction of tightened inspection to force the producers to submit better quality. Even if the proponents of Bayes' solutions are developing their schemes on the basis of a fixed prior distribution (which eventually will lead to the tabulation of plans corresponding to various values of the parameters in the type of distribution chosen), this does not imply that they are not aware of the interdependence between costs, distribution of submitted quality and the sampling scheme, but the possibility of experimenting with a "dynamic" Bayes' scheme hinges on the availability of adequate tables. The main difficulty for constructing a set of such tables is that adequate information on prior distributions and cost parameters is not at hand. For more than a year I have had the complete single-sampling Bayes' solution for the linear cost model and the prior distributions discussed in my 1960 paper coded on an electronic computer and I am willing to compute tables of this kind for well-specified inspection problems in exchange for the corresponding empirical evidence of cost parameters and prior distributions.

By studying MIL-STD-105 I have tried to disclose the basic relationships between the parameters. Looking first at the relation between lot size and sample size for single sampling and inspection level II (the level ordinarily used), it will be seen that $\log n$ *is a piecewise linear function of* $\log N$ *with decreasing slope.* (Plot n against the geometric mean of the limits for the lot size intervals.) Essentially the relationships are given by

$$\log n = -0{\cdot}36 + 0{\cdot}80 \log N \ (10 < N < 1{,}000)$$

and

$$\log n = 1{\cdot}20 + 0{\cdot}31 \log N \ (5{,}000 < N < 250{,}000).$$

These two lines have been joined by a third to cover the small interval $(1{,}000 < N < 5{,}000)$ which gives a more smooth appearance of the whole curve. One would think from the diagram that originally the authors had the idea of a linear double-log relationship between N and n but that the results of using one slope over the entire range in some way disagreed with their experience of what constitutes a reasonable relationship in practice. It is, however, interesting to notice that the two slopes, 0·80 and 0·31, are on each side of 0·50 and their average is not very far from 0·50 which is the correct slope corresponding to a Bayes' scheme based on a continuous prior distribution. The Bayes' solution with discontinuous prior distribution and the Dodge–Romig schemes lead, however, to semi-log relations between N and n. The relationship between lot size and sample size in MIL-STD-105 and DEF-131 is therefore not only perfectly arbitrary but also disagrees with all existing theoretical relationships.

The only simple relation between c and n in MIL-STD-105 I have been able to find is a linear double-log relation between $(c+1)$ and n for each AQL, i.e.

$$\log (c+1) = \alpha(p) + \beta(p) \log n$$

where p denotes AQL. For the normally employed AQL values the slope is approximately 0·8 but it increases slowly with AQL. This kind of relationship does not seem to have any theoretical foundation and it leads to a rather complicated expression for the producer's risk as a function of lot size.

The opening remark in Dodge and Romig's paper (1929) is: "One of the common questions in every day inspection work is, How Much Inspection?" The fundamental weakness of MIL-STD-105 and DEF-131 is that they do not answer this question, or

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rather that they give 11 and 7 answers, respectively, and leave the user to make his choice on the basis of his requirements to the O.C. curve. I still therefore hold the view expressed in my paper (1960) that these systems may best be viewed as a convenient way of indexing a collection of O.C. curves and that we first of all need a continuation of the work started by Dodge and Romig by developing sampling schemes corresponding to more detailed specifications of conditions and purposes.

Nevertheless, MIL-STD-105 has proved to be of great practical value and I think that the changes made are wisely chosen so that DEF-131 is a definite improvement. Mr Hill has succeeded very well in giving us a better solution of an unsolvable problem and in particular has taught us how to employ such sampling schemes better than before by stressing the concepts of economic incentiveness and tightened inspection.

Mr HILL replied briefly at the meeting, and subsequently in writing, as follows:

The main criticism of my paper, and of DEF-131, has been the failure to incorporate the economic theory of inspection. In the paper, I wrote, "I remain to be convinced that the problem . . . is really soluble along the lines of the economic optimum." I still do, but this is not the same thing as saying that I am convinced that it is not soluble along these lines, at least in part. Again, to quote the paper, "There is everything to be said in favour of an experiment", and my hope had been to see DEF-131 launched and available for use, and then to persuade some of the Inspectorates to introduce experiments with alternative ways of using it in selected cases. The text of DEF-131 includes the wording "this specification [is not] mandatory unless invoked by the contract or order", and "The Authority reserves the right to specify inspection levels other than those given in Tables I and II" so as to enable such experiments to be made.

What I was not prepared to do was to recommend a leap in the dark and the introduction of untried principles without the chance of experimenting first to discover what difficulties there are and how they may be overcome. Now that I have changed my job, I shall not get the chance to engage in this desirable experimentation, but I very much hope that someone else will do so.

We should always remember Anscombe's (1958) maxim: "What is important is that we realize what the problem really is, and solve that problem as well as we can, instead of inventing a substitute problem that can be solved exactly but is irrelevant." Anscombe was arguing in favour of the economic optimum (and I am not, in quoting his maxim, seeking to associate him with my views) but he was discussing the case of a manufacturer inspecting his own goods, where I agree that the economic optimum does represent the real problem. For the Government Inspectorate the economic optimum is part of the real problem, but it is not the whole problem.

What is wanted is a scheme which makes sense both from the economic optimum point of view and from the economic incentive point of view, which is simple to operate, does not require a knowledge of too many parameters, is in a suitable form to be quoted in specifications, and has been tested by practical experiment. Are we in a position to put forward such a scheme at the moment? I think not, in which case any suggested scheme must be a compromise. I do not claim that DEF-131 is the best compromise for all circumstances, but I believe that it is a good compromise for many circumstances.

Dr Horsnell has asked for safeguards against large batches obtained by grouping several rational sub-groups together. The text of DEF-131 includes "Each batch shall, as far as is practicable, consist of units . . . produced under essentially the same conditions" and "The minimum and maximum batch size . . . may be determined by the Authority". There is also considerable discussion of this point in the forthcoming Defence Guide DG-7 which I hope will meet his requirements.

His point about the process average limit tables is partially valid, but the number of units in the samples from five batches can vary widely when sequential sampling is used. In addition, when a production run first starts, the process average is estimated from fewer

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than five batches until five are available, and more than five are sometimes required in checking whether reduced inspection is permissible. It is better to give the complete table in each case to avoid any possibility of a required value not being present.

The idea of basing the inspection levels on a "minimum average amount of inspection" criterion, as in Dodge and Romig, but assuming the process average to equal the AQL instead of using an observed value of process average, is interesting. This could be done with the existing tables of sample sizes and acceptance numbers, thus giving the consumer as well as the producer some of the advantage when a larger sample size is used, rather than standardizing the consumer's risk. The total amount of inspection at the AQL is not necessarily the best thing to optimize but is likely to be better than an arbitrary scale which does not try to optimize anything. If this procedure is tried for an AQL of 1·0 per cent., the result is found to be quite similar to inspection level I. A difficulty, however, is that this would lead to different inspection levels being recommended for different AQLs and it is often desirable to specify one given inspection level for inspecting a given type of article although different features on that article may have different AQLs. This enables the same physical sample to be used for each feature, whereas if sample sizes were different it might be necessary to draw a different random sample for each feature.

It is a pity that both Dr Horsnell and Dr Moore took inspection level IV for their examples. This level was really designed to allow the largest sample sizes to come into play with the tightest AQLs, thus avoiding the situation where the acceptance number is always zero. Level IV would not normally be used in other circumstances.

Dr Wetherill claims that the form of the prior distribution is not as critical as I imply. Yet Vagholkar's method, which he cites in support, is not satisfied by the sketchy information which Dr Wetherill implies is sufficient. To apply the method we need to know the sum, over all values of proportion defective less than the break-even quality, of the probability of meeting any given quality multiplied by the decision loss if we make the wrong decision at that quality, and a similar sum for all values greater than the break-even quality. Vagholkar then fits a double binomial to keep these sums unchanged. To know that we can do this without upsetting the optimum sampling plan is interesting, but I find it difficult to imagine a situation where I would know these sums without knowing the prior distribution itself.

In Anscombe's 1961 paper it is stated, "it is reasonable to contrast the following two sorts of inspection: (a) Final inspection of a product by the producer . . . (b) Acceptance inspection of a product by a would-be buyer" and "Rectifying inspection in case (a) . . . is the topic of the present paper." Undoubtedly some of his conclusions would apply also to case (b), which is the case for which DEF-131 is designed, but, equally certainly, all of them would not.

In assuming that considerable knowledge of the prior distribution is necessary for a firm economic optimum to be derivable, I rely on Barnard's (1954)† example in which two prior distributions, both double binomials with the same mean and the same variance, had economic optimum sample sizes in the ratio of three to one even though the acceptance number was defined in advance and was the same in both cases.

Another point to be remembered is that however flat an optimum may be it is inevitable that the acceptance number, and the sample size with it, must change in sudden jumps from one integer to another as certain limiting conditions are crossed. This produces the zig-zag type of O.C. curve, as shown in Fig. 4 of Hill (1960), which is so undesirable for the Government Inspectorate, who must be scrupulously fair to all producers.

I agree that economic calculations would be of value in choosing one of the given inspection levels or in designing an alternative level.

I also should like to see more emphasis on the process average, in the form of some cumulative sum technique, but we must be careful not to frighten people away from

† Barnard, G. A. (1954), "Sampling inspection and statistical decisions", *J. R. statist. Soc.* B, **16**, 151–174.

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sampling methods altogether by making things too difficult. The present process average calculations have been criticized on these grounds.

Professor Barnard is right in saying that the ARL has some advantages compared with the O.C. curve. Where both the sampling plan and the quality offered are constant, the two functions show the same thing in a different form. Where either the plan or the quality, or both, vary, neither function can show in a simple form all the things that one might want to know. But I agree that Tables 2 and 3 of the paper would have been improved by being put in ARL form.

I am rather surprised that Professor Barnard should say that "We should always think . . . of producing regularly the best quality we can produce". It is nearly always possible to improve quality by a sufficient increase in cost, and there must be some limit to the extent to which this is worth while. I hope he is not suggesting that an exhortation to "do the best you can" should replace the specification of an AQL, or some alternative parameter, as this would almost certainly lead to a worsening of quality. Producer A might offer 10 per cent. defective, Producer B 1 per cent. defective, Producer C 0·5 per cent. defective, and they might all be obeying the terms of the contract in doing the best they could. It is only when a specification calls for an AQL of 0·25 per cent. defective, say, that Producers A and B decide not to try for a production order at all, and Producer C decides to install new machinery to meet the requirements.

Dr Broadbent wants some flexibility rather than the strict application of a sampling plan. I agree with him that this can be advantageous in particular cases, but I should want to be sure that the right to exercise this flexibility should be given only to someone with intelligence and statistical knowledge. Otherwise the whole inspection exercise might well fall to the ground, and the sampling scheme would later be blamed for faults of which it was not guilty. An alternative might be to write more flexibility into the scheme, thinking out in advance the circumstances in which one might wish to make a change and designing the scheme to allow for them.

Dr Broadbent has misunderstood the first sentence of Section 8.9. What I intended to say was that where single, double and multiple plans were available, the single and double plans would normally be used except when it was necessary to keep the average sample size as small as possible. I was not comparing the use of multiple with sequential where both are available.

The inconvenience of sampling going on for a long time with sequential plans has been overcome in DEF-131 by giving a maximum sample size with appropriate acceptance and rejection rules if, exceptionally, the maximum is reached. I think the main reason why sequential sampling has not been much used has been the absence of a convenient set of tables, and I hope that DEF-131 will lead to a new interest in the method.

I hope that, where Mr Hopkinson has experienced process averages of 0·9 per cent. for 6·5 per cent. AQLs, he has been able to make good use of reduced inspection.

I think that if Dr Moore looks at some of the other inspection levels he will find that the document has sufficient flexibility for many purposes, but it sounds as though some of the cases he mentions would be satisfied better by a variables, rather than an attributes, scheme.

Dr Crawford questions the need for the extended tables. An opposing viewpoint was put forward when a draft of DEF-131 was tried out on an inspector before a final decision was reached on its form; he questioned the need for the summary tables. I am not sure just where the optimum does lie between too much bulk on the one hand, and too much condensation of the tables, at the expense of mistakes in their use, on the other, but I am disappointed that the price of DEF-131 has turned out to be so high.

I am grateful for Professor Hald's kind remarks, particularly the one about "a better solution of an unsolvable problem". I am interested in his work on the Dodge and Romig tables; he seems to be uncovering some attractive properties. However, in accordance with his view that "these systems may best be viewed as a convenient way of indexing" he suggests using the Dodge and Romig plans in quite different ways. This is where I have

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tried to differentiate between sampling plans and sampling schemes. I accept that the plans given in the Dodge and Romig tables can be used in alternative ways with advantage, but this is not the Dodge and Romig scheme and it is the scheme rather than the plans which I have claimed has disadvantages for a Government Inspectorate.

I should like to end my reply by quoting from Defence Guide DG-7 (which I hope will be published before too long):

"The sampling plans given in DEF-131 are, of course, only a selection from all possible plans. . . . Any such selection is bound to be a compromise to some extent, and there are occasions when it is not desirable to compromise.

"If, in a particular situation, information is available of the true costs of mistakenly rejecting good articles and accepting bad ones, and if from long experience it is known just how often batches of any given quality can be expected to be presented, this may well be one of the occasions when compromise is not desirable, since it should be possible to calculate a more efficient scheme on the basis of the economic information available . . . It should not be thought that the existence of DEF-131 rules out the use of such alternative methods when applicable and desirable."

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On the Analysis of Interaction in Multi-dimensional Contingency Tables

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Summary

This paper is mainly concerned to meet the needs of researchers who are faced with the practical problem of interpreting multi-way contingency tables and are unable to obtain guidance from standard text books. It presents a general review of the more important methods of analysis, together with a selection of procedures which are computationally the simplest available, and which may be adapted for use with different sampling schemes and/or with theoretical rather than estimated parameters. Since there is no limit to the number of hypotheses that might be proposed in respect of multi-way tables, there can clearly be no claim of completeness, but it is believed that there are no major omissions.

1. Introduction

It is now over 25 years since Bartlett (1935) first commented on the lack of interest shown in contingency tables of three or more dimensions. Since then several important papers have appeared on the subject, but there is still no coordinated information available, and the treatment of these tables is still widely neglected in standard text books. For instance, the Siegel (1956) collection contains only one footnote reference to multi-dimensional tables, and this is to a method due to Wilson (1956) which was subsequently discredited by McNemar (1957, 1958).

Mood (1950) is rather exceptional in recognizing the potential usefulness of such tables. His book contains several illustrations of the way in which multi-dimensional tables can help to throw light on suspected relationships, and he also outlines two methods of analysis. Of the 30–40 text books in current use, Anderson and Bancroft (1952) give a brief account of Bartlett's original test for tables of size $2\times2\times2$, and Snedecor (1956) considers a few examples that can be handled by analysis of variance techniques. The remaining texts either make no mention of multi-dimensional tables or fail to acknowledge that they present any special difficulties. For instance, Davies (1954), after discussing the standard types of two-way contingency table, raises and summarily dismisses the three-dimensional case as follows: "Such examples may be treated by an extension of the methods already explained which the reader will have no difficulty in making if he has understood the principles."

Ostle (1954) appears to hold a similar opinion, and yet it will become apparent in the following sections that even the minimal extension (from tables of size 2×2 to those of size $2\times2\times2$) poses entirely new conceptual problems. On the other hand, there are no new problems involved in making the extension from tables of three dimensions to those of four or more dimensions.

In the present paper no attempt is made to discuss the mathematical theory of multi-dimensional contingency tables, but such discussions can be found in many of the references cited. Anyone interested in the canonical analysis of multi-way tables

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should, for instance, refer to Lancaster (1960), who treats the three-way case in some detail, as an extension and generalization of his earlier work on two-way tables. As a bonus, Lancaster includes a good historical bibliography of theoretical references. To these should be added the recent exposition in volume 2 of Kendall and Stuart (1961).

2. NOTATION

In the general case of the three-dimensional $r \times c \times l$ contingency table with r row, c column, and l "layer" categories, let n_{ijk} denote the observed frequency in the cell of the ith row, jth column and kth layer. Similarly let p_{ijk} denote the probability of an observation falling in the (ijk)th cell.

If the n_{ijk} frequencies are summed over all values of i (from 1 to r), the result will clearly be the "marginal" total of the jth column in the kth layer. This marginal total is accordingly designated $n_{.jk}$, so that

$$\sum_{i=1}^{i=r} n_{ijk} = n_{.jk} \quad \text{by definition.}$$

Similarly $$\sum_{j=1}^{j=c} n_{ijk} = n_{i.k} \quad \text{and} \quad \sum_{k=1}^{k=l} n_{ijk} = n_{ij.}$$

If the n_{ijk} frequencies are summed over all values of both i and j it is equally apparent that they will yield the total of the whole kth layer. This total is designated $n_{..k}$ with $n_{i..}$ and $n_{.j.}$ defined in like manner.

Finally, if the n_{ijk} frequencies are summed over all values of i, j, and k, the result will be the grand total of the contingency table. This is sometimes designated $n_{...}$ but is more conveniently written as N.

A similar notation is used for the p's (except that $p_{...} = 1$), and the entire system of notation is easily generalized to contingency tables involving more than 3 ways of classification.

3. TESTS OF MUTUAL INDEPENDENCE

3.1. *Random Sampling throughout Table*

The first example is taken from a market research enquiry in which 270 persons were classified according to their ability (or inability) to recall 3 major features

TABLE 1

Classification of 270 *persons according to success or failure in recalling* 3 *attributes*

	C_1		C_2	
	B_1	B_2	B_1	B_2
A_1	44(n_{111})	35(n_{121})	24(n_{112})	19(n_{122})
A_2	47(n_{211})	41(n_{221})	28(n_{212})	32(n_{222})
$n_{.jk}$	91($n_{.11}$)	76($n_{.21}$)	52($n_{.12}$)	51($n_{.22}$)
$n_{..k}$	167($n_{..1}$)		103($n_{..2}$)	

A, B, and C of a commercial advertisement. A $2 \times 2 \times 2$ summary of the data is shown in Table 1, where A_1, B_1, and C_1 denote successful recall (of features A, B, and C), whilst A_2, B_2, and C_2 denote failure to recall. The notation described in Section 2 has

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also been inserted in brackets, the A, B, and C classifications representing the rows, columns and layers, respectively.

If $p_{1..}$, $p_{.1.}$, and $p_{..1}$ are the probabilities of recalling A, B, and C respectively, it is of interest to enquire whether the corresponding events are independent, or whether, e.g., the recall of one feature appears to facilitate (or inhibit) the recall of others. In the (most usual) case where the probabilities are estimated from the data:

$$\hat{p}_{1..} = n_{1..}/N = (44+35+24+19)/270 = 122/270,$$

$$\hat{p}_{.1.} = n_{.1.}/N = (44+47+24+28)/270 = 143/270,$$

$$\hat{p}_{..1} = n_{..1}/N = 167/270,$$

so that the corresponding estimated probabilities for failure to recall are

$$\hat{p}_{2..} = 1-\hat{p}_{1..} = 148/270, \quad \hat{p}_{.2.} = 127/270, \quad \text{and} \quad \hat{p}_{..2} = 103/270.$$

When the 3 recall probabilities are independent, the expected frequency for each cell may be computed directly by multiplication. Thus the expected frequency for the (122) cell $= N\hat{p}_{122}$

$$= N(\hat{p}_{1..})(\hat{p}_{.2.})(\hat{p}_{..2}) = 270(122/270)(127/270)(103/270)$$

$$= 21{\cdot}8194 \text{ as compared with the observed value of } 19.$$

The test for complete mutual independence is now achieved by setting up the null hypothesis:

$$H_0 : p_{ijk} = p_{i..}p_{.j.}p_{..k}$$

and testing (against the general alternative $H_A \neq H_0$) by the formula:

$$\chi^2 = \sum_{ijk}\left(n_{ijk} - \frac{n_{i..}n_{.j.}n_{..k}}{N^2}\right)^2 \Bigg/ \left\{\frac{n_{i..}n_{.j.}n_{..k}}{N^2}\right\}$$

$$= \Sigma(\text{Observed} - \text{Expected})^2/\text{Expected}$$

$$= \{\Sigma(\text{Observed})^2/\text{Expected}\} - N = 2{\cdot}194.$$

This value (with 4 degrees of freedom) is not significant.

Note that there are 4 d.f. since N is fixed in advance, and 3 parameters $p_{1..}$, $p_{.1.}$, and $p_{..1}$ are estimated from the data. The above procedure may be readily extended to more complicated cases. In the 2^k case there would be (2^k-k-1) d.f. and in the three-way $r \times c \times l$ case there would be $(rcl-r-c-l+2)$ d.f.

3.2. *Further Tests* (*Homogeneity of Proportions*)

The non-significant result obtained on Table 1 was somewhat unexpected. Prior to the analysis, it was supposed that the recall of any one feature would "facilitate" the recall of others—in which case a considerable amount of interaction might have been found. As it happens, the data in Table 1 could be fitted to an empirically testable stochastic model, in which the probability of recalling a particular feature at time t_i is a function of the time that has elapsed since the recall of another feature. In this case somewhat different conclusions might have been drawn.

If the result of the χ^2 test is accepted at its face value, however, then the hypothesis of complete mutual independence must be accepted, and it then becomes interesting to test whether some features are more likely to be recalled than others, i.e. to test

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whether $p_{1..}, p_{.1.}$, and $p_{..1}$ are significantly different. In this case it must be remembered that each subject is classified on all 3 features A, B, and C, so it is necessary to use the Cochran (1950) Q-test for the comparison of correlated proportions.

The method is to reset the Table 1 data as in Table 2, and to compute

$$Q = \frac{(c-1)\{c\Sigma T^2 - (\Sigma T)^2\}}{c\Sigma fr - \Sigma fr^2}$$

where 1 0 1 denotes the category of persons recalling A and C but not B, etc.,

f = number of persons per category ($= n_{ijk}$),

r = number of recalls per category,

c = number of columns,

T = column "recall" totals such that, e.g., $T_A = (44+35+24+19) = 122 = n_{1.}$

TABLE 2

Resetting of Table 1

(*Category*)						
A_1	B_1	C_1	f	r	fr	fr^2
1	1	1	44	3	132	396
0	1	1	47	2	94	188
1	0	1	35	2	70	140
0	0	1	41	1	41	41
1	1	0	24	2	48	96
0	1	0	28	1	28	28
1	0	0	19	1	19	19
0	0	0	32	0	—	—
122	143	167	270		432	908
(T_A)	(T_B)	(T_C)	(N)		(Σfr)	(Σfr^2)

In the table above:

$$Q = 2[(122^2+143^2+167^2)\,3-432^2]/(3\times 432-908)$$
$$= 6084/388 = 15{\cdot}68$$

where Q follows the chi-square distribution with $(c-1)$ d.f.

With $(c-1)=2$ d.f. this value exceeds the ·1 per cent. level of significance ($p<{\cdot}001$) so it may be concluded that the 3 proportions (of persons recalling A, B, and C) are not homogeneous.

Given a significant Q-value, it is in order to carry out a more detailed comparison of *pairs* of proportions. For this purpose Cochran's formula could again be used, but for differences between just two proportions his formula reduces (after correction for continuity) to:

$$\chi^2 = (|n_{12.}-n_{21.}|-1)^2/(n_{12.}+n_{21.}).$$

This formula is equivalent to one proposed by McNemar (1947) and has one degree of freedom. It is also the limiting case of Bowker's (1948) test of symmetry. Its application to the Table 1 proportions would show that the high Q-value derives

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most of its significance from the excessive difference between $p_{1..}$ and $p_{..1}$. $p_{1..}$ and $p_{.1.}$ do not differ significantly ($P = \cdot 08$).

For a discussion of the rationale and limitations of the Q-test, the reader should refer to Cochran's original (1950) article. For an alternative account of the computational procedure, Siegel (1956) is satisfactory.

3.3. *Random Sampling within each* $(r \times c)$ *Section*

In Table 1 the final form of the 8-cell distribution of frequencies was left wholly to chance, since only the grand total N of the table was specified in advance by the sampling procedure. This is not always the case. Sometimes (e.g. when random sampling might produce disproportionately low frequencies in some sections of the $r \times c \times l$ table) the experimenter might decide to specify not only the sample size N, but also the size of the sub-totals $n_{..k}$ of each of the $(r \times c)$ layers. If the Table 1 sub-totals $n_{..1} = 167$ and $n_{..2} = 103$ had been fixed in this way, the distribution of frequencies would have been unconstrained within each 2×2 layer, but not within the $2 \times 2 \times 2$ table as a whole. Furthermore, since $p_{..k} = n_{..k}/N$ (when estimated from the border totals), the $p_{..k}$ values would not have been any different, but they would have been determined *before* the sample had been taken, and it would be necessary to take account of this fact by expressing the null hypothesis in the form:

$$H_0 : p_{ijk} = p_{i..}p_{.j.}n_{..k}/N.$$

This is the only modification, since the chi-square computation and the number of degrees of freedom remain unchanged. The power functions are, however, not the same for both tests. Also, if a non-significant result is obtained it should be remembered that the $p_{..k}$ values are arbitrarily fixed by the experimenter, so it would normally be of interest to examine differences between $p_{i..}$ and $p_{.j.}$ only.

Some writers (Kullback, 1959; Mood, 1950; Roy, 1957) would modify the null hypothesis differently from the manner indicated above. For if the $n_{..k}$ totals are fixed in advance, it might be argued that there are in effect l distinct tables of size $r \times c$. In these circumstances p_{ijk} denotes the probability that an observation falls in the (ij)th cell of the kth two-way table. Moreover, if each two-way table is considered separately:

$$p_{ijk} = n_{ijk}/n_{..k} \quad (\text{not } n_{ijk}/N)$$

and

$$p_{..k} = 1 \quad (\text{not } n_{..k}/N).$$

Hence the hypothesis:

$$H_0 : p_{ijk} = p_{i..}p_{.j.}p_{..k} \quad \text{would be modified to}$$

$$H_0 : p_{ijk} = p_{i..}p_{.j.} \quad \text{for all } k = 1, 2, \ldots, l.$$

There are theoretical grounds for preferring this type of modification, but it requires careful handling and is likely to mislead anyone who is not thoroughly familiar with it.

3.4. *Random Sampling within each Column*

Instead of specifying the size of the totals $n_{..k}$, the sampling procedure sometimes specifies the size of the individual column totals $n_{jk.}$ within each $r \times c$ layer. (This logically entails the fixing of $n_{.j.}$ and $n_{..k}$.) In Table 1 there are 4 such totals, and if these are fixed in advance (an unrealistic possibility in this particular case) then variation is still possible within each of the 4 pairs 44/47, 35/41, 24/28, and 19/32, but

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the distribution of frequencies is too heavily restricted to enable a test of complete mutual independence to be meaningfully carried out. However, there are other tests of interest. For instance, it might be desired to test whether the 4 pairs of frequencies divide in the same ratio as the marginal frequencies $n_{1..}/n_{2..}$. Alternatively the 4 frequency ratios could be tested for "conditional" homogeneity within each 2×2 layer or within each 2×2 column. The respective null hypotheses are:

$$H_0 : p_{ijk} = p_{i..}n_{.jk}/N,$$
$$H_0 : p_{ijk} = p_{i.k}n_{.jk}/n_{..k},$$
$$H_0 : p_{ijk} = p_{ij.}n_{.jk}/n_{.j.},$$

all of which are considered in subsequent sections (see also Section 10.2).

3.5. *Alternative Sampling Constraints*

In addition to the types of sampling constraint already discussed, many other kinds can be envisaged. For instance, it is possible to fix the $n_{i..}$ as well as the $n_{.jk}$ totals, or to fix all or some of the totals $n_{i..}$, $n_{.j.}$, and $n_{..k}$. Since such restrictions are likely to be rare in practice, they are not discussed here. In any event it will nearly always turn out (as in Section 3.3) that the chi-square computation is the same whether the marginal totals are fixed or not. In such cases it will be only the power function and the form of the hypothesis that vary.

4. Tests of Partial Independence

4.1. *One Classification Independent of All Others*

If the chi-square test for mutual independence gives a *significant* result, it should not be assumed that all 3 classifications interact. It might be the case that just two of the classifications interact, and that the third is completely independent. This gives rise to 3 easily testable hypotheses, since any of the 3 classifications could be the independent one.

To test whether the row classification of Table 1 is independent of the others, it is only necessary to see whether the 4 ratios 44/47, 35/41, 24/28, and 19/32 may be regarded as chance deviations from the over-all marginal ratio 122/148 ($=n_{1..}/n_{2..}$). Thus, if the 91 cases in the left-hand column had divided in the ratio 122/148 the frequencies n_{111} and n_{211} would have been 41·1185 and 49·8815 respectively (i.e. $91\times 122/270$ and $91\times 148/270$).

Similar computations can be made in respect of the other 3 pairs of cells, and the null hypothesis for this test is:

$$H_0 : p_{ijk} = p_{i..}p_{.jk}$$

so that

$$\chi^2 = \sum_{ijk}\left(n_{ijk}-\frac{n_{i..}n_{.jk}}{N}\right)^2 \Big/ \frac{n_{i..}n_{.jk}}{N}$$
$$= \{\Sigma\,(\text{Observed})^2/\text{Expected}\} - N = 1{\cdot}706$$

but with only 3 d.f. on this occasion, since there are 3 pairs of frequencies that are free to vary, and just 1 d.f. within each pair. In the general $r\times c\times l$ case, the test of independence of the row (r) classification would have $(cl-1)(r-1)$ degrees of freedom, and similar extensions can be made for higher-order tables. For instance, the test for complete independence of any *one* classification in the 2^k case would have $2^{k-1}-1$ degrees of freedom.

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As expected, the chi-square value of 1·706 is not significant, so the null hypothesis (that there is no interaction between the row classification and the other two) is accepted. It should be noted that this test has been applied to the Table 1 data for purposes of illustration only. Since it has already been accepted that the 3 classifications are mutually independent, it is unnecessary (and may also be improper) to test each classification individually.

Since the $p_{.jk}$ values are estimated from the border totals $n_{.jk}$, the test proceeds as if the $n_{.jk}$ totals were fixed in advance. The chi-square computation is therefore not in any way changed if the $n_{.jk}$ totals are fixed (see Section 3.4), but the power function of the test will have a different structure, and the null hypothesis should be expressed in the form:

$$H_0 : p_{ijk} = p_{i..} n_{.jk}/N$$

to indicate that only $p_{i..}$ are left undetermined by the sampling procedure.

Finally, it must be noted that if one classification is completely independent, several important implications follow. Thus:

	$p_{ijk} = p_{i..}p_{.jk}$	implies
	$p_{ij.} = p_{i..}p_{.j.}$	(by summing over k)
and	$p_{i.k} = p_{i..}p_{..k}$	(by summing over j)
and	$p_{ijk} = p_{i.k}p_{.jk}/p_{..k}$	(by appropriate substitution)
and	$p_{ijk} = p_{ij.}p_{.jk}/p_{.j.}$,	

all of which are considered in subsequent sections.

4.2. *One Classification Independent of One Other*

If the row classification is completely independent of the other two, it follows that the row × column interaction is zero, and that the row × layer interaction is also zero. This raises the general possibility that in any multi-way table, certain classifications might be independent of some, *but not all*, of the others.

To test for zero row × column interaction, the irrelevant "layer" classification is eliminated by summation (see Section 9) in order to give a straightforward two-way table of size $r \times c$, which has cell frequencies of $n_{ij.}$ and marginal frequencies of $n_{i..}$ (for rows) and $n_{.j.}$ (for columns). The null hypothesis of zero row × column interaction is therefore:

$$H_0 : p_{ij.} = p_{i..}p_{.j.}$$

which is tested in the normal way (for two-way tables) and has $(r-1)(c-1)$ d.f.

Similarly, the null hypothesis of zero row × layer interaction is:

$$H_0 : p_{i.k} = p_{i..}p_{..k} \quad \text{with } (r-1)(l-1) \text{ d.f.}$$

For the Table 1 data it will be found that $\chi^2 = \cdot 688$ for row × column interaction, and $\chi^2 = \cdot 795$ for row × layer interaction. With 1 d.f. in each case, these values are non-significant and entirely consistent with the already established finding that the row classification is independent.

Although the hypothesis of complete independence of the row classification ($p_{ijk} = p_{i..}p_{.jk}$) implies the composite hypothesis

$$p_{ij.} = p_{i..}p_{.j.} \quad \text{and} \quad p_{i.k} = p_{i..}p_{..k},$$

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this composite hypothesis does not imply the former. The reason for this is that the former hypothesis *also* implies that there is no conjoint row × column × layer interaction. This fact has accordingly been exploited by Roy *et al.* (1956) to provide a generalized condition of the absence of "3-factor" interaction (see Section 11.2).

5. Tests of Conditional Independence

In some three-way tables it is of interest to test the hypothesis that given, for example, any layer, the row and column classifications are independent.

This hypothesis of conditional independence is particularly relevant if the layer totals $n_{..k}$ are fixed in advance, since it is likely that attention would then be mainly directed towards the $r \times c$ interaction within each layer. In fact the fixing of the $n_{..k}$ totals usually implies that there are l independent two-way tables of size $r \times c$, so the chi-squares of these l tables may be summed to provide an over-all test of conditional independence.

For the $r \times c \times l$ case in which there is random sampling throughout, the null hypothesis is:

$$H_0 : p_{ijk} = p_{i.k}p_{.jk}/p_{..k}.$$

If the $n_{..k}$ totals are fixed, the null hypothesis is amended by substituting $n_{..k}/N$ for $p_{..k}$, and the power function of the test is different. However, the chi-square computation remains the same and is:

$$\chi^2 = \sum_{ijk} (n_{ijk} - n_{i.k}n_{.jk}/n_{..k})^2/(n_{i.k}n_{.jk}/n_{..k})$$

with $l(r-1)(c-1)$ degrees of freedom. (It is worth noting here that this pooled χ^2 is insensitive in that no account is taken of similarities of pattern in the l separate $r \times c$ tables.)

Since the above hypothesis involves $p_{i.k}$ and $p_{.jk}$ it is relevant to point out that

if $p_{i.k} = p_{i..}p_{..k}$ (i.e. there is no $r \times l$ interaction)

or if $p_{.jk} = p_{.j.}p_{..k}$ (i.e. there is no $c \times l$ interaction)

then the original hypothesis becomes:

$p_{ijk} = p_{i..}p_{.jk}$ (complete independence of rows)

or $p_{ijk} = p_{.j.}p_{i.k}$ (complete independence of columns),

both of which imply:

$p_{ij.} = p_{i..}p_{.j.}$ (absence of $r \times c$ interaction).

It is therefore apparent that if the row and column classifications are independent for each layer, it does not follow that there is over-all independence between rows and columns, for the original hypothesis

$p_{ijk} = p_{i.k}p_{.jk}/p_{..k}$ does not, by itself, imply

$p_{ij.} = p_{i..}p_{.j.}$

Hence some extra condition, such as the absence of $r \times l$ or $c \times l$ interaction, must be superimposed before the row and column classifications become *both conditionally and unconditionally* independent. If *both* these extra conditions are simultaneously

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imposed, then the original hypothesis becomes (after substituting $p_{i.k} = p_{i..}p_{..k}$ and $p_{.jk} = p_{.j.}p_{..k}$):

$$H_0 : p_{ijk} = p_{i..}p_{.j.}p_{..k}$$

which is the condition of complete mutual independence.

It is now apparent that the hypothesis of complete independence can be exactly broken down into 3 component hypotheses which are mutually exclusive in logic, and which are concerned with (a) conditional independence of rows and columns, (b) absence of $r \times l$ interaction, and (c) absence of $c \times l$ interaction. Of course, the roles of r, c, and l can be permuted here. If the chi-squares associated with these three component hypotheses are designated χ_1^2, χ_2^2, and χ_3^2 respectively, and if the chi-square for complete independence is designated χ^2, it is therefore reasonable to ask whether:

$$\chi_1^2 + \chi_2^2 + \chi_3^2 = \chi^2.$$

A sketchy and incomplete answer to this enquiry has been provided by Roy (1957) who states that, unlike what happens in standard analysis of variance procedures, there is *no additivity in the usual algebraic sense*, but additivity does occur in probability and asymptotically as $N \to \infty$.

For the case being discussed, Mitra (1955) has shown (according to Roy) that under the total null hypothesis

$$\chi_1^2 + \chi_2^2 + \chi_3^2 \to \chi^2 \quad \text{in probability as} \quad N \to \infty.$$

If the arithmetic is followed through for the Table 1 data, it will be found that

$$\chi_1^2 = \cdot 926 \quad \text{with 2 d.f.},$$
$$\chi_2^2 = \cdot 795 \quad \text{with 1 d.f.},$$
$$\chi_3^2 = \cdot 410 \quad \text{with 1 d.f.},$$

so that $\chi_1^2 + \chi_2^2 + \chi_3^2 = 2 \cdot 131$ (with 4 d.f.) as compared with the value $\chi^2 = 2 \cdot 194$ (with 4 d.f.) already computed in Section 3.1. For this particular case, in which $N = 270$, the degree of approximation is fairly good but is not necessarily typical, since it is not clear how rapidly the approximation improves as N increases. As there are several other situations in which hypotheses can be broken down into component hypotheses (that do not yield additive chi-squares) it is apparently necessary to reconsider the importance of additivity in this type of problem. This point is particularly relevant for the discussion in Sections 10, 11, and 14.

6. Homogeneity of Two-way Tables

With suitable hypotheses and restrictions an $r \times c \times l$ table may be thought of as being a set of r two-way tables of size $c \times l$. Conversely, r independent samples of a $c \times l$ table may be treated as an $r \times c \times l$ three-way table. In such cases it is often interesting to test whether the $c \times l$ tables are homogeneous. (This is especially so if the $n_{i..}$ total of each $c \times l$ table is fixed in advance.)

The two-way tables will be homogeneous if the probabilities associated with corresponding cells are homogeneous, i.e. if

$$p_{1jk}/p_{1..} = p_{2jk}/p_{2..} = \ldots = p_{rjk}/p_{r..} = p_{.jk}.$$

The null hypothesis (for random sampling throughout) is therefore:

$$H_0 : p_{ijk} = p_{i..}p_{.jk}$$

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which is precisely the same as the hypothesis for the complete independence of the row classification (see Section 4.1).

Since $p_{ijk} = p_{i..}p_{.jk}$ implies (by summing over k)

$p_{ij.} = p_{i..}p_{.j.}$ and also implies

$p_{ijk} = p_{ij.}p_{.jk}/p_{.j.}$ (see Section 4.1),

and since the latter two equalities imply the former, it follows that the $(c \times l)$ tables are homogeneous if and only if:

(a) the row and column classifications are independent ($p_{ij.} = p_{i..}p_{.j.}$),

(b) the row and layer classifications are independent given the column classification ($p_{ijk} = p_{ij.}p_{.jk}/p_{.j.}$).

As in Section 5, the chi-squares associated with the two component hypotheses do not sum to the chi-square for the original hypothesis, if the usual "expected frequency" method is used. However, an *exactly* additive analysis can be obtained in this case and many others, by means of the information theory approach of Kullback (1959). The superiority of information theory, in this respect, derives from the fact that additivity of information (for independent events) is postulated as a requisite property in almost all axiomatic developments of the theory.

When there are r independent samples of size $c \times l$ (i.e. $n_{i..}$ are fixed), Kullback uses the convention mentioned in Section 3.3, and expresses the null hypothesis in the form:

$$H_0 : p_{ijk} = p_{.jk}.$$

This hypothesis is then shown to imply (and be implied by):

$$p_{ij.} = p_{.j.} \quad \text{and} \quad p_{ijk} = p_{ij.}p_{.jk}/p_{.j.}$$

Hypotheses concerning the homogeneity of the $c \times l$ tables can be expressed in a variety of ways, some of which turn out to be equivalent. These alternative formulations are apt to be confusing, but most of them are carefully followed through by Kullback (1959).

7. Independence in Higher-Order Tables

The tests of independence, described in previous sections, can easily be generalized to cover cases of interest in tables having more than 3 ways of classification. In a 5-way table, for instance, there might be no completely independent classification, but one pair of classifications (which interact with each other) might nevertheless be independent of the remaining three (which also interact mutually). The null hypothesis

$$H_0 : p_{ghijk} = p_{gh...}p_{..ijk}$$

would represent such a case, and would yield a set of expected frequencies to which the chi-square test could be applied as before.

Similarly, it is possible to envisage more elaborate forms of homogeneity or conditional independence, but once again these do not involve any new conceptual problems and do not therefore merit special consideration.

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8. The Use of Theoretical Parameters

In all the analyses so far considered, a set of expected frequencies has been obtained by estimating certain parameters such as $p_{i..}$ or $p_{.jk}$, etc. from the data. This procedure is needed only if the relevant parameters cannot be theoretically specified. Thus if there had been grounds in the Table 1 example for expecting subjects to have, e.g., a 50/50 chance of recalling (or not recalling) attributes A, B, and C, the correct procedure would be to assign the value $\frac{1}{2}$ to each of the parameters $p_{i..}$, $p_{.j.}$, and $p_{..k}$. This would have increased the number of degrees of freedom by 3 (since $p_{i..}$, etc. no longer have to be estimated from the data); it would also have yielded a different set of expected frequencies, and a much larger value of chi-square—which would have reflected not only the lack of independence between the 3 classifications, but also the extent to which the observed values of $p_{i..}$, etc. differ from the theoretical values of a $\frac{1}{2}$.

9. Grounds for reducing Multi-way Tables in Dimension

9.1. *Minimum Criteria*

If a classification is found to be completely independent of the others, there is normally no point in retaining it, unless it is proposed to introduce still more cross-classifications. The table may therefore be reduced by one dimension by combining appropriate sets of cell frequencies. In the $r \times c \times l$ table there are r component tables of size $(c \times l)$ to be added together to eliminate the row classification, but it is not strictly in order to carry out this addition unless each of the $(c \times l)$ tables shows identical interaction. (In actual practice it is unlikely that the interactions would ever be identical, so it is usually assumed—without proof, as Brownlee (1955) points out—that reduction is permissible if the interactions are not significantly different.)

In a $2 \times 2 \times 2$ table, for instance, the simplest measure of interaction in a 2×2 (component) section having cell frequencies $\frac{a \mid c}{b \mid d}$ is the cross-product ratio $(ad)/(bc)$ which equals unity when interaction is zero. If this measure (or some simple function of it) is adopted, it is clear that the 2×2 section $\frac{a \mid c}{b \mid d}$ could be added to another of the form $\frac{ta \mid uc}{tb \mid ud}$ since:

$$\frac{a.d}{b.c} = \frac{ta.ud}{tb.uc} = \frac{(1+t)\,a.(1+u)\,d}{(1+t)\,b.(1+u)\,c} \quad \text{for any } t \text{ or } u,$$

so the interactions in the two 2×2 sections are identically preserved in the combined table.

If $t = u$ in this example, the cells of one 2×2 section are isomorphically in proportion $(1 : u)$ to the cells of the other 2×2 section. For this special case the eliminated classification is completely independent of the other two, i.e. the 2×2 sections are homogeneous. However, the reduction from three dimensions to two is equally valid if $t \neq u$, so it follows that *complete* independence (of the eliminated classification) is not a necessary condition of reduction. As indicated above, the only thing that matters is that the reduced table should accurately preserve the interactions in the component tables prior to reduction. For the $2 \times 2 \times 2$ case, a precise statement of the conditions of reduction is given by Simpson (1951). For more complicated cases (especially those involving more than three ways of classification) no precise formulation is available, so the analyst can only maintain a general watch to ensure

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that a particular reduction does not seriously distort the inter-relationships of the remaining classifications.

Sometimes, for the sake of clarity, it becomes imperative to reduce a complex table. In such cases, distortion may often be minimized by adopting procedures (e.g. of using percentages, ratios, mean frequencies, etc.) of the kind outlined by Zeisl (1958).

9.2. *Additional Criteria*

In the previous section it has been suggested that the reduction of a $2\times2\times2$ table is permissible if it does not entail (significant) distortion of the component interaction terms. Some writers (e.g. Kullback, 1959) would not be satisfied with this condition, and would make the stronger demand that the component 2×2 sections should not be combined unless they are homogeneous.

As indicated in Section 6, the 2 two-way sections are homogeneous if and only if the eliminated classification is completely independent of the rest. In terms of the Section 9.1 example, this means that reduction should not be allowed unless $t = u$.

The point of making the stronger demand is that if $t = u$ the reduced table not only preserves interaction, but *also* preserves the values of the marginal probabilities of each 2×2 section. Thus:

$$\frac{(a+c)}{(a+b+c+d)} = \frac{u(a+c)}{u(a+b+c+d)} = \frac{(1+u)(a+c)}{(1+u)(a+b+c+d)}, \quad \text{etc.}$$

In these circumstances, reduction cannot conceal the fact that the marginal probabilities might differ significantly for different categories of the eliminated classification. For investigators interested in comparing interactions only, the criteria of Simpson, etc. in the previous section may be regarded as adequate. It is also pertinent to remark that the "strong" condition of homogeneity can be more readily generalized to cover tables of size greater than $2\times2\times2$.

10. LANCASTER'S PARTITION OF CHI-SQUARES

10.1. *Outline of Procedure*

This paper has so far been mainly concerned with some important tests of independence and the bearing that they have on the (often urgent) problem of reducing a multi-way table to more manageable dimensions. If no independent classification(s) can be found, it might be desired to probe the contingency table interactions more closely, and Lancaster (1951) has provided an extremely straightforward method of doing this.

In a manner analogous to that of analysis of variance, a total chi-square value is computed to provide a gross measure of the extent to which the cell frequencies depart from expectation. This value is then partitioned into additive components (each of which is associated with a proportion of the total degrees of freedom) to show how much is attributable to individual classifications (e.g. rows, columns, layers), and how much is due to "first-order" interaction (e.g. rows × columns, columns × layers, layers × rows), and how much to "second-order" interaction (e.g. rows × columns × layers). For contingency tables involving more than 3 ways of classification higher-order interactions arise, and Lancaster's procedure can be extended to cover such cases. Since they are likely to occur infrequently in practice, and involve no new points of principle, the technique will be demonstrated for only the more common three-way case.

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A special merit of Lancaster's method is that it can be used either with theoretical parameters or with parameters estimated from the data (see Section 8). It is also applicable to the type of case discussed in Section 3.4 for which sub-totals $n_{.jk}$, etc. are fixed in advance.

In the Table 1 example, for instance, if there were theoretical grounds for expecting the probabilities $p_{i..}$, $p_{.j.}$, and $p_{..k}$ to be independent and to each take the value $\frac{1}{2}$ (see Section 8), then each of the 8 cells would be expected to contain the same number (i.e. 270/8 = 33·75) of observations. The extent to which the observed frequencies depart from this expectation could be computed in the normal way by:

$$\chi^2 = \Sigma(\text{Observed} - \text{Expected})^2/\text{Expected}$$
$$= \{\Sigma(\text{Observed})^2/\text{Expected}\} - N = 20{\cdot}252.$$

This chi-square value is now partitioned "piecemeal".

First, the actual row totals $n_{1..} = 122$ and $n_{2..} = 148$ are compared with the expected row totals of $Np_{1..} = 135$ and $Np_{2..} = 135$. The usual chi-square test gives:

$$\chi_r^2 = (122-135)^2/135 + (148-135)^2/135 = 2{\cdot}504.$$

Similarly $\chi_c^2 = {\cdot}948$

and $\chi_l^2 = 15{\cdot}170,$

where the subscripts r, c, and l show that the chi-squares are in respect of row, column, and layer comparisons.

If the $2 \times 2 \times 2$ table is now reduced by eliminating the row classification, a four-fold $(c \times l)$ table is formed with cell frequencies $n_{.jk}$ of 91, 76, 52, and 51 (see Table 1). The corresponding *expected* frequencies in this reduced table are $Np_{.j.}p_{..k}$ which equals 270/4 or 67·5 per cell in this particular example. The first-order $(c \times l)$ interaction component is designated χ_{cl}^2 and is obtained by means of the formula:

$$\chi_{cl}^2 = \Sigma(\text{Observed} - \text{Expected})^2/\text{Expected} - \chi_c^2 - \chi_l^2$$
$$= \Sigma(\text{Observed})^2/\text{Expected} - N - \chi_c^2 - \chi_l^2$$
$$= (91^2 + 76^2 + 52^2 + 51^2)/67{\cdot}5 - 270 - {\cdot}948 - 15{\cdot}170$$
$$= {\cdot}726.$$

Similarly $\chi_{lr}^2 = {\cdot}237$

and $\chi_{rc}^2 = {\cdot}533.$

Finally, the "second-order" interaction χ_{rcl}^2 is determined by subtracting all the above component chi-squares from the total chi-square value of 20·252, as in Table 3.

Instead of using theoretical parameters, it might be desired to estimate these from the data, as in Section 3.1 where $\hat{p}_{i..} = n_{i..}/N$, etc. In this case χ_r^2, χ_c^2, and χ_l^2 all become zero, and the first-order interaction terms such as χ_{cl}^2 become the chi-squares of the appropriately reduced four-fold table computed in the usual manner, e.g. by the formula $\chi^2 = (ad-bc)^2 N/[(a+b)(c+d)(a+c)(b+d)]$. Thus

$$\chi_{cl}^2 = (91 \times 51 - 52 \times 76)^2 270/[(167)(103)(143)(127)]$$
$$= {\cdot}410.$$

Similarly $\chi_{lr}^2 = {\cdot}795$

and $\chi_{rc}^2 = {\cdot}688.$

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Since estimated parameters are being used in this particular case, the total chi-square value is the one already computed in Section 3.1 and equals 2·194. Once again χ^2_{rcl} is obtained by subtraction, as in Table 3.

Also recorded in Table 3 is the apportionment of the total number of degrees of freedom for the general $r \times c \times l$ case. This presents no difficulties and is easily generalized to tables of higher order. With $r = c = l = 2$ it is apparent that there is only 1 d.f. per component, and the usual continuity correction could be applied (to the first-order chi-squares) in such cases. It is also apparent that the choice of parameters (estimated or theoretical) typically effects the values of all 7 components, as indeed it should. Another fully worked comparison may be found in the original Lancaster (1951) article.

TABLE 3

Summary of component chi-squares

Comparison	*Value of χ^2 using theoretical parameters*	*Value of χ^2 using estimated parameters*	*Degrees of freedom*
χ^2_r	2·504	0	$(r-1)$
χ^2_c	·948	0	$(c-1)$
χ^2_l	15·170	0	$(l-1)$
χ^2_{rc}	·533	·688	$(r-1)(c-1)$
χ^2_{cl}	·726	·410	$(c-1)(l-1)$
χ^2_{lr}	·237	·795	$(l-1)(r-1)$
χ^2_{rcl}	·134	·301	$(r-1)(c-1)(l-1)$
Total χ^2	20·252		$(rcl-1)$
Total χ^2		2·194	$(rcl-r-c-l+2)$

The only significant value in the above table is $\chi^2_l = 15{\cdot}170$ which with $l-1 = 1$ d.f. reaches the ·01 per cent. level of significance approximately. The conclusion is that $p_{\cdot\cdot 1} \neq \frac{1}{2}$, and that there is more than a 50/50 chance of recalling attribute C of the advertisement.

10.2. *Additional Applications*

Since the Lancaster method partitions the total chi-square into additive components, it is possible to combine these in various ways to test additional hypotheses. In Table 3, for instance, there are 3 interaction terms χ^2_{rc}, χ^2_{lr}, and χ^2_{rcl} in which the *row* classification is involved, and if the sum of these terms (with 3 d.f.) is not statistically significant it can usually be concluded that the row classification is independent of the rest. In Table 3 (with parameters estimated from the data)

$$\chi^2_{rc} + \chi^2_{lr} + \chi^2_{rcl} = 1{\cdot}784$$

which, with 3 d.f., is not significant.

This value compares quite closely with that of 1·706 obtained in Section 4.1, but the two values cannot be expected to be equal unless the totals n_{jk} are fixed in advance (Snedecor, 1958). If the n_{jk} totals are so fixed, it must be remembered that they become the cell frequencies of the reduced $(c \times l)$ table from which the χ^2_{cl} component

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is computed. For this reason it is usual to select $n_{.jk}$ so that $\chi^2_{cl} = 0$, and this means imposing the condition that

$$n_{.jk} = n_{.j.} n_{..k}/N.$$

This condition is most simply met by making all the $n_{.jk}$ totals equal, and a worked example of such a case may be found in Lancaster's paper. It would of course be very odd for an investigator to fix the $n_{.jk}$ totals at the amounts shown, e.g., in Table 1. In fact if $n_{.jk} \neq n_{.j.} n_{..k}/N$, Lancaster shows that his method cannot be justifiably applied unless each value n_{ijk} is first subjected to the transformation

$$n_{ijk} \rightarrow (n_{ijk} - p_{i..} n_{.jk})/\sqrt{(p_{i..} n_{.j.} n_{..k}/N)}.$$

10.3. *Limitations of the Lancaster Method*

In the $2 \times 2 \times 2$ case in which $p_{i..}$, etc. are estimated from the data, Lancaster's method defines second-order interaction *residually*—as the difference between the total chi-square and the chi-square due to the sum of the first-order interactions. It is therefore important to note that his first-order interactions are derived from the *reduced* 2×2 tables, and to the extent that, e.g., A/B interaction is different in C_1 from C_2 the reduced table will not give an accurate account of this first-order (A/B) interaction. It follows that Lancaster's method marks off the boundary between first-order and second-order interaction in a manner that may not be intuitively satisfactory, and it is in fact possible to construct artificial examples to demonstrate this. This raises the question of whether it is possible to set up criteria which determine the boundary in a more convincing way.

Since there is not even general agreement on how to measure first-order interaction in a simple two-way table, it might seem that the investigator is free to set up (and justify) a wide variety of possible criteria. However, different measures of interaction will (in general) produce different lines of demarcation between the two kinds of interaction, and this fact can be utilized to help ensure that a particular method of analysis is intuitively acceptable.

In a three-way table, for instance, there can clearly be no second-order interaction if one classification is completely independent of the rest. In the $2 \times 2 \times 2$ example discussed in Section 9, there is complete independence when $t = u$, and for this case it is easy to show that Lancaster's method and Bartlett's method (described in Section 11) both give second-order interaction of zero.

However, the independence of one classification cannot be made a *necessary* condition of zero second-order interaction, since it is quite possible to envisage a situation in which all three classifications interact, but in pairs only. For reasons such as this, Simpson (1951) has suggested a more general condition of zero second-order interaction in $2 \times 2 \times 2$ tables (having 3 ways A, B, and C of classification):

> "Granting that there is an association between A and B we shall say that there is no second-order interaction between A and B on the one hand and C on the other, if the degree of association between A and B is the same in class C_1 as in class C_2."

This statement formulated in terms of A/B interaction might equally well be formulated in terms of B/C interaction (in A_1 and A_2) or C/A interaction (in B_1 and B_2). Each of these statements could be tested separately, but for the purpose of deriving an *over-all* measure of second-order interaction it should clearly make no difference which statement is used. Simpson has accordingly suggested imposing the condition that all 3 statements should be satisfied simultaneously. This condition is logically

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attractive, since it guarantees that second-order interaction is defined in a manner which is symmetrical with respect to all 3 ways of classification. If the cross-product ratio (or some simple function of it) is used as the measure of interaction in each 2×2 section, Simpson goes on to show that all 3 statements are satisfied if

$$p_{111}p_{221}p_{212}p_{122} = p_{222}p_{112}p_{121}p_{211}$$

which is precisely the form of the hypothesis given by Bartlett.

In the $2 \times 2 \times 2$ example of Section 9, the above condition clearly holds if $t \neq u$ since

$$a.d.tb.uc = b.c.ta.ud \quad \text{for any } t \text{ or } u.$$

Bartlett's method therefore yields zero second-order interaction in this case, but if $t \neq u$ Lancaster's method does *not* (in general) yield zero second-order interaction.

Nevertheless, Lancaster's procedure has its points. A rather obvious merit is that the component chi-squares *exactly* total the chi-square of the table considered as a whole. If this additive property is to be retained, then any alternative definition of second-order interaction (such as Bartlett's) must eventually come to grips with the problem of how *first-order* interactions are to be measured. Such arguments are, of course, highly relevant in assessing the relative merits of various models of analysis, but the fact remains that if Simpson's criteria are considered to be intuitively compelling, it is Bartlett's test, and not Lancaster's, which should be applied. For some alternative ways of defining second-order interaction, Section 12.2 may be consulted.

11. Bartlett's Test of Second-order Interaction

11.1. *Outline of Procedure*

The rationale of Bartlett's test is best explained by reference to tables of size $2 \times 2 \times 2$. In this limiting three-way case, the cross-product ratio is used as the measure of first-order interaction (in each 2×2 part) and the *ratio of these ratios* is used as the measure of second-order interaction.

Thus, if a $2 \times 2 \times 2$ table has one layer with cell frequencies $\frac{a|c}{b|d}$ and the other layer with frequencies $\frac{e|g}{f|h}$ the first-order interactions are $(ad)/(bc)$ and $(eh)/(fg)$ respectively, and the second-order interaction is the ratio $[(ad)/(bc)]/[(eh)/(fg)]$, i.e. $(adfg)/(bceh)$.

Furthermore, if the interactions in the two 2×2 layers are identical then $(ad)/(bc) = (eh)/(fg)$.

Hence
$$(adfg)/(bceh) = 1,$$

or
$$adfg = bceh$$

and this corresponds with Bartlett's statement (expressed in probabilities) of the condition of zero second-order interaction for the $2 \times 2 \times 2$ case (Bartlett, 1935).

It is therefore required to find the minimum deviation $|x|$ common to all 8 cells, which will bring about this relationship for zero second-order interaction. The value of x is accordingly determined by the equation:

$$(a+x)(d+x)(f+x)(g+x) = (b-x)(c-x)(e-x)(h-x).$$

This yields a cubic equation (with one acceptable root) which is solvable by standard numerical methods (Littlewood, 1958) or by tables (e.g. Salzer *et al.*, 1958). The 8 expected cell frequencies $(a+x), (b-x) \ldots (h-x)$, are now computed, whence

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chi-square (adjusted for continuity if $|x| > \cdot 5$) is:

$$\chi^2 = (|x| - \cdot 5)^2 \left(\frac{1}{a+x} + \frac{1}{b-x} + \ldots + \frac{1}{h-x} \right) \quad \text{with 1 d.f.}$$

For the Table 1 data the cubic equation is:

$$270x^3 - 674x^2 + 285{,}104x - 303{,}632 = 0$$

so $x = 1 \cdot 067$ approximately, and after correction for continuity $\chi^2 = \cdot 083$ with 1 d.f.

This value is considerably different from the Lancaster value of ·301, but the difference is partly attributable to the fact that the correction for continuity has a disproportionately large effect when x is small. Without this correction Bartlett's $\chi^2 = \cdot 293$, and it is apparent that the adjustment cannot be made at all if $|x| < \cdot 5$. However, this is quite immaterial for if x is small the inference (that there is no second-order interaction) is the same either way.

11.2. *Assessment of Bartlett's Procedure*

Bartlett's definition of second-order interaction is simple, and logically consistent with his measure of first-order interaction. Each is expressed as a ratio, and each is measured (in magnitude and direction) by the extent to which the ratio is greater or less than unity.

Furthermore, it is easy to show that Bartlett's definition satisfies Simpson's condition of symmetry, since there are 3 possible pairs of first-order ratios in the $2 \times 2 \times 2$ table, viz.:

$$(ad)/(bc) \text{ and } (eh)/(fg), (ag)/(be) \text{ and } (ch)/(df), \text{ and } (af)/(ce) \text{ and } (bh)/(dg)$$

and the ratio of these pairs equals $(adfg)/(bceh)$ in each case.

Similarly, if there is identical (first-order) interaction within each 2×2 section, then $(ad)/(bc) = (eh)/(fg)$, etc.,

$$\text{i.e.} \quad adfg = bceh,$$

so Bartlett's simple expression fully satisfies Simpson's criterion for zero second-order interaction.

From the point of view of convenience, it is worth remarking that Bartlett's test is unaltered if, e.g., the marginal totals $n_{jk.}$ are fixed in advance. In fact Bartlett's original article was explicitly concerned with such a case. However, it is highly reasonable to expect that different sampling schemes should produce different estimates of second-order interaction, so this particular feature of Bartlett's method should be viewed with some misgiving.

In contrast with Bartlett's formulation, Lancaster's definition of second-order interaction is lengthy and difficult to verbalize. For the $2 \times 2 \times 2$ case in which $p_{i..}$, etc. are estimated from the data, and only the total N is fixed in advance (cf. Section 3.1), it can be shown that Lancaster's second-order interaction is zero if

$$a(qq'q'') + d(pp'q'') + f(pq'p'') + g(qp'p'') = b(pq'q'') + c(qp'q'') + e(qq'p'') + h(pp'p'')$$

where p, p' and $p'' = p_{1..}, p_{.1.}$, and $p_{..1}$

and q, q' and $q'' = p_{2..}, p_{.2.}$, and $p_{..2}$.

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This expression satisfies Simpson's condition of symmetry, but is substantially different from Bartlett's expression. In considering the nature of the difference Lancaster (1951) shows more specifically that:

> "Bartlett's χ^2 is asymptotically equal to our second-order interaction.... However, the asymptotic approach to equality may not be rapid as the observed numbers increase. There may be discrepancies even at a size not likely to be met with in practice, especially if there is any considerable degree of first-order interaction."

An example of a substantially large difference can be found in a short article by Snedeco r(1958) which compares the methods of Bartlett, Lancaster, and Mood (see Section 14.1).

In so far as the two methods give different results, researchers must decide which is more appropriate to their own particular needs. Lancaster's method has the advantages of computational ease and additivity, but there is little to be said in favour of an easy method of computation if it measures interaction in an intuitively unacceptable way. And the fact that a test has been especially constructed to make the chi-squares add together may be a reason for distrusting it rather than preferring it.

The computational disadvantage of Bartlett's test becomes much greater for tables larger than $2\times2\times2$. In the $2\times2\times2$ case it is necessary to solve only one cubic equation in one unknown, but in more complex tables, e.g., of size $2\times2\times l$ the test involves the solving of $(l-1)$ simultaneous third-degree equations in as many unknowns. However, Norton (1945) has demonstrated a rather neat iterative procedure for solving these systems of equations.

More recently, Roy and Kastenbaum (1956) have extended the "no second-order interaction" hypothesis to the general $r\times c\times l$ case, and have shown that the computation here involves $(r-1)(c-1)(l-1)$ simultaneous third-degree equations in as many unknowns. For this general case, Roy and Kastenbaum's condition of zero second-order interaction (described by them as the "no three-factor interaction" hypothesis) is:

$$p_{rcl}p_{ijl}p_{ick}p_{rjk} = p_{icl}p_{rjl}p_{rck}p_{ijk}$$

for $i = 1,2,\ldots,(r-1)$ and $j = 1,2,\ldots,(c-1)$ and $k = 1,2,\ldots,(l-1)$, which does in fact yield a set of $(r-1)(c-1)(l-1)$ equations after appropriate substitution of all possible values of i, j, and k.

In the $2\times2\times2$ table $r = c = l = 2$ and i,j, and k can each take the value of 1 only, so Roy and Kastenbaum's condition is demonstrably identical with Bartlett's for this case, i.e.

$$p_{222}p_{112}p_{121}p_{211} = p_{122}p_{212}p_{221}p_{111}.$$

For all the more complicated three-way tables, Kastenbaum and Lamphiear (1959) have developed a technique of computation which is well suited for high-speed computers, and which ultimately gives a chi-square with $(r-1)(c-1)(l-1)$ degrees of freedom.

It is perhaps a good indication of the degree of acceptance of Bartlett's test that so much ingenuity has been applied to overcoming the computational complexities of his method. What is now wanted is a thorough reappraisal of the validity of his model. By way of emphasis the next section deals with a relatively little-used method of analysis which, for certain kinds of social and psychological data, may often seem more appropriate than either Bartlett's or Lancaster's method. (Note, however, that some additional virtues of the Bartlett method emerge in Section 12.2.)

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12. STOUFFER'S METHOD OF CROSS-TABULATION

12.1. *Outline of Procedure*

Whenever two variables are found to be associated, it is almost always possible to throw more light on their association by the systematic introduction of additional variables (Zeisl, 1958). Many three-way tables arise in this way. In such cases, interest usually remains focused upon a two-variable association which is cross-tabulated to see how the association fluctuates in the presence of other variables.

TABLE 4

306 *parolees cross-tabulated with respect to* 3 *characteristics*

	Parolees with previous record		*Parolees without previous record*	
Parole record	*"Lone" offender*	*"Group" offender*	*"Lone" offender*	*"Group" offender*
Violator	21	36	26	28
Non-violator	26	49	27	93
Σ	47	85	53	121

$\hat{p}_1 = 21/47 = \cdot4468$ $\quad \hat{p}'_1 = 26/53 = \cdot4906$

$\hat{p}_2 = 36/85 = \cdot4235$ $\quad \hat{p}'_2 = 28/121 = \cdot2314$

$d = \cdot0233$ $\quad d' = \cdot2592$

Stouffer and his colleagues appear to have been the first to recognize the potential usefulness of cross-classification procedures. In an early series of papers (Stouffer, 1933; Stouffer and Dorn, 1933; Tibbitts and Stouffer, 1934) they applied the technique to a number of situations and also indicated some useful tests of significance.

Table 4 is adapted from an original article by Tibbitts and Stouffer (1934) in which a highly significant association had been found between parole violation and type of offender. It appeared that the "Lone" type of offender was much more inclined to break parole than the "Group" type of offender, and the authors were interested to know how this association would vary if the previous record of the parolee were taken into account. Table 4 therefore shows the type of offender/parole record association broken down into two tables according to whether the parolees had (or had not) a previous criminal record.

In this case there are no "independent classifications" and a rather different question is being asked in which a difference between proportions (of parole violators) within one 2×2 section is compared with the corresponding difference in another 2×2 section.

As a starting point in the analysis it is readily seen that the proportions of violators (among lone and group offenders) are almost identical in the left-hand 2×2 section of Table 4. In fact the difference between the proportions equals

$$d = (p_1 - p_2) = (21/47 - 36/85) = \cdot0233 \text{ only,}$$

and it is this difference which is tested (for significant departure from zero) in the conventional 2×2 chi-square test. An application of this test shows that $\chi^2 = \cdot07$ which, with 1 d.f., is rather smaller than might be expected by chance, and certainly non-significant.

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In contrast, the difference between the proportions in the right-hand 2×2 section is considerably larger ($d' = \cdot 2592$), and $\chi^2 = 6 \cdot 6$ which, with 1 d.f., almost reaches the 1 per cent. level of significance. As Tibbitts and Stouffer point out, however:

> "It is entirely possible that one difference may be significant, and the other not, and yet the difference between the two differences may not be significant. Conversely it is possible, if differences have opposite algebraic signs, that neither may be significant when considered alone, but that the excess of one difference over another will be significantly large. . . . A simple formula is needed to provide an evaluation of the excess of one difference between two proportions over another difference between two proportions, such as the excess of ·2592 over ·0233."

To achieve such an evaluation it is first necessary to estimate the standard error of the difference between the differences d and d'. This estimate is provided by the formula

$$\sigma_{d-d'} = \sqrt{(p_{1..}\, p_{2..} \Sigma\, 1/n_{jk})}$$

where $p_{1..}$ = total proportion of violators $= (21+36+26+28)/N = \cdot 3627$,

$$p_{2..} = (1-p_{1..}) = \cdot 6373,$$

$$\Sigma 1/n_{jk} = 1/47+1/85+1/53+1/121 = \cdot 0602.$$

Hence $$\sigma_{d-d'} = \sqrt{(\cdot 3627)(\cdot 6373)(\cdot 0602)} = \cdot 1180.$$

The extent to which the observed difference $(d-d')$ departs from hypothetical zero may now be tested by setting up the critical ratio:

$$(d-d')/\sigma_{d-d'} = \cdot 2359/\cdot 1180 = 2 \text{ approximately.}$$

If the value of 2 standard deviation units is referred to a table of areas under the normal curve,

$$P = 2(\cdot 5 - \cdot 4773) = 2(\cdot 0227) = \cdot 0454,$$

so the difference just exceeds the 5 per cent. level of significance.

This moderately significant result supports the view that the difference between lone offenders and group offenders (in respect of parole violation rate) is definitely greater among parolees having no previous criminal record.

In the original article several cross-tabulations were introduced in succession—each one operating as a *post hoc* control of some variable that was uncontrolled (or uncontrollable) at the time the sample was taken. Tibbitts and Stouffer remark:

> "Such a process of successive subdivision has obvious limits as the data soon thin out. Nevertheless, it is perhaps the most direct and fruitful method of statistical investigation in sociology, and often yields surprising facts which are distorted or concealed when the factors are considered in the mass."

12.2. *Comparison with Other Methods*

It should be noted at once that the Stouffer comparison of two 2×2 tables is not equivalent to the Section 6 test that the two 2×2 tables are homogeneous. This can be illustrated by applying the homogeneity test to the two 2×2 sections of Table 4, where it will be found that $\chi^2 = 9 \cdot 598$ with 3 d.f. so $p < \cdot 025$.

The difference between the two methods derives from the fact that the Stouffer technique is concerned solely with the comparison of $(p_1 - p_2)$ in one 2×2 table with $(p_1' - p_2')$ in the other. Since these two differences can be equal even if $p_1 \neq p_1'$ and $p_2 \neq p_2'$ it follows that the Stouffer test can yield a non-significant result even if the two 2×2 tables are *not* homogeneous.

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However, the converse does not hold, for if the two 2×2 tables *are* homogeneous, the Stouffer test is bound to give a non-significant result (and so also is the Bartlett test).

It is also instructive to compare the Stouffer test with Bartlett's by means of an artificial example. If the numbers $\frac{60\,|\,5}{20\,|\,10}$ and $\frac{60\,|\,20}{5\,|\,10}$ be substituted for $\frac{a\,|\,c}{b\,|\,d}$ and $\frac{e\,|\,g}{f\,|\,h}$ respectively, it will be seen that the interaction (cross-product) ratio is identical in each 2×2 section, and equals $(60\times10)/(20\times5)=6$. Hence the Bartlett test yields a chi-square of zero. However, the difference between proportions in the first 2×2 section is $60/(60+20)-5/(5+10)=\cdot42$, whereas the difference between proportions in the other 2×2 section is $60/(60+5)-20/(20+10)=\cdot26$. Thus these two differences between proportions are substantially different, notwithstanding the fact that the interaction ratios are identical in each 2×2 section.

Clearly the differences between proportions will not be equal unless

$$\frac{a}{(a+b)}-\frac{c}{(c+d)}=\frac{e}{(e+f)}-\frac{g}{(g+h)},$$

i.e. unless

$$\frac{ad-bc}{(a+b)(c+d)}=\frac{eh-fg}{(e+f)(g+h)}.$$

It is this relationship which is tested by the Stouffer technique, and an independent discussion of it (and of its lack of symmetry) can be found in the Simpson (1951) reference. Since this relationship does not imply (and is not implied by) Bartlett's $(ad)/(bc)=(eh)/(fg)$ it might appear that Bartlett's test is *never* appropriate for testing the kind of difference (involving differences between proportions) in which psychologists are so often interested. However, the problem is not as easy as this, and it is desirable to examine more closely the circumstances in which one test might be preferred to the other.

Since the values of p can only lie in the range $0-1$, it is first necessary to point out that at the extremes of this range (e.g. $p<\cdot1, p>\cdot9$) it is not at all reasonable to expect (p_1-p_2) to remain constant. Thus if the left-hand section of Table 4 had shown frequencies $\frac{20\,|\,10}{80\,|\,90}$ this would have indicated that, among parolees having a previous criminal record, there were 20 per cent. "lone" violators but only 10 per cent. "group" violators. If among the parolees having no previous record the "lone" violation rate had dropped from 20 per cent. to 10 per cent. it could hardly be expected that the "group" violation rate would correspondingly reduce from 10 per cent. to zero. Instead, it might be expected that there would be a proportionate decrease to one half of the original percentage in both groups, giving $\frac{10\,|\,5}{90\,|\,95}$, and a departure from *proportionality* would be described as an interaction between the effects of the parolees and the types of previous record. In these circumstances the proportions of violators and non-violators, in the two 2×2 sections, might be expected to take the following form:

p_1	λp_1	p_1'	$\lambda p_1'$
$(1-p_1)$	$(1-\lambda p_1)$	$(1-p_1')$	$(1-\lambda p_1')$

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where p_2 and p_2' have been replaced by λp_1 and $\lambda p_1'$ to indicate that they represent identical proportionate decreases $(1 : \lambda)$ in p_1 and p_1' respectively. The quantity λ lies in the range $0-1$ of course, and it may be supposed that $p_1 > p_1'$.

In such tables it is easy to show that Bartlett's second-order interaction ratio of $(adfg)/(bceh)$ must always lie between 1 and $(1-p_1')/(1-p_1)$. For small p_1 and p_1' this ratio approximates closely to unity so it follows that if, due to the presence of small p_1-values, the parole group differences are likely to be proportional rather than linear, Bartlett's test is more suitable than Stouffer's.

It is also worth remarking that if l kinds of previous record had been distinguished, so as to give l 2×2 tables (of the above kind) for simultaneous comparison, then Bartlett's ratio would approximate to unity for every pair of 2×2 tables having *similar* p_1-values. If it should happen that almost all the l p_1-values are similar and fairly small (say in the 0–·5 range) and if differences are expected to be proportional within each 2×2 table, then Bartlett's test—as extended by Norton (1945)—should again be preferred.

An alternative expectation (which also reinstates the Bartlett test) might be that the *odds* $p_1 : (1-p_1)$ would be changed by the same factor in each 2×2 table. Thus the numbers $\begin{array}{c|c} 92 & 46 \\ \hline 8 & 54 \end{array}$ would indicate odds of 92 : 8 [i.e. (23/2) : 1] and 46 : 54 [i.e. (23/27) : 1] in favour of parole violation by lone offenders and group offenders respectively. If the odds (23/2) : 1 are reduced by a factor of, e.g., (21/46) to (21/4) : 1, and if the odds (23/27) : 1 are reduced by the same factor to (7/18) : 1, the percentages of violations and non-violations in the new 2×2 table are given by the numbers $\begin{array}{c|c} 84 & 28 \\ \hline 16 & 72 \end{array}$ where $84 : 16 = (21/4) : 1$ and $28 : 72 = (7/18) : 1$. For this case (and for all other cases in which the odds are changed by the same factor) Bartlett's ratio is exactly equal to unity, so it turns out that his test is (once again) applicable whenever there is reason to suppose that the odds might change in this manner.

Note that the *effect* of the above assumptions (about the way in which the odds should change) is to give extra weight to changes in percentages at the extremes of the range. Thus, in the example just considered, a change from 92 per cent. violation to 84 per cent. violation is held to be comparable with a change from 46 per cent. to 28 per cent.—so there is no expectation that (p_1-p_2) should remain constant. This is plausible since the first change from 92 per cent. to 84 per cent. doubles the non-violation rate (from 8 per cent. to 16 per cent.) and is clearly more notable than a change of 8 per cent. occurring nearer the centre of the range. As Jolly (1950) has pointed out, the weighting that is implicit in Bartlett's method is exactly equivalent to that resulting from the logit transformation. A slightly lighter weighting of the extreme percentages is also provided by the probit and other similar transformations, but reference should be made to specialized works on this subject (e.g. Finney, 1952; Berkson, 1953) for exact details.

For practical purposes, it is worth remembering that for data that can be arranged in the manner of Table 4, if *all* proportions lie within the range ·2–·8, Stouffer's test, Bartlett's test, and tests made consequent to the probit transformation (Jolly, 1950) are all likely to give roughly similar results. Since the actual data will rarely indicate the "correct" choice of test, the method of analysis will presumably be determined either by some *a priori* assumption about the way in which the percentages are likely to behave, or (more probably) by considerations of convenience in computation. If any proportions lie outside the ·2–·8 range, Stouffer's test becomes somewhat questionable,

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but within the range ·1–·9 Bartlett's test (entailing the logit transformation) and tests using the probit transformation give very similar weightings to the proportions involved. Outside the range ·1–·9 the differences between the logit and probit transformations become more marked and require expert handling.

12.3. *Generalizations* (+ *Cochran's Criteria*)

Table 4 was derived from data collected about foreign-born parolees. If a pair of 2×2 tables had also been set up for native-born parolees, a similar analysis could have been conducted, and it might *then* have been interesting to test whether the finding (given above) in respect of foreign-born parolees was significantly different from that of native-born parolees. This further comparison would have made the order of differences compared higher, since it would have entailed testing differences between differences between differences between proportions. The original Tibbitts and Stouffer (1934) article does in fact provide a generalized formula for all higher-order differences, and this formula is reproduced by Hagood (1941) who expounds the Stouffer method at great length.

A different generalization would be required if Table 4 had consisted of more than two 2×2 sections. For if l kinds of previous record (e.g. for $0, 1, \ldots, l-1$ prior convictions) had been distinguished, there would be l 2×2 tables, and l values of $(p_1 - p_2)$ to be tested for homogeneity. Theoretical difficulties arise here. Thus if the totals of each 2×2 table show marked differences (e.g. in excess of the ratio 2 : 1 for any pair of totals) some weighting procedure is desirable to take account of the fact that the smaller-totalled tables are less reliable in detecting a difference between p_1 and p_2. Also, if the p_1 and p_2 values vary over a wide range (e.g. 0–50 per cent.) it cannot be expected that $(p_1 - p_2)$ will remain constant for all levels of p. Furthermore, the variance of $(p_1 - p_2)$ will be different for each 2×2 table. Of course, these are all arguments for not considering $(p_1 - p_2)$ in such cases, and Tibbitts and Stouffer did not in fact generalize their formulae to cover the $l > 2$ case. Hence the nearest test available is that for the homogeneity of l 2×2 tables or alternatively the Bartlett test (having due regard to the comments in Section 12.2 preceding).

If the l values of $(p_1 - p_2)$ are heterogeneous in sign, or small in magnitude, the possibility arises that they are all chance deviations from zero, and that there is therefore no significant difference between p_1 (proportion of lone violators) and p_2 (proportion of group violators) in the population as a whole. The null hypothesis is:

$$H_0 : (P_1 - P_2) = 0$$

where $(P_1 - P_2)$ denotes the $(P_1 - P_2)$ value for the total population from which the l samples were drawn.

One method of testing the significance of the difference in the total population (of parolees) is to combine all the data into a single 2×2 table for which χ^2 is computed in the usual way. As indicated in Section 9, this procedure is not legitimate if the individual 2×2 tables are not homogeneous, etc., since the combined data will not accurately reflect the information contained in the original l tables.

Another popular method is to compute χ^2 for each 2×2 section, and add them—making use of the fact that the sum of these l values of chi-square also follows the chi-square distribution with l degrees of freedom. This "additive chi-square" technique is a poor one, since it takes no account of the signs of the differences $(p_1 - p_2)$. As Cochran (1954) points out, it lacks power in detecting a difference that shows up consistently in the same direction in all (or most) of the individual tables.

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An alternative is to compute the χ (i.e. $\sqrt{\chi^2}$) values and add them, taking account of the signs of the differences. Since χ is approximately normally distributed with mean 0 and with unit S.D., the sum of l independent χ values (where each χ is given the sign of p_1-p_2 for that table) is approximately normally distributed with mean 0 and S.D. $=\sqrt{l}$. Hence the test criterion $\Sigma\chi/\sqrt{l}$ may be referred to a table of areas under the normal curve.

For the Table 4 data, the (p_1-p_2) values are both positive, and $\Sigma\chi/\sqrt{l}=2$ standard deviation units approximately, which corresponds to a P-value of about ·0454 (the similarity between this value and the one obtained in Section 12.1 being entirely coincidental).

In section 8 of the paper by Cochran (1954) the theoretical difficulties noted above are discussed at some length, and the foregoing test criterion is recommended as being suitable for all sets of 2×2 tables in which:

(a) the totals of the l tables do not fall outside the ratio 2 : 1,

(b) the p-values all lie within the range ·2–·8.

In the same paper, Cochran proposes (and justifies) an alternative criterion for cases in which conditions (a) and/or (b) are not satisfied. See also Yates (1955).

It is important to note in conclusion that the Cochran criterion does *not* test the homogeneity of the (p_1-p_2) values. For if there were just two 2×2 sections involved, and if they each yielded highly significant χ-values which were equal in magnitude but opposite in sign, the Cochran criterion would equal zero. This would indicate (appropriately enough) that there was no significant difference between p_1 and p_2 in the population as a whole, but it would clearly be wrong to infer that the (p_1-p_2) values were homogeneous.

12.4. *Special Uses of Cross-tabulation*

One of the great merits of cross-tabulation is that it can be used to throw more light on the *authenticity* of an association between two (or more) variables. Thus if a 2×2 table shows significant association between a particular skill and age, a dichotomized "test" variable (e.g. high *v.* low visual acuity) could be introduced. In the $2\times2\times2$ table that results from the extra subdivision, it might be found that there is now a significant association between skill and visual acuity, and also between visual acuity and age; but for each level of visual acuity considered separately there is no longer any association between skill and age. In these circumstances it would be concluded that the "real" association is between skill and visual acuity, and the original (spurious) association only arose because older subjects tend to have poorer visual acuity. The fact that a 2×2 table showing association can be decomposed into two or more tables showing no association has been well known since Yule's classic discussion on "illusory association" (Yule and Kendall, 1950), and has been systematically exploited in Lazarsfeld's work on Latent Structure Analysis (Lazarsfeld, 1955; Stouffer *et al.*, 1950). The general principle is also implicitly covered in Section 9 above.

In addition to testing the validity of an observed association, it is sometimes possible to infer the direction of their causal relationship. For instance Blau (1955) used cross-tabulation techniques to establish that in certain respects *self-image* is determined by the *attitude of others* (as perceived by the self) and not *vice versa.*

These are just a few of the many subtle ways in which cross-tabulation can serve to enrich one's understanding of the manifest data. A detailed formalization of the

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analytic procedures involved has been attempted by Kendall and Lazarsfeld (1950) and an enlarged account may be found in chapter VII of Hyman (1955). For a clarification of the logical issues involved in such procedures the reader should refer to Simon (1957). For an excellent non-technical discussion on the uses of cross-tabulation Zeisl (1958) is the best source, since it lucidly illustrates almost all the ways in which cross-tabulation can clarify relationships appearing in the conventional two-way type of table.

13. Multiple Contingency Tables showing Attitude Change

In addition to the cases already considered, there is a special type of contingency table, which is particularly important for social and psychological researches, and which is used for depicting attitude changes. Table 5 is adapted from Appendix B of Hovland *et al.* (1949) and is fairly typical.

TABLE 5

Record of attitude change

(Experimental Group) Propaganda given between 1st and 2nd response

		1st response			
		A	B	C	(Σ)
2nd response	A	19	1	—	20
	B	28	35	1	64
	C	2	10	4	16
	(Σ)	49	46	5	100

(Control Group) No propaganda given

		1st response			
		A	B	C	(Σ)
2nd response	A	40	9	—	49
	B	4	38	5	47
	C	1	1	2	4
	(Σ)	45	48	7	100

It will be seen that each 3×3 section completely specifies the pattern of attitude change occurring in a "before–after" type of experiment. Thus the n_{21} value of 28 in the left-hand table indicates that 28 persons shifted from a category *A* response to a (more moderate) category *B* response, after being subjected to propaganda.

Since opinions tend to fluctuate, and also to "regress" towards the average opinion (Edwards, 1957; Torgerson, 1958) irrespective of propaganda influence, it is clear that a control group is needed for comparison. Moreover, if the experimental group shows very marked attitude changes, it might be desirable to cross-tabulate or "stratify" the group according to some possibly relevant variable such as educational level. This would mean breaking down the 3×3 table into two or more component tables (also of size 3×3) to ascertain whether attitude changes were different for different educational levels. In any case, the problem is one of analysing a set of 3×3 tables of a rather special kind, and there are several hypotheses that need to be tested in such cases.

As a starting point, it is important to establish that the control and experimental groups are well matched in initial attitude. The distribution of initial attitudes is provided by the marginal column totals $n_{.k}$ of each 3×3 section, and these two distributions may be compared by means of the standard chi-square test.

The two distributions of "second" opinions can naturally be compared in the same way (by using the $n_{j.}$ totals), but any differences that are found would have to

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be carefully related to *initial* differences between the two groups (even if these were not significant) and also to miscellaneous effects such as regression, etc., before any confident assertion could be made about the effectiveness of the propaganda.

Furthermore, it should be noted that tests based on the marginal totals do not make full use of the information contained within the 3×3 sections, since these totals can only indicate *net* changes of attitude. Thus the category A response in the control group of Table 5 shows a marginal change from 45 to 49, but in actual fact 5 persons shift away from category A, and 9 persons shift into category A. If these internal changes are examined more closely, it is seen that the cells n_{11}, n_{22}, and n_{33} of the descending diagonal (of each 3×3 section) show the number of persons who do not alter their response. These cells therefore provide a measure of the stability of the responses in each group, whereas the remaining cells reflect the "turnover" or instability of responses due to random fluctuations, regression, and propaganda, etc.—and can be tested for symmetry by means of a test proposed by Bowker (1948). In the absence of symmetry, the weaker hypothesis of equality of marginal distributions may also be of interest, and this can be tested by a method due to Stuart (1955).

Since the 3×3 tables provide a complete account of the pattern of attitude changes, and since these tables cannot be reduced without loss of information, there is clearly a need for some method by which a gross comparison can be made between one 3×3 (or, in the general case, $l \times m$) table and another. The chi-square test of homogeneity of the 3×3 tables is in fact mentioned in Appendix B of Hovland *et al.*, but certain theoretical difficulties arise—especially in the interpretation of a significant result. For instance, differences in patterns of attitude change are confounded with differences due to the fact that the two groups are not initially (or finally) homogeneous, and no allowance is made for correlation between first and second responses. A further weakness is that if the response categories are ordinally scaled (in the sense that a shift from A to C is bigger than a shift from A to B) the test will not take this into account.

The foregoing paragraphs have deliberately given only the briefest indication of the difficulties encountered in measuring attitude changes, since the class of contingency tables involved is quite atypical and only requires mentioning for the sake of completeness. (There is a sense in which it can be argued that such sets of 3×3 tables cannot properly be described as multi-dimensional. The opening remarks of Section 6 are relevant here, but the objection is not in any event decisive, since it is possible to imagine a "genuine" $3 \times 3 \times 3$ table in respect of first, second, *and third* opinions. An article by Anderson (1954) does in fact deal with such cases.)

For an extremely thorough discussion of all the major problems associated with the measurement of attitude *changes*, Hovland *et al.* (1949) is still the best source, since it covers both qualitative and quantitative methods of analysis, and also indicates a number of useful significance tests. A later contribution of some importance is the already-mentioned article by Anderson (1954) on time changes in attitude. Anderson's methods are all based on the realization that the Table 5 type of contingency table can be immediately converted into a matrix of estimated transition probabilities, if each cell frequency is divided by the total N. Hence the entire problem is conceptualized as one which requires the comparison of two or more matrices. It is worth pointing out, however, that Anderson's methods also fail to take account of the fact that for two-way tables of size greater than 2×2, the response categories are (in general) ordinally scaled.

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14. ALTERNATIVE METHODS OF ANALYSIS

14.1. *Likelihood Ratio Tests*

This paper has been exclusively concerned with large sample tests of the null hypothesis, and much use has been made of the fact that, for large N, the test criterion

$$\Sigma(\text{Observed} - \text{Expected})^2/\text{Expected}$$

approximately follows the chi-square distribution.

In the statistical literature many writers (Anderson and Bancroft, 1952; Mood, 1950; Roy, 1957) have found it convenient to develop test criteria in *likelihood-ratio* form. This gives something different from the above, but the two criteria are asymptotically equivalent, and there are no known theoretical reasons for preferring one to the other in small samples. In general the distributions of both test criteria are very close to that of the chi-square distribution, but if N is excessively small the approximation naturally breaks down. Since for all practical purposes the expected frequency and likelihood-ratio tests give the same answer, the well-known expected frequency method should be used, since it is computationally easier.

For very small N it is worth noting that Freeman and Halton (1951) have generalized Fisher's "exact" 2×2 test to cover the $r \times c$ and $r \times c \times l$ cases. The computational procedure is straightforward but very laborious, despite the availability of several short cuts.

14.2. *Analysis of Variance*

In many three-way tables that arise in practice, one classification has only two categories, such as success or failure, etc. In these cases the investigator is likely to be more interested in the behaviour of the percentages of successes (under differing conditions) than in the ratio of successes to failures, although it may still be relevant to apply Bartlett's test of ratios (cf. Section 12.2 above). An alternative procedure would be to reduce such tables to two-way form with a percentage as the variable, and to apply the method of analysis indicated in Section 9.10 of the revised Snedecor (1956).

This method is not illustrated here, since it is only applicable when one classification is dichotomous, and in such cases other methods of analysis (already described) are available. Briefly, the procedure is to transform the percentages to angles by means of the arcsin transformation, and to analyse the variance of these angles in the normal way. The effect of the arcsin transformation is to normalize the data by giving extra weight to the small percentages which have small variance. However, the transformation strictly requires equal values of $n_{.jk}$, otherwise difficulties of weighting are involved. For a fuller discussion and justification of this particular transformation the interested reader should consult Eisenhart *et al.* (1947).

Throughout this paper it has been assumed that each $r \times c \times l$ table consists of nothing more elaborate than a simple cross-tabulation of 3 factors divided into r, c, and l categories respectively. It is obvious that more sophisticated arrangements are possible. For instance, the row, column, and layer classifications might conceivably represent blocks, treatments, and associated sub-treatments in a manner analogous to that of certain analysis of variance designs. For such cases as this, no special techniques have yet been developed, although Roy and his colleagues are apparently working on the problem at the present time (Roy, 1957).

For the special case in which the data of interest are proportions, more progress has been made. In an important paper by Dyke and Patterson (1952) a method of

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analysis (involving use of the logit transformation) is given which provides quantitative estimates of the effects of 4 different factors on cancer knowledge. The important point to note about the Dyke and Patterson method is that it differs radically from the classical approach to the analysis of qualitative data by means of chi-square—for although the chi-square analysis provides tests of significance it cannot lead to estimates of the magnitudes of various effects.

14.3. *Information and Correlation Analysis*

A comparatively recent development has been the application of information theory to the analysis of multi-way contingency tables. In general, information measures are applicable whenever data can be arranged in contingency table form, for, if two or more classifications interact, each can be thought of as providing information (or reducing uncertainty) about the other(s). In the three-way case, for instance, both the row and column classifications can contribute information (jointly or severally) about the layer classification, and by procedures similar to analysis of variance the total amount of transmitted information can be partitioned into additive components associated with individual classifications and their interactions. However, there is no need to demonstrate such procedures here, as they have recently been set out in very comprehensive form, for both two-way and three-way tables, in chapter 8 of Kullback (1959). In the course of his exposition, Kullback shows that many of his information measures are numerically almost identical to the chi-square measures indicated in this paper. Where this is not the case, however, it is invariably the information measures which provide an additive analysis of component variation. This is not surprising, since additivity is the basis for the logarithmic function used in information theory (see also Section 6).

Since there now appears to be a surfeit of analytical methods, it is highly desirable to examine the relationships between them. In an important series of papers McGill and Garner have shown that there is a common logical structure underlying analysis of variance, information analysis, and correlation analysis (Garner and McGill, 1956; McGill, 1955). In particular, it turns out that analysis of variance and information analysis are analogous techniques for partitioning variability into components reflecting main effects and interactions, but information analysis has more generality, since it is applicable to data having the (non-metric) properties of only a nominal or ordinal scale. In contrast, analysis of variance can be meaningfully applied only to variables having the properties of at least an interval scale—but in such cases analysis of variance is more sensitive since (unlike information analysis) it retains information about the metric. In effect, this means that information analysis can conveniently be regarded as a non-parametric analysis of variance (Frick, 1959) having all the usual advantages and disadvantages of increased generality, and Kullback (1959) does in fact use it in this way (for nominally scaled data) in a number of illustrative examples.

McGill (1955) has also shown that the isomorphism of structure connecting analysis of variance and information analysis extends to correlation analysis, for in all three methods an attempt is made to account for the variation in some variable by the variation in one or more other variables. The relationship between correlation analysis and the chi-square tests described in this paper is similarly emphasized by Roy (1957). For instance, the Section 5 hypothesis of conditional independence is the analogue of the hypothesis of no partial correlation (between rows and columns, given layers). Similarly, the Section 4.1 hypothesis that the row classification is completely independent is the analogue of the hypothesis that there is no multiple

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correlation between rows and (columns, layers). There is no point in elaborating on correlation methods, however, since these are adequately dealt with in many standard text books.

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University Students, Staff and Recurrent Grants, Pre-war and Post-war

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Summary

The extent and pattern of the post-war increases of students at the English universities are discussed in Part I with reference to the distribution of the students amongst the universities, type of residence and fields of study. Part II presents data on changes in student-staff ratios and the composition of the academic profession in respect to grades, and in Part III a few aspects of the rise in central government grants to the universities are examined, particularly its relation to the rise in numbers of students.

Introduction

As the universities are coming under greater public scrutiny and the Robbins Committee is engaged in reviewing the structure and future of higher education in Great Britain, it may be useful to present information supplementary to that presented by Sir Keith Murray, Chairman of the University Grants Committee, in this *Journal* (Murray, 1958) and by the Committee itself in its periodic reviews (University Grants Committee, 1948, 1953, 1958).

Sir Keith was concerned mainly with the student population, staff, finance, etc. of the universities as a group. Here comparable information, based largely on the statistics published by the University Grants Committee, is given for individual universities. University work external to the institutions receiving grants from the Committee is thus ignored. Only the English universities are treated in detail; the constituent colleges of the University of Wales are treated as a group, as are the Scottish Universities. This limitation was self-imposed for reasons of brevity but in educational matters it is common to review the countries of the U.K. separately. Moreover, the official statistics are re-arranged, partly to make possible tentative comparisons between the individual universities, and for this purpose the English universities seem heterogeneous enough without introducing in detail the special cases of Wales and Scotland. It is easy to see some of the snags which comparisons of individual universities must encounter. Oxford and Cambridge have unique features. For example, their resident and academically active members can be associated in varying degrees with university, college and/or research institute and in the published figures there is no attempt to assess the full extent of their participation in teaching; consequently it is impossible to compare the student-staff ratios at Oxford and Cambridge with those at other universities. The University of London is a group of 39 institutions pursuing a large variety of academic activities. The University of Durham is (as yet) also a federal university but of only two constituent parts. There are seven large and five small civic universities, the specialist Manchester College of Science and Technology and the young University College of North Staffs. The development of the newly founded universities is a matter for the next 20 years.

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Comparisons of universities, though difficult and inevitably tentative, have some practical importance. Present government plans are for a rapid expansion of university education, particularly in science and technology, and the case for this has been strengthened by comparisons with some other highly developed countries. These comparisons are also difficult, but they are revealing and help to disturb complacency. What is almost certain is that future university expansion will not follow a completely foreseen course. Inter-university comparisons are necessary to understand what is happening, to help ensure that the provision for students at different institutions is kept more or less in line with intended policy, and to guide policy changes. The published statistics hardly serve these purposes adequately. As constitutional guardian of the autonomy of the universities as well as custodian of the funds provided by the central government for them, the University Grants Committee has a nice balance to strike in determining its policies; but the regular publication of more detailed information on the institutions to which it makes grants would probably advance the Committee's objectives in both its roles. Meanwhile this investigation attempts cautiously to see what light data at present published throw on the past pattern of development amongst the universities.

The period investigated is the twenty years 1938/9 to 1958/9, though the war period is skipped and the emphasis is often on the decade 1948/9–1958/9, i.e. from a year after the war in which the universities were coping with large numbers of ex-service students to a year in a period of a rapid expansion in which full-time students were almost exactly twice as numerous as in 1938/9. Between the beginning and end of the decade student numbers fell from a peak in 1949/50 to a low point in 1953/4 before rising again. It is for this reason that statistics relating to the year 1953/4 are often employed.

A terminological point concludes these preliminaries. To avoid tiresome repetition the terms "university" or "university college" are normally omitted; universities are specified by their geographical location.

Part I

STUDENTS

1. Distribution of Students by Faculty

The proportions of students in arts, pure science, technology, etc. vary greatly from university to university (Table 1). Some universities, like Oxford and Cambridge and the small universities of Exeter, Hull, Leicester and North Staffs., are mainly "arts", all except Cambridge predominantly so. London, Durham, Reading and the seven large civic universities (Manchester, Birmingham, Liverpool, Leeds, Sheffield, Bristol and Nottingham) are predominantly "non-arts". Only one university—Southampton—changed its character in this respect over the twenty years. Before the war nearly two-thirds of its students were "arts"; now students of science and technology are just in the majority.

Between 1938/9 and 1958/9 there was in Great Britain very little change in the proportion of students following arts courses (which include the social sciences and education); indeed in England it rose very slightly. It is plausible to think that part of the explanation of this fact is that arts faculties can usually be more easily expanded than other faculties, but Sir Keith Murray has warned us that "the comparison of the pre-war and post-war percentages is somewhat misleading in that there have been certain changes in the nature of what is included in arts and in the length of courses"

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TABLE 1

Distribution of full-time students by faculties, 1938/9 and 1958/9

Per cent.

		Arts	*Pure science*	*Tech-nology*	*Medicine, dentistry and vet. sci.*	*Agricul-ture and forestry*	*Total no. of students (= 100%)*
Birmingham .	1938/9	38·0	13·0	19·1	29·9	—	1,433
	1958/9	33·9	22·8	26·2	17·1	—	3,942
Bristol . .	1938/9	38·0	20·3	8·6	31·9	1·2	1,005
	1958/9	41·4	29·1	11·4	17·8	—	3,080
Cambridge .	1938/9	58·2	19·7	11·0	8·3	2·8	5,931
	1958/9	54·3	24·8	11·4	7·1	2·4	8,844
Durham . .	1938/9	38·7	14·2	15·3	28·0	3·7	1,709
	1958/9	39·8	22·2	19·2	15·4	3·4	4,954
Exeter . .	1938/9	74·2	24·4	0·5	1·0	—	422
	1958/9	66·1	33·9	—	—	—	1,232
Hull . . .	1938/9	64·2	24·1	10·5	1·2	—	162
	1958/9	65·5	34·5	—	—	—	1,420
Leeds . .	1938/9	31·5	14·2	14·8	35·0	4·5	1,757
	1958/9	39·4	17·9	26·5	13·8	2·4	4,538
Leicester† . .	1938/9	54·3	45·7	—	—	—	105
	1958/9	64·9	35·1	—	—	—	1,053
Liverpool . .	1938/9	25·4	13·4	20·0	41·3	—	2,055
	1958/9	29·9	29·2	15·8	25·1	—	3,667
London . .	1938/9	30·3	14·9	9·9	44·8	—	13,191
	1958/9	35·4	19·9	12·2	31·4	1·1	20,993
Manchester .	1938/9	39·8	22·0	10·7	27·4	—	2,108
	1958/9	42·8	28·4	13·8	15·0	—	4,626
,, C. of S. & T.	1938/9	—	—	100·0	—	—	354
	1958/9	—	—	100·0	—	—	1,487
N. Staffs.‡ . .	1958/9	78·1	21·9	—	—	—	689
Nottingham .	1938/9	39·3	42·3	16·5	1·4	0·5	582
	1958/9	42·1	35·1	13·7	—	9·1	2,367
Oxford . .	1938/9	81·4	11·4	0·9	4·6	1·7	5,023
	1958/9	73·2	20·3	1·8	3·5	1·3	8,699
Reading . .	1938/9	39·0	16·6	—	—	44·3	584
	1958/9	46·7	27·2	—	—	26·1	1,425
Sheffield . .	1938/9	27·5	14·0	27·0	31·6	—	767
	1958/9	27·5	25·4	29·6	17·5	—	2,715
Southampton .	1938/9	63·1	26·1	10·8	—	—	268
	1958/9	47·4	35·8	16·9	—	—	1,446
England . .	1938/9	43·6	16·1	11·3	27·1	1·8	37,456
	1958/9	44·2	23·4	14·8	15·8	1·8	77,177
Wales . .	1938/9	56·0	20·1	6·6	11·1	6·3	2,779
	1958/9	46·3	31·8	10·9	5·1	5·8	5,851
Scotland . .	1938/9	46·2	11·4	8·8	31·6	2·0	10,034
	1958/9	36·7	20·2	17·5	23·9	1·8	17,176
Gt. Britain . .	1938/9	44·8	15·4	10·6	27·2	2·1	50,269
	1958/9	43·1	23·4	16·5	15·0	2·1	100,204

† The 105 students at Leicester in 1938/9 include 23 postgraduate students in the Teachers' Training Course. A published figure of 82 (University Grants Committee, 1948, p. 103) excludes them.

‡ All students (202 in 1958/9) following the 1st year general course at N. Staffs. are classified to Arts.

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TABLE 2

University expansion by faculties

A 1948/9 full-time students as percentage of 1938/9 full-time students
B 1958/9 full-time students as percentage of 1948/9 full-time students

		Arts	*Pure science*	*Technology*	*Medicine etc.*	*Agriculture*	*All*	*Pure science and technology*
Birmingham	A	225	352	261	153	—	227	298
	B	109	137	144	103	—	121	141
Bristol	A	245	324	234	137	8†	223	297
	B	136	136	175	124	1,000†	138	145
Cambridge	A	128	143	116	98	270	131	134
	B	109	131	133	131	46	114	132
Durham	A	242	344	295	178	309	249	318
	B	123	131	123	90	84	116	128
Exeter	A	206	303	—	—	§	228	297
	B	126	134	—	—	—	128	134
Hull	A	464	721	—	250†	—	475	502
	B	193	174	—	—	—	185	174
Leeds	A	219	232	254	106	133	183	243
	B	148	140	182	96	103	141	163
Leicester	A	602	469	—	—	—	541	469
	B	199	164	—	—	—	185	164
Liverpool	A	183	191	194	122	—	161	193
	B	115	204	73	89	—	111	125
London	A	151	147	122	104	§	128	137
	B	123	144	161	107	129	124	150
Manchester	A	232	250	189	133	—	204	230
	B	102	113	162	83	—	107	126
,, C. of S. & T.	A	—	—	251	—	—	251	251
	B	—	—	167	—	—	167	167
North Staffs.								
Nottingham	A	365	264	294	—	9,367‡	352	272
	B	119	128	115	—	77	116	124
Oxford	A	134	203	179	153	248	145	201
	B	116	152	183	85	52	119	154
Reading	A	170	185	—	—	158	167	185
	B	172	217	—	—	91	146	217
Sheffield	A	312	353	237	157	—	249	277
	B	113	183	164	125	—	142	172
Southampton	A	260	359	693	—	—	333	457
	B	156	206	121	—	—	162	168
England	A	169	206	186	116	275	164	200
	B	123	145	145	103	77	125	145
Wales	A	162	240	271	105	267	185	247
	B	108	139	128	91	72	114	136
Scotland	A	151	203	283	143	309	169	238
	B	90	148	119	90	50	101	133
Gt. Britain	A	165	208	205	122	280	166	207
	B	116	145	138	100	71	120	142

† Negligible numbers.
‡ Negligible numbers in 1938/9.
§ Exeter none in 1938/9, 5 in 1948/9, none in 1958/9.
London none in 1938/9, 177 in 1948/9.

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(Murray, 1958, p. 394). Moreover, a scrutiny of the individual universities throws further light on the matter. The percentage of arts students was greater in 1958/9 than in 1938/9 at the predominantly "non-arts" universities (except Sheffield where the percentage was unchanged and Birmingham) and smaller at the predominantly "arts" universities (except Hull and Leicester, both very small university colleges before the war). The same is broadly true if 1948/9 is compared with 1938/9. It is thus tempting to conclude that the post-war expansion of student numbers and the

TABLE 3

Full-time graduating and first diploma students entering for the first time

		1958/9 *Number*			1955/6–1958/9 *% Increases*		
		Arts	*Pure science*	*Technology*	*Arts*	*Pure science*	*Technology*
England .	Men	7,037	4,108	3,285	24	27	33
	Women	3,255	1,181	74	Nil	17	106
Wales .	Men	475	466	216	32	32	73
	Women	370	146	—	16	24	—
Scotland .	Men	815	653	1,000	25	37	43
	Women	833	247	15	9	17	150
Gt. Britain	Men	8,327	5,227	4,501	25	29	36
	Women	4,458	1,574	89	3	17	107

greater emphasis on science and technology has been consistent—at least so far—with what may be tentatively regarded as a better balance of the faculties amongst the English universities. Such a judgement, however, would be superficial. The rise in the proportions of students following arts courses at London, Durham and the larger civic universities is mainly a consequence of the marked fall in the *relative* importance of medicine, dentistry and veterinary science. This statement cannot be made without explicitly noting that to combine medicine, dentistry and veterinary science is to conceal important differences in the trends between 1948/9 and 1958/9 in student numbers in these three faculties themselves. Medicine, overwhelmingly the largest of the three, had (1,330) fewer students in 1958/9 than in 1948/9, whereas dentistry and veterinary science had more (396 and 859 respectively). In the late 1940's the extra-mural dental schools in Edinburgh and Glasgow and the veterinary schools at London and Glasgow affiliated with their local universities and new veterinary schools were established at Cambridge and Bristol.

The number of students in science and technology has in general increased more than the number of students in arts. Between 1938/9 and 1948/9 (as Table 2 shows) it rose by 100 per cent. and between 1948/9 and 1958/9 by 40 per cent. The corresponding increases in arts were 70 per cent. and 20 per cent. There were, nevertheless, a few English universities at which the relative increases in arts both immediately after the war and over the last decade were about equal to the increases in pure science and technology. Though it might be natural to think so in view of the emphasis now placed on science and technology, the recent relative rates of increase of students in arts and in pure science and technology are not out of line with experience over the whole of the

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decade 1948/9–1958/9. Between 1955/6 and 1958/9 arts expanded by 18 per cent. and pure science and technology by 30 per cent. Furthermore, the figures in Table 3 for full-time students entering for the first time (of whom an analysis by faculties has been available since 1954/5) show the same pattern. But they also show that amongst women the relative increases in science and technology students greatly exceed the relative increase in arts students, though of course the number of women who study technology is negligible.

2. The Increase in the Number of Students

Table 2 further shows that what may be called the "ex-servicemen's expansion" after the war was almost everywhere proportionately greater than the increases over the subsequent decade. But to leave it at that would be to miss a significant point. When in 1948/9 the universities were coping with the ex-service students, few people would have predicted that 10 years later every single university would have more students in full-time attendance. As the ex-servicemen completed their courses numbers did fall in each of the next three successive sessions from a peak in 1950/1. In early 1953 the University Grants Committee reasonably assessed the future: "There are good reasons for assuming that the rapid rate of increase in total student numbers which marked the immediate post-war period will not be resumed in the near future: indeed, it is not improbable that there may be some slight fall during the next few years" (University Grants Committee, 1953, p. 19). In 1954/5 student numbers rose and have continued to do so in every ensuing year. The Committee later remarked on its failure to foresee that growing proportions of pupils would remain at school until the age of 17 or over and qualify for admission to universities (University Grants Committee, 1958, p. 74).

Relative expansions have varied widely: just after the war Nottingham and Southampton were well over three times as big as before it, Hull and Leicester nearly five times; the expansions at London, Cambridge and Oxford were comparatively small. The subsequent expansions ranged more narrowly. Hull and Leicester were nearly 90 per cent. bigger in 1958/9 than in 1948/9; numbers at Manchester and Liverpool were not much greater. What is less well known is that the relative rates of growth of the universities over the two decades have not in all cases been consistently high or low. The immediate post-war expansions of Durham and Nottingham were large, their subsequent expansions amongst the smallest. Birmingham affords a similar though less striking case. In Reading, on the other hand, the "ex-servicemen's expansion" was comparatively low, the expansion over the last decade comparatively high. The large universities of London, Cambridge and Oxford have had consistently low relative expansions, as has Liverpool. The small universities of Hull, Leicester and Southampton stand alone in having had relatively high rates of growth over both periods. This is not simply a consequence of their small sizes. The comparable University of Exeter was three times bigger after the war than before; its expansion between 1948/9 and 1958/9 was amongst the smallest.

3. The Distribution of the Increase of Students

The low relative rates of growth of Oxford, Cambridge and London have led to a fall from nearly 50 per cent. in 1938/9 to nearly 40 per cent. in 1958/9 in the proportion of Great Britain's full-time students who attend these institutions, but, simply as a consequence of their size, a large proportion of additional students have been absorbed in them. They in fact took a quarter of the increase in full-time students after the war

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(Table 4). What is even more striking is that they took well over 40 per cent. of the increase between 1948/9 and 1958/9, London itself taking a little over a quarter. Moreover, they have so far absorbed a larger proportion of the 1953/4–1958/9 increase in places created by more young people staying on in the sixth form to seek university

TABLE 4

University shares in the increases in full-time students

Per cent.

	Total students 1938/9	1938/9–1948/9	1948/9–1958/9	1953/4–1958/9	*Total students* 1958/9
Birmingham	2·9	5·4	4·4	4·7	3·9
Bristol	2·0	3·7	5·3	2·3	3·1
Cambridge	11·8	5·6	6·7	5·2	8·8
Durham	3·4	7·6	4·4	4·6	5·0
Exeter	0·8	1·6	1·7	1·5	1·2
Hull	0·3	2·1	4·1	3·6	1·4
Leeds	3·5	4·3	8·4	6·5	4·5
Leicester	0·2	1·4	3·1	2·1	1·1
Liverpool	4·1	3·8	2·2	3·8	3·7
London	26·3	11·0	26·0	14·6	21·0
Manchester	4·2	6·6	2·0	3·9	4·6
,, C. of S. & T.	0·7	1·6	3·8	4·4	1·5
North Staffs.	—	—	—	1·0	0·7
Nottingham	1·2	4·4	2·0	1·8	2·4
Oxford	10·0	6·8	8·9	8·5	8·7
Reading	1·2	1·2	2·8	1·6	1·4
Sheffield	1·5	3·4	5·1	4·0	2·7
Southampton	0·5	1·9	3·5	2·5	1·4
England	74·5	72·1	94·4	76·5	77·2
Wales	5·5	7·1	4·4	7·2	5·9
Scotland	20·0	20·8	1·2	16·2	17·2
Gt. Britain	100·0	100·0	100·0	100·0	100·0
Total increase of full-time students		33,421	15,825†	19,602	
		1938/9	1948/9	1953/4	1958/9
Total no. of full-time students Gt. Britain		50,269	83,690	80,602	100,204
(Index)		(100)	(166)	(160)	(199)

† Excludes 1958/9 students at N. Staffs (689).

admission than they did of the increase created after the war by the ex-service students. These figures in part exemplify the often-noted attraction of these universities, though it must be added that in 1957/8 and 1958/9 Oxford and Cambridge were probably affected more than other universities by the extension—to cover men born in 1939—of the deferment rules which foreshadowed the end of national service. Both these universities had previously been taking many students after national service and in these years squeezed in additional numbers to alleviate hardship.

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The expansion of British universities contemplated for the 1960's is one of 60–70 per cent. This is a rate of growth 3 times greater than that of the previous decade. If the shares of Oxford, Cambridge and London in this projected increase equal their shares in the increase between 1953/4 and 1958/9 they would each be about 40 per cent. greater in 1970 than now, Cambridge and Oxford with 12,500 students each, London with 30,000. Though further expansion of these universities is now in progress or in plan it is very unlikely to reach these proportions. Oxford and Cambridge have experienced decades of growth at something like this rate but not for 70 or 80 years (Saunders, 1947). The shares of the increased number of students taken by other universities must rise. Over the last decade Hull, Leicester and Southampton each took less than 5 per cent. of the additional students. Yet their relative expansions were 60–80 per cent. Even to make equally small relative contributions to the projected expansion they must in ten years transform themselves into institutions about 2½ times their present size. The need for the new universities now being established and for some of those being discussed is obvious. Nor is the problem simply one of the cessation of growth at the larger universities as a whole. Individual departments or schools within them may call a halt and their counterparts at some other universities must expand correspondingly.

The large and unforeseen growth in the demand for university places since 1945 was one of the reasons (the rise in costs was the other) which caused the government in February 1959 to announce an extra £8 mn. for the universities for the last three years of the current quinquennium (1957–62). If the total demand has been unexpected, its distribution amongst the universities must also have been so. Certainly between 1954/5 and 1958/9 the shares of each university in the total increases of students fluctuated strikingly year by year. For example, in 1955/6 London took nearly a quarter of the increase (in Great Britain), in 1957/8 only 7 per cent.; Southampton's share has ranged from 12 per cent. to 1 per cent., Birmingham's from 10 per cent. to 2 per cent. These fluctuations are, of course, the consequences of the interacting decisions of the individual universities and of the decisions each year of the thousands of acceptable candidates for university places. It cannot be exactly foreseen, if and when the larger universities or departments cease to grow, how the smaller universities or departments will be affected.

4. Part-time and "Technical" Students

The variations between the universities in their relative expansions are the result of many factors. There have been differing demands for courses. Some universities have had more space in existing buildings or for erecting additional buildings. Some could make room more easily and free lecturers by shedding other commitments. Presumably because the non-recurrent grants which finance them are not subject to the detailed scrutiny of Parliament (though their control has been given close attention by the University Grants Committee), detailed information on the universities' building programmes is not published; no examination of this crucial aspect of expansion is therefore possible here.

On the shedding of other teaching commitments the figures do allow something to be said. The University Grants Committee's returns give the numbers of students who follow courses not of university standard and who have, presumably, mainly technical examinations in view (Table 5). In 1958/9 there were fewer such students than in either 1938/9 or 1948/9 and the number is being steadily reduced. In recent years (1955/6–1958/9) their number in Great Britain has fallen from 11,031 to 9,903.

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In Scotland they are found only at the Glasgow Royal College of Science and Technology where they now number about 550; in Wales there are none. In 1958/9 they were numerous only at Sheffield and Southampton and at the Manchester College of Science and Technology—where they outnumbered full-time students by four to one.

TABLE 5

Part-time students and students following courses not of university standard

A As percentage of full-time students, 1938/9
B As percentage of full-time students, 1958/9
X 1948/9 students as percentage of 1938/9 students
Y 1958/9 students as percentage of 1948/9 students
No. Number of students in 1958/9

	Part-time students					*"Non-university" students*				
	A	*B*	*X*	*Y*	*No.*	*A*	*B*	*X*	*Y*	*No.*
Birmingham	13	4	108	84	174	9	8	263	92	327
Bristol	13	6	383	36	188	6	—	—†	—	—
Cambridge	—	—	—	—	—	—	2	—	∞	205
Durham	36	4	67	45	185	22	5	76	88	249
Exeter	33	6	166	34	79	50	—	—‡	—	—
Hull	n/a	4	n/a	86	54	n/a	1	n/a	100	10
Leeds	23	4	113	44	201	3	—	76	—	—
Leicester	n/a	14	n/a	760	152	n/a	—	n/a	—	—
Liverpool	22	9	96	79	337	—	—	—	—	—
London	48	38	130	97	7,986	2	4	325	108	899
Manchester	16	13	218	80	601	20	2	125	20	103
,, C. of S. & T.	22	7	244	54	102	1,741	436	141	75	6,478
North Staffs.	—	—	—	—	—	—	—	—	—	—
Nottingham	60	8	43	124	186	374	—	—§	—	—
Oxford	—	—	—	—	—	—	—	—	—	—
Reading	5	2	85	118	26	135	10	31	56	137
Sheffield	46	12	65	138	315	326	22	82	28	586
Southampton	44	3	51	78	47	609	25	150	15	366
England	26	14	126	89	10,633	40	12	105	60	9,360
Wales	13	4	75	84	232	2	—	—‖	—	—
Scotland	36	29	168	84	5,014	8	3	136	51	543
Gt. Britain	27	16	135	87	15,879	31	10	106	60	9,903

† In 1938/9 Bristol had 65 students following courses not of university standard.
‡ In 1938/9 Exeter had 210 students following courses not of university standard.
§ In 1938/9 Nottingham had 2,178 students following courses not of university standard.
‖ In 1938/9 University of Wales had 66 students following courses not of university standard.

Their considerable reduction since the war has facilitated at these universities the increase of full-time degree students. Nottingham and Exeter had eliminated them before their large expansions immediately after the war. The large expansions of Hull and Leicester since the war owe nothing to the shedding of such work.

In contrast, the universities (with the possible exceptions of Durham and Exeter) have not been materially helped in taking more full-time degree students by an

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accompanying fall in the number of part-time students. In 1958/9 part-time degree students were not much more numerous than just before the war or much less numerous than just after. It is the relative not the absolute numbers of such students which have been markedly affected by the greater ease with which intending students can now secure grants for full-time study. At the same time the composition of part-time students has notably changed. In 1948/9 in England (in Scotland part-time students have always been overwhelmingly occasional students, in Wales mainly) nearly a half were doing advanced work, a proportion over twice as high as before the war and considerably higher than in 1948/9. It is largely the facilities which London—both the town and its University—provides which make part-time advanced work possible. Three-quarters of the students so engaged are at London; the proportion was only a little lower before the war.

5. Post-graduate Students

The prestige and interest of academic life depend to some extent on the opportunities for advanced teaching and work. Between 1938/9 and 1958/9 there was a substantial rise in the number of full-time advanced students. Whilst the number of full-time first degree students doubled, full-time advanced students quintupled (3,094 to 14,839 in Great Britain). This fivefold increase is partly a consequence of the change in the treatment of students taking Teachers' Diplomas in Education who before 1950/1 were not, as they are now, classified as advanced students. But even leaving such students out of account, full-time advanced students were over 3½ times more numerous in 1958/9 than in 1938/9. The University Grants Committee's returns do not distinguish advanced students reading for higher degrees and those reading for higher diplomas, now including the fourth-year Teacher's Diploma in Education, but they do distinguish the number of higher degrees and higher diplomas actually awarded, some of course to part-time students. Even allowing for the reclassification of Teachers' Diplomas, the number of higher diplomas increased greatly. The increase in higher degrees awarded—as in first degrees—has been approximately twofold (from 1,480 to 3,088).

Table 6 analyses changes at the English universities in the number, distribution and relative importance of higher degrees. Data are combined for two years to obviate distortion arising from year to year fluctuations.

The broadly equal increases of first and higher degrees in part reflect the fact that university growth has so far not involved radical changes in the standards and structure of degree courses. If Britain follows at least some way along the American road and the standard and status of bachelor degrees deteriorate, the relative number of higher degrees is likely to rise. In the United States, in 1956/7, higher degrees were about 22 per cent. of bachelor degrees, in Great Britain about 16 per cent. (Payne, 1960, p. 182). Whatever the final outcome of this issue over the next twenty years the experience of individual universities will no doubt differ. Comparing 1938/9 with 1958/9, there has been a substantial rise in the proportion of higher degrees at Birmingham, Reading, Sheffield and in Scotland, whereas at Bristol, Durham, Leeds, Liverpool and the Manchester College of Science and Technology there has been a fall. These figures, however, refer only to this particular time period: they do not, for example, apply if 1958/9 is compared with 1948/9. To relate higher degrees to concurrently awarded first degrees, when—as now—student numbers are rising rapidly, is somewhat to depreciate the proportion of holders of bachelor degrees who go on to do advanced work: the masters and doctors of one year are largely recruited

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from the ranks of the bachelors of two or three years earlier. Whilst one can be confident that ignoring this lag does not upset broad inferences it does bear differently on pre-war and post-war figures. Just before the war student numbers were comparatively stable and the lag between first and second degrees was therefore immaterial.

TABLE 6

Higher degrees awarded†

	1937/8 *and* 1938/9		1957/8 *and* 1958/9		*Percentage increases*		
	% *of total*	*As %* 1*st degrees*	% *of total*	*As %* 1*st degrees*	1948/9–1949/50 *over* 1937/8–1938/9	1953/4–1954/5 *over* 1948/9–1949/50	1957/8–1958/9 *over* 1953/4–1954/5
Birmingham	3·7	23·2	7·2	28·2	171	160	143
Bristol	1·7	15·6	2·8	11·7	153	151	140
Cambridge	11·9	9·6	11·2	12·6	168	107	106
Durham	4·7	21·0	3·8	11·6	95	158	108
Exeter	0·4	8·3	0·3	3·9	100	109	158
Hull	n/a	n/a	0·3	3·3	n/a	500	127
Leeds	5·0	25·8	3·0	11·5	144	84	99
Leicester	n/a	n/a	0·4	6·5	n/a	400	183
Liverpool	7·4	33·8	4·7	20·1	117	90	121
London	26·9	24·3	24·9	22·0	143	129	101
Manchester	6·1	24·8	6·0	18·5	126	149	105
„ C. of S. & T.	1·5	35·8	1·7	22·5	188	79	153
North Staffs.	—	—	0·2	4·9	—	—	—
Nottingham	0·7	10·7	2·5	12·1	142	393	137
Oxford	12·3	15·1	9·7	15·7	153	101	103
Reading	0·5	7·5	1·3	13·0	236	190	108
Sheffield	0·9	10·3	2·9	18·3	300	195	111
Southampton	0·1	3·3	0·7	6·1	200	213	235
England	83·8	17·9	83·5	16·0	146	123	111
Wales	4·7	11·1	3·8	9·8	116	138	102
Scotland	11·5	8·5	12·7	14·6	126	172	103
Gt. Britain	100·0	15·5	100·0	15·5	143	129	109
Total number	2,943		5,920				

† Excludes higher degrees gained without further study, examination or other tests subsequent to the first degree.

Although the number of postgraduates produced by Oxford and Cambridge is large in absolute terms, the number of higher degrees awarded is not strikingly high expressed in relation to the number of first degrees. Both before the war and now these percentages are below the average and in striking contrast to London. The percentage of all higher degrees awarded by these three universities has recently been distinctly lower than before the war (46 per cent. as compared with 51 per cent.). The decline in the percentage of first degrees awarded by them has over the same period been even sharper (49 per cent. to 41 per cent.) but since 1948/9 the opposite

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has been true: the percentage of first degrees awarded by them has fallen negligibly, the percentage of higher degrees significantly. Whatever may be felt about the proposal that these universities, particularly Oxford and Cambridge, should become increasingly or predominantly postgraduate institutions, it is clearly contrary to recent trends.

6. Overseas Students

In Great Britain, though not in England, the percentage which full-time overseas (Commonwealth and foreign) students composed of all full-time students was about the same—just over 10 per cent.—in 1958/9 as in 1938/9. (It was distinctly lower in 1948/9.) It is another illustration of how individual universities vary that this can be said of only about 5 of the English universities: 3 had noticeably larger proportions,

TABLE 7

Full-time overseas students†

Per cent.

	Total		*Percentage of all full-time students*		*Increase in numbers*		
	1938/9	1958/9	1938/9	1958/9	1938/9–1948/9	1948/9–1958/9	1953/4–1958/9
Birmingham	2·4	3·3	8·7	8·6	14·0	2·7	5·0
Bristol	1·8	2·2	9·3	7·4	4·1	2·5	2·3
Cambridge	13·8	7·9	12·1	9·2	−7·2	3·3	−2·7
Durham	1·8	3·4	5·4	6·9	24·8	1·8	0·3
Exeter	1·2	1·1	15·2	9·3	4·8	0·4	−2·5
Hull	0·1	1·3	2·5	9·4	1·7	2·8	2·1
Leeds	2·8	4·6	8·3	10·4	5·1	6·7	5·8
Leicester	‡	0·4	n/a	4·3	2·1	0·7	0·5
Liverpool	2·4	2·4	6·0	6·6	6·5	1·7	3·3
London	42·2	35·5	16·7	17·3	−27·3	37·9	42·7
Manchester	2·1	2·6	5·1	5·7	3·3	3·1	3·1
,, C. of S. & T.	2·1	2·0	31·4	14·1	−1·3	2·5	2·5
N. Staffs.	—	0·2	—	2·8	—	0·4	0·5
Nottingham	1·0	0·9	9·1	4·1	3·0	0·5	0·6
Oxford	10·8	8·8	11·2	10·4	18·5	4·9	0·3
Reading	0·8	0·7	7·4	5·3	−1·1	0·9	1·5
Sheffield	1·6	1·9	10·7	7·3	1·0	2·5	2·7
Southampton	0·4	0·6	7·8	4·1	1·4	0·7	0·5
England	87·2	80·1	12·1	10·6	53·4	76·0	68·5
Wales	1·2	2·8	2·2	4·9	7·8	4·0	4·7
Scotland	11·7	17·1	6·1	10·1	38·9	20·0	26·9
Gt. Britain	100·0	100·0	10·4	10·2	100·0	100·0	100·0
Total number	5,217	10,200			707	4,276	2,220

† Commonwealth and foreign.
‡ Negative.

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8 noticeably smaller. In 1958/9 there were 4 English universities, including Oxford, where at least 1 in every 10 students was from overseas.

Overseas students are concentrated in London to a much greater extent than all full-time students. Over 40 per cent. of them were there in 1938/9, just over a third in 1958/9. (The post-war percentage in London was lower by about the same number of points as the Scottish universities' percentage—17 in 1958/9—was higher.)

The concentration of overseas students in London can be explained partly by the higher proportion of postgraduates among them than among all full-time students—in 1958/9 42 per cent. compared with 15 per cent.—but the attractions of London and the presence there of large numbers of compatriots are probably the most important reasons. It seems legitimate to doubt whether so heavy a concentration is academically desirable particularly as, in spite of the contrast between 1938/9 and

TABLE 8

Students in selected branches of study: honours degrees and advanced work

Per cent.

Main subject	*Honours degrees obtained*						*Advanced† work current*					
	Men			*Women*			*Men*			*Women*		
	38/9	48/9	58/9	38/9	48/9	58/9	38/9	48/9	58/9	38/9	48/9	58/9
Languages	22	19	15	50	40	39	8	9	7	22	18	20
History	12	12	8	14	15	12	5	5	3	11	10	6
Geography	5	5	4	6	7	7	1	1	1	1	3	1
Social sciences	4	8	8	4	6	7	8	7	8	8	7	15
Law	9	8	8	1	1	2	4	4	4	1	1	3
Mathematics	4	4	4	6	5	4	3	4	3	3	2	2
Biological sci.‡	2	2	4	6	6	8	9	5	7	15	16	13
Chemistry	8	8	9	2	3	3	22	17	15	11	8	5
Physics	4	6	8	1	1	3	7	11	8	4	5	2
Other natural sci.	9	6	5	6	5	4	2	2	3	—	—	—
All natural sci.	23	22	26	16	16	18	40	35	33	30	29	21
Aeronautical eng.	—	—	1	—	—	—	1	1	—	—	—	—
Chemical eng.	—	1	2	—	—	—	2	2	4	—	—	—
Civil eng.	1	2	2	—	—	—	1	2	3	—	—	—
Electrical eng.	2	2	3	—	—	—	2	2	3	—	—	—
Mechanical eng.	1	2	3	—	—	—	2	2	2	—	—	—
Other technology	6	6	6	—	—	—	6	7	8	2	1	1
All technology	11	13	16	—	—	—	14	15	20	2	2	1
Medical subjects	3	1	3	1	2	3	6	9	10	7	10	16
Miscellaneous	6	8	9	3	8	9	10	10	11	14	18	15
	100	100	100	100	100	100	100	100	100	100	100	100
Total number	3,995	8,037	9,920	1,267	2,291	3,459	2,661	5,478	9,664	369	793	1,406
(Index)	(100)	(201)	(249)	(100)	(181)	(272)	(100)	(206)	(362)	(100)	(214)	(382)

† Full-time students only, excluding education.
‡ Includes biochemistry.

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1958/9, it may not now be diminishing: between 1953/4 and 1958/9, 43 per cent. of the increase in overseas students went to London.

7. Fields of Study: Honours and Advanced Work

The University Grants Committee's classification by subject of honours degrees awarded and the current work of advanced students has been condensed in Table 8 to show the pattern before and since the war. Some arbitrary classificatory decisions have been made: for example, biochemistry has been included in the biological sciences, psychology excluded from both the social and natural sciences. Advanced work in education has been excluded altogether. The advanced work is, moreover, that of full-time students only. Part-time work has always been a more important avenue to post-graduate qualifications than to honours degrees: in 1958/9 a little under 30 per cent. of the men and a little over 30 per cent. of the women doing advanced work were part-time students; just before and just after the war the figure was 40 per cent. for both. The pattern of part-time advanced work is not, however, greatly different from the pattern of full-time advanced work except that the share of medicine is much greater.

Languages are relatively less important amongst men than amongst women, technology is overwhelmingly more so. The difference between the sexes in relation to the natural sciences is not so great, largely because the biological sciences attract a good percentage of women students. The most conspicuous change is the decline, common to both men and women, in the proportion of honours degrees in languages, a decline which has not, however, been reflected in advanced work. Notably, too, the natural sciences and technology claim a larger share of advanced work than of honours degrees, but although both have increased their share of honours degrees (technology more than the sciences), in advanced work the share of the sciences has declined in contrast to the share of technology, almost wholly because of the fall in the proportion of chemistry students.

8. Student Residence

A major problem of university expansion is to find suitable living quarters for students. This problem has been complicated since the war by the great fall, except in Scotland, in the proportion of students, men and women alike, living at home. Before the war the proportion was about 40 per cent., in 1958/9 it was half this. The proportion of students in hall is much the same—again for men and women alike—but there has been a great increase in the proportion in lodgings. The most important reason for this is the more ready availability of student grants (Butler, 1960). Every English university, except Oxford and Cambridge, has experienced the same trends, and so has the University of Wales. Before the war there were relatively few Oxbridge students living at home: the same is true now. At least in this respect the other universities have become more like the two ancient universities, although none (except N. Staffs.) has so small a proportion living at home. There are still six English universities where a quarter or more of the students are living at home. Before the war, the proportion exceeded a half at seven universities.

Most universities have maintained the proportion of students in college or hall at something like the pre-war level. In Birmingham, Liverpool, London and in Scotland it was actually higher in 1958/9 than in 1938/9. It is only at the small universities of Exeter, Hull and Southampton that the situation has been radically transformed, though their proportions are still higher than average.

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TABLE 9

Residence: full-time students

Percentage in

	College or Hall				*Lodgings*				*Home*			
	Men		*Women*		*Men*		*Women*		*Men*		*Women*	
	38/9	58/9	38/9	58/9	38/9	58/9	38/9	58/9	38/9	58/9	38/9	58/9
Birmingham	10	10	25	19	27	65	14	62	63	25	61	20
Bristol	31	22	56	38	24	67	9	53	45	11	36	10
Cambridge	46	52	88	83	52	47	8	16	1	1	4	1
Durham	29	31	34	41	13	45	9	33	58	25	57	27
Exeter	79	44	85	64	10	51	2	35	11	5	13	2
Hull	66	30	78	58	2	61	—	38	33	9	22	4
Leeds	14	10	44	37	26	72	10	50	60	18	46	13
Leicester	n/a	48	n/a	65	n/a	47	n/a	31	n/a	6	n/a	4
Liverpool	6	8	26	35	27	53	7	29	67	39	68	36
London	6	12	28	33	45	55	29	42	48	33	43	26
Manchester	16	12	39	32	12	59	6	44	72	29	55	25
„ C. of S. & T.	15	6	—	19	34	66	—	47	51	28	100	34
N. Staffs.	—	94	—	100	—	6	—	—	—	—	—	—
Nottingham	36	26	55	56	29	67	1	41	36	7	44	3
Oxford	58	46	74	72	42	53	23	25	—	1	3	3
Reading	66	41	82	78	19	47	2	18	15	12	15	4
Sheffield	18	15	40	23	22	65	3	66	60	20	57	11
Southampton	59	36	67	32	1	55	—	62	40	9	33	7
England	27	26	43	43	38	56	18	40	36	19	39	17
Wales	6	17	52	45	46	65	8	39	48	19	41	16
Scotland	6	10	16	21	33	39	25	33	61	52	59	47
Gt. Britain	22	23	37	39	37	53	19	38	41	24	43	23
Total number Gt. Britain	8,298	17,103	4,368	9,507	14,372	40,412	2,230	9,288	15,824	18,364	5,064	5,530

Part II

STAFF

9. STUDENT-STAFF RATIOS

The universities' student-staff ratio (the number of full-time students per full-time members of staff) has improved since the war (Murray, 1958, p. 396). Though comparisons are obviously more dubious the longer the period, the figures in Table 10 suggest that the 1958/9 ratio is also better than the ratio just before the war. The improvement has covered almost all the individual universities: all but three (Hull, the Manchester College of Science and Technology, and Southampton) have lower student-staff ratios than in 1948/9; in Scotland the improvement over the same period has been very marked. Six of the eleven universities for which continuous figures are available appear to be better off than before the war.

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TABLE 10

Student-staff ratios†

	1938/9	1948/9	1958/9
Birmingham	9·0	8·8	7·2
Bristol	6·4	8·7	7·3
Cambridge	(om)	(om)	(om)
Durham	9·5	10·3	8·8
Exeter	7·7	11·2	9·1
Hull	3·4	7·1	9·0
Leeds	7·1	7·0	6·9
Leicester	n/a	8·7	8·4
Liverpool	10·8	11·1	8·2
London	12·5	8·5	7·1
Manchester	10·2	10·3	7·5
„ C. of S. & T.	4·3	7·2	7·7
N. Staffs.	—	—	7·0
Nottingham	7·2	9·8	8·4
Oxford	(om)	(om)	(om)
Reading	4·5	5·6	4·6
Sheffield	6·0	7·9	7·8
Southampton	4·6	7·1	8·8
England	8·9‡	8·7‡	7·4‡
Wales	7·5	10·2	7·8
Scotland	14·0	13·0	8·4
Gt. Britain	10·1‡	9·6‡	7·6‡
Number of full-time staff G.B.‡	3,867	7,149	10,823
Number of full-time students G.B.‡	39,210§	68,608	82,661
Number of full-time staff G.B.‖	5,210	8,150	12,234
Number of full-time students G.B.‖	50,164§	83,690	100,204

† No. full-time students ÷ No. full-time staff.
‡ Excluding Oxford and Cambridge.
§ Excludes Leicester (105).
‖ Including Oxford and Cambridge. The Oxbridge staff figures are partial, including professors, readers and also lecturers, etc. combining university and college work.
(om) = omitted.

Student-staff ratios are notoriously difficult to interpret. Faculty structures differ markedly from one university to another, and these differences affect student-staff ratios. For example, data which have been supplied privately by the University Grants Committee and which exclude Oxford and Cambridge show that in 1958/9 the student-staff ratio in science and technology was 7·9 as compared with 8·6 in arts. The persistence of Reading's apparently favourable student-staff ratio is largely due to the prominence of agriculture and agricultural research, which employ a large number of full-time academics with minimal teaching duties. There are, moreover, divergencies of policy in staffing particular faculties and departments at different universities, as well as variations in their sizes. Some full-time academics, like those in adult education,

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rarely teach full-time students; some may be obliged to undertake the instruction of part-time students; others have their duties in teaching full-time students lightened by the assistance of part-time lecturers. Account must also be taken of the extent of advanced teaching and supervision: post-graduate instruction usually requires more preparation and intimate communication than undergraduate instruction, though advanced students can often help by undertaking some of the teaching of (say) first-year students. Universities (and faculties and departments) can differ in all these respects. Moreover these differences can change with time: the improvement between 1948/9 and 1958/9 in London's ratio is partly accounted for by the development there of the British Postgraduate Medical Federation.

Faculty and departmental differences between the universities in any one year are almost certainly greater than the differences within single universities at the beginning and end of the decades under examination, but some doubts arise if part-time and "technical" students are considered. To what extent part-time students add to the teaching duties of full-time staff is impossible to say: part-time staff (the numbers of which go unrecorded in the University Grants Committee's returns) may be appointed to take over much of the work. As shown earlier, the trend in the proportion of part-time students has been sharply downwards, but whilst the direction of the change at each university has been similar its extent has varied widely. In this respect the differences over time at individual universities must be as great as, perhaps greater than, between the universities in single years. A very rough assessment, however, suggests that comparisons of student-staff ratios over time are not vitiated by changing trends in part-time students. If all students, whether full or part-time, are counted as equivalent (a procedure which ignores, amongst other things, the rise in the proportion of advanced students amongst part-timers), one finds that the order of the universities in respect to their student-staff ratios is hardly affected (only the position of London is radically changed) and it still remains true that at only 3 universities did student-staff ratios worsen between 1948/9 and 1958/9—Hull, Southampton and Leicester—not this time the Manchester College of Science and Technology. Students following courses not of university standard are not instructed exclusively by full-time academics, but one must reckon that the reductions of their numbers, which have been noted earlier, offset to some indeterminable extent the rise between 1948/9 and 1958/9 in the student-staff ratios at Southampton and the Manchester College of Science and Technology. Of course, if such students are taken into account, these two universities, and more particularly Sheffield, have higher absolute student-staff ratios than appear on the basis of full-time students alone.

For all these reasons it is useful to put the student-staff position another way. Table 11 gives the percentages which each English university's full-time staff bears to its "national-average staff", i.e. what its staff would be if each of its faculties (as distinguished by the University Grants Committee) had the average student-staff ratio for that faculty in all English universities. These percentages would all be 100 per cent. if the student-staff ratio in a given faculty were the same at every university and the student-staff ratios of individual universities differed simply because faculties were combined differently. In fact the percentages vary widely and their order is very similar to the order of the aggregate student-staff ratios. Only the position of the Manchester College of Science and Technology is markedly different; the College, as is to be expected from the large number of students following courses below university standard, has a lower student-staff ratio (on the basis of full-time students only) than most other faculties of technology. One may therefore conclude that it is not

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differences in broad faculty structure which are mainly responsible for the relative positions of the universities in respect to student-staff ratios; there are considerable differences amongst the universities in the student-staff ratios of individual faculties. Information supplied privately by the University Grants Committee confirms this. At the 10 English universities (excluding Oxford and Cambridge) which possess arts, science and technology faculties (some having other faculties as well), there is a tendency for comparatively low ratios in one of the three faculties to be associated with comparatively low ratios in the other two.

TABLE 11

Actual full-time academic staff as percentage of "national average" staff†

	1953/4	1958/9
Birmingham	105	110
Bristol	99	104
Cambridge	(om)	(om)
Durham	91	85
Exeter	89	86
Hull	132	86
Leeds	129	110
Leicester	103	93
Liverpool	89	92
London	97	103
Manchester	99	102
,, C. of S. & T.	163	118
N. Staffs.	123	113
Nottingham	75	79
Oxford	(om)	(om)
Reading	113	112
Sheffield	103	102
Southampton	93	90

† It is assumed that at each university each faculty has the average student-staff ratio for that faculty in all English universities except Oxford and Cambridge. The University Grants Committee distinguishes arts, pure science, technology, medicine, dentistry, agriculture and forestry, veterinary science. Universities differ in the number of faculties which they possess.

(om) = omitted.

Over the three periods (1938/9–1948/9, 1948/9–1953/4, 1953/4–1958/9) the figures reveal only a weak—though persistent—tendency for staff to increase relatively most at universities where students have increased relatively more. Perhaps this is more to be expected. For a number of reasons it is not easy—and this is very relevant to the last of the three periods—for universities to achieve increases of staff commensurate with very large sudden increases of students: certainly the universities (the Manchester College of Science and Technology, Hull, Leicester and Southampton) where expansion just before 1958/9 was greatest did not achieve it. The future increase of students is uncertain in total and in distribution amongst the universities. Academic planning and the concomitant allocation of government funds—now the main source of income for all universities—is quinquennial and there is some inter-quinquennial inflexibility. The figures here, however, do not suggest that any university has so far suffered a

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disastrous worsening of its student-staff ratio. Each university is, of course, to some extent master of its own fate; if it cannot appoint more staff it can limit its intake of students, although the political and social arguments for expanding may be impossible to resist entirely.

To maintain a constant student-staff ratio a university has to maintain the same ratio between additional students and additional staff. For short periods of a year or so this constancy is unlikely to be observed even if it were the broad long-term aim;

TABLE 12

Number of additional students per additional member of staff

	1948/9–1953/4	1953/4–1958/9
Birmingham	− 3·3	8·5
Bristol	+ 3·4	8·8
Cambridge	(om)	(om)
Durham	− 1·6	52·9
Exeter	− 0·9	17·6
Hull	− 2·1	32·0
Leeds	+ 0·4	25·4
Leicester	+ 2·6	13·4
Liverpool	− 6·1	9·0
London	+ 2·4	6·3
Manchester	− 3·7	10·0
,, C. of S. & T.	−37·9	13·7
N. Staffs.	—	16·2
Nottingham	− 0·9	13·3
Oxford	(om)	(om)
Reading	+ 1·4	10·5
Sheffield	+ 0·4	10·6
Southampton	+35·5	13·4
England	+ 0·2†	10·7†
Wales	− 6·8	10·3
Scotland	− 6·6	11·4
Gt. Britain	− 1·7†	10·8†

† Excluding Oxford and Cambridge.
(om) = omitted.

recruitment cannot follow a strict formula. Moreover, during a period like the early 1950's when the number of students is falling after a large temporary expansion the opportunity presents itself of improving student-staff ratios. Table 12 presents the number of additional students per additional member of staff in the two post-war periods, 1948/9–1953/4 and 1953/4–1958/9. In both periods there have been great differences amongst the English universities. Between 1948/9 and 1953/4, however, all universities except Southampton improved their student-staff ratios: the number of additional students per additional member of staff was, except at Southampton, much below the total student-staff ratio. Indeed, there were 8 universities which had fewer students but more staff. In interpreting these differences regard must be paid to

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differences in the rates of growth and the initial sizes of universities. This cannot be illustrated in detail. But the practical relevance of this point at a time when student-staff ratios are tending to worsen is that a small university with a large relative expansion may seem to be doing better in additional staff for additional students than a large university with a small relative expansion, although in fact its aggregate ratio is undergoing a greater deterioration.

Staff, it is widely thought, is likely to be the most important limitation on future university growth. A great increase in the number of university graduates does not mean a larger field from which academics can be recruited: most young people of intellectual distinction, the potential senior members of universities, have long secured university education. They are in future bound to form an even smaller proportion of each year's crop of graduates. Moreover, competing demands for them are probably more numerous and intense than ever. Potential academics may easily be enticed away to other professions. Fortunately, perhaps, the universities are the places of training for the young people concerned; they thus have the first opportunity to allure (or condition) them! It is because they are so close to the natural limits of their field of recruitment that the universities must wish, as they expand, to build up their academic staffs steadily, and to anticipate future increases in students and so try to avoid a deterioration in the quality of staff. Some deterioration may after all be unavoidable. No more decisive argument can be advanced for trying to mitigate it. The smaller universities have especial difficulties. Many potential young academics do not regard them as attractive. They may doubt the reputation of many of their departments and fear that their research facilities are poor or that the cultural or natural amenities of their localities are scanty. If their student-staff ratios rise sharply their attractiveness may be further diminished and their recruiting difficulties intensified. Educationally the consequences may be serious. As the university population expands, the now small universities will have to cater, as they already are doing to some extent, for increasing numbers of students with limited intellectual interests and capacities, who need the close guidance which, without a favourable student-staff ratio, may be impossible.

10. Grades of Staff

The attractiveness of the academic profession depends in part on remuneration and promotion prospects, and these in turn depend partly on the distribution of academics amongst the various grades of the profession. Unfortunately, it is not possible, for 1938/9, to distinguish senior lecturers—who are most conveniently regarded, with professors and readers, as forming the senior grades—from lecturers. Even so, and recognizing that the grade of senior lecturer was much less common before the war than now, it is clear that between 1938/9 and 1958/9 there has been an increase in the proportion of academics in the central grade of lecturer. Correspondingly, and in every faculty, there has been a marked fall in the proportion of professors and readers, and also—though not in agriculture and forestry—in the proportion who are in the junior grades of assistant lecturer, demonstrator, instructor, etc. The combination in Table 13 of veterinary science and dentistry with medicine again conceals differences in these faculties. Dentistry shows most conspicuously a rise in the proportion in junior grades, and in common with veterinary science only a slight rise in the proportion of lecturers.

There is a noteworthy contrast between arts on the one hand and science, technology and medicine on the other. In arts the fall in the proportion of professors and readers

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has been much greater than in the other three faculties, whereas the reverse is true of the proportion in the junior grades. This is not to be explained by the greater relative increase in the number of arts staff. Comparing 1938/9 with 1958/9, staffs in science and technology have grown less but in medicine a great deal more than in arts. In any case, in respect to senior grades, a similar contrast is to be seen over the years 1953/4 to 1958/9, when the position was reversed and staff in science and technology increased

TABLE 13

Distribution of full-time academic teaching staff by grade and faculty, 1938/9, 1953/4, 1958/9

Faculty	*Year*	*Profs.*	*Readers etc.*	*Senior lects.*	*Lects.*	*Asst. lects. and demonstrators*	*Others*	*Total staff* %	*No.*	*Index*
Arts . .	1938/9	27·8	13·0	38·9		15·9	4·4	100·0	1,449	100
	1958/9	16·6	6·9	60·0		10·0	6·5	100·0	3,892	269
Pure science	1938/9	20·4	7·9	40·6		24·0	7·1	100·0	1,260	100
	1958/9	11·7	9·0	58·1		14·1	7·1	100·0	2,780	221
Technology .	1938/9	14·5	6·8	41·9		22·5	14·3	100·0	635	100
	1958/9	10·8	5·8	68·5		7·3	7·6	100·0	1,537	242
Medicine,	1938/9	25·3	6·7	29·9		31·5	6·7	100·0	479	100
dent. & vet.	1958/9	14·1	8·8	54·5		12·9	9·7	100·0	2,352	491
Agriculture	1938/9	9·4	4·1	33·9		17·5	35·1	100·0	171	100
& forestry	1958/9	6·2	2·0	38·1		9·4	44·3	100·0	564	330
All . .	1938/9†	22·3	9·2	38·6		21·4	8·4	100·0	3,994	100
	1958/9†	13·5	7·4	58·4		11·2	9·4	100·0	11,125	279
Arts . .	1953/4	15·0	6·7	6·4	49·8	13·5	8·4	100·0	3,292	100
	1958/9	14·6	5·7	7·7	54·8	10·4	6·8	100·0	3,739	114
Pure science	1953/4	11·1	6·5	9·2	49·6	15·4	8·1	100·0	2,151	100
	1958/9	10·5	7·5	10·6	49·3	14·5	7·4	100·0	2,686	125
Technology .	1953/4	9·5	5·2	11·9	57·1	7·4	8·8	100·0	1,156	100
	1958/9	10·3	5·3	12·8	56·5	7·3	7·7	100·0	1,519	131
Medicine,	1953/4	13·4	7·9	16·6	39·3	11·8	11·0	100·0	2,141	100
dent. & vet.	1958/9	13·5	8·4	19·0	36·2	13·1	9·8	100·0	2,322	108
Agriculture	1953/4	5·9	1·6	6·7	23·8	9·4	52·6	100·0	509	100
& forestry	1958/9	5·7	1·3	6·1	32·5	9·5	44·9	100·0	557	109
All . .	1953/4‡	12·6	6·5	10·1	46·8	12·6	11·4	100·0	9,249	100
	1958/9‡	12·2	6·4	11·5	48·5	11·5	9·7	100·0	10,823	117

† Includes Oxford and Cambridge.
‡ Excludes Oxford and Cambridge.

more rapidly, in medicine less rapidly, than in arts: during these five years the proportion of senior staff in arts remained constant whilst in science, technology and medicine it rose. At the same time the proportion in the junior grades in arts declined more rapidly.

Numerous factors determine the proportion of academics in the various grades in different faculties. How quickly a faculty is expanding is one, but there are others like the nature of the expansion, how easy it is to recruit staff into junior grades, whether there is a view—as at present there is—about what is the right ratio of senior non-professorial staff to total non-professorial staff, and so on. In line with this

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multiplicity of factors there is a good deal of variation amongst the universities in the proportion of senior staff in each faculty. Amongst the English universities it ranged in 1958/9 in arts from about 16 per cent. to 36 per cent., in science from 17 per cent. to 37 per cent., and in technology from 19 per cent. to 39 per cent. Nevertheless—and perhaps not surprisingly—where the proportion of senior staff in a university is relatively high or low in one of the faculties it is on the whole relatively high or low in the others. At the same time the general trend observed earlier is reflected in the experiences of individual universities: in 1953/4 there were 6 English universities where the proportion of senior staff in arts was higher than in science and technology; in 1958/9 only 3.

The proportion of senior staff has been selected for comment as it is frequently singled out as relevant to the promotion prospects of each new generation of academics. If the proportion is kept constant it sets a lower limit to the average proportion of each new generation of academics who reach the senior grades. But the lower limit may be exceeded as the proportion is not the only relevant consideration. Another equally important one is how long the academics who reach the senior grades typically spend in them before retirement. However, the careers of the present generation of young academics will be affected more by the increase in the number of students and universities than by these structural and demographic considerations.

Part III

RECURRENT GRANTS

11. The Rise in Central Government Grants

The gradual extension of the coverage of the University Grants Committee's returns is relevant to a scrutiny of Table 14 which displays certain aspects of the universities' income over the last two decades. For example, between 1938/9 and 1948/9 Hull, Leicester and four constituent institutions of the University of London (Institute of Advanced Legal Studies, Warburg Institute, Wye College and the British Postgraduate Medical Federation) were added to the Committee's grant list. The University College of North Staffs. was founded in 1949. From 1956/7, after the Manchester College of Science and Technology assumed independent status, its *total* income and expenditure were given in the University Grants Committee's returns and not 60 per cent. (the proportion relating to university work) as in previous years. By 1958/9 the list of the University of London's faculties and schools receiving grants had lengthened further. Thus some part of the rise in income since the war (as of the rise in students) is to be ascribed to the increased number of institutions receiving grants: about £3½ mn. of 1958/9 income (as defined in Table 14). Still, broadly speaking, in 1958/9 the universities' total income was eight times what it was in 1938/9 and, since there were almost exactly twice as many students, expenditure per student was about four times greater. At a rough estimate and allowing for increased numbers of grant-receiving institutions, this provision adequately compensated for the nearly threefold rise in the price level in the intervening period and also kept the universities abreast of the rise of about 20 per cent. in the country's standard of living. Estimates of the income the universities would have needed in the selected years to allow for price changes, increased numbers of students and improvements in the standard of living since 1938/9 are given in Table 14. In interpreting them it must be remembered that they are based—arbitrarily—on 1938/9 and that changes in the price-level and

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standard of living are not uniquely measurable concepts. In comparison with them the actual income of 1953/4, like that of 1958/9, seems adequate but not that of 1948/9 when the universities were improvising to accommodate the "ex-servicemen's bulge". The (formal) reason for the contrast is plain; recurrent Treasury grants, though in 1948/9 five times what they were in 1938/9, had not increased sufficiently to offset the "stickiness" of the universities' income from other sources. Over the twenty years the rise in university income other than direct subventions from the government has only

TABLE 14

University income in Great Britain; selected years

	Total income†	*Income other than Parliamentary grants*‡	*Recurrent Treasury grants*‡	*Consumers' prices approx.*§	*Consumers' expenditure per head at constant prices*‖	*Students*†	*"Standard income"*¶
	£*mn.*	£*mn.*	£*mn.*	1938/9 = 100			£*mn.*
1938/9	6·7	4·3	2·1	100	100	100	
1948/9	18·2	7·4	10·3‡‡	189	98	166	20·6
1953/4	31·1	9·2	21·0§§	232	105	160	26·1
1958/9	48·6††	12·2	35·5§§	267	117	199	41·7

† Between 1938/9 and 1958/9 the number of institutions receiving grants increased: see text.

‡ Recurrent Treasury grants though the chief (in 1958/9 97·5 per cent.) are not the only Parliamentary grants.

§ Based on Consumers' Expenditure Average Value Index. London and Cambridge Economic Bulletin.

‖ Based on Consumers' Expenditure at Constant Prices. London and Cambridge Economic Bulletin.

¶ Rough estimates of income necessary to allow for rises in prices, student numbers and real consumption standards since 1938/9.

†† 1958/9 income excludes "Payments for Research" which, in the earlier years, the U.G.C. did not specifically require the universities to give in their income returns. Some universities did in fact include them under other heads. 1953/4's income of £31·1 mn. to be strictly comparable with the figure of £48·6 mn. given for 1958/9 should be reduced (by about £$\frac{3}{4}$ mn.?) (University Grants Committee, 1958, p. 70).

‡‡ After the war the Treasury assumed the financial responsibilities of the Ministry of Agriculture and Fisheries and the Department of Agriculture for Scotland in respect of higher agricultural education and the training of veterinary surgeons (University Grants Committee, 1953, p. 70).

§§ From 1951/2 recurrent grants covered *replacements* of furniture and equipment—previously covered by non-recurrent grants. In the first year of change recurrent grants were thereby increased £$\frac{3}{4}$ mn. (University Grants Committee, 1953, p. 68).

kept pace with the fall in the value of money; it has made no contribution to finance the increase in student numbers. Just after the war few could foresee how dependent on direct help from the central government, inflation and the increase in students would make the universities nor was it a constitutionally attractive prospect (University Grants Committee, 1953, p. 75).

The proportionate increases in recurrent grants over three periods at the individual universities are given in Table 15. On comparing them with increases in full-time students, it emerges that it is only over the last of the three periods (1953/4–1958/9) that recurrent grants on the whole increased most where students increased most. In

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TABLE 15

University expenditures† and recurrent Treasury grants per full-time student

	1938/9			1948/9			1953/4			1958/9			
	Grant £'000	£ *Per student*		*Grant as % 38/9*	£ *Per student*		*Grant as % 48/9*	£ *Per student*		*Grant as % 53/4*	£ *Per student*		*Grant £'000*
		Grant	*Expen.*		*Grant*	*Expen.*		*Grant*	*Expen.*		*Grant*	*Expen.*	
Birmingham .	87	61	145	549	145	200	180	283	342	184	399	473	1,573
Bristol . .	66	66	168	512	151	231	205	264	378	154	346	529	1,066
Cambridge‡ .	121	20	(om)	583	91	(om)	198	179	(om)	172	272	(om)	2,403
Durham . .	86	50	123	493	100	157	220	230	282	172	324	393	1,603
Exeter . .	19	45	111	474	94	147	201	194	216	172	252	300	311
Hull . .	—	—	n/a	—	124	153	204	271	311	177	242	296	343
Leeds . .	82	47	145	511	131	213	205	262	346	173	327	448	1,484
Leicester .	—	—	n/a	—	127	167	204	226	280	197	274	332	289
Liverpool .	98	48	117	438	129	181	193	283	315	164	369	444	1,354
London . .	646	49	120	504	193	267	202	363	439	161	503	596	10,559
Manchester .	94	45	122	589	129	189	190	273	324	160	365	475	1,688
,, C. of S. & T.§	16	45	367	231	42	347	208	123	447	935	484	711	720
N. Staffs. .	—	—	—	—	—	—	—	257	327	184	340	424	234
Nottingham .	32	55	136	556	87	140	239	212	237	168	302	403	716
Oxford‡ . .	108	22	(om)	623	92	(om)	188	180	(om)	173	252	(om)	2,188
Reading . .	42	72	211	393	169	321	182	268	521	165	348	650	496
Sheffield . .	59	77	197	446	138	214	206	281	357	179	357	460	969
Southampton .	20	75	142	555	124	145	199	229	282	187	286	363	413
England . .	1,573	42	121	527	135	206	201	268	349	170	368	474	28,412
Wales . .	158	57	122	315	97	149	201	225	281	168	287	383	1,678
Scotland . .	347	35	88	427	87	134	223	236	292	163	314	399	5,386
Gt. Britain .	2,078	42	114	494	123	188	204	260	336	169	354	456	35,476
Total expenditure .	£5·7 mn.			£15·8 mn.			£27·0 mn.			£45·7 mn.			
Grant . .	£2·1 mn.			£10·3 mn.			£21·0 mn.			£35·5 mn.			

† Expenditure excludes "Miscellaneous expenditure" (U.G.C. Returns 1953/4, 1958/9) or "Other maintenance expenditure" (1938/9, 1948/9), "Capital expenditure met from income" and "Allocations to reserves".

‡ Published income and expenditure of Oxford and Cambridge are not comparable with those for other universities since a substantial part of the expenditure on teaching and administration is defrayed by the colleges. This warning presumably has less force against comparisons over time. The England and Gt. Britain figures incorporate published Oxford and Cambridge data. As averages, therefore, they are too low.

§ The 1958/9 figures for the Manchester College of Science and Technology are not comparable with the figures for previous years. On August 1st, 1956, the college assumed independent status. Before that the published figures on income and expenditure represented 60 per cent.—the proportion relating to university work—of the total.

(om) = omitted.

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neither of the other two periods is there any association either way. Admittedly, comparisons between single years, even five or ten years apart, can be misleading. Similar comparisons were therefore made using average student numbers and recurrent Treasury grants for three pre-war years (1936/7–1938/9) and the first and second university quinquennia after the war (1947/8–1951/2 and 1952/3–1956/7). The same conclusion is suggested. It seems unlikely that it can be explained by the changes (shown in Table 14) in the nature of recurrent grants, even though these changes would not affect all universities in the same proportion. There are in any case general grounds for thinking that the University Grants Committee may have changed its emphasis in allocating public money to the universities.

Money does not go equally far in providing a university education for all students. "Non-arts" students are nearly twice as expensive to educate as "arts" students (Murray, 1958, p. 407). They normally have both more teachers (who need more technical assistants) and more equipment. Aggregate expenditure figures (for 1958/9) reflect this fact. Amongst the English universities the smaller the proportion of arts students the higher, on the whole, is expenditure per student. Obviously the allocation of increased recurrent grants to individual universities must be made with some regard to how the additional students are distributed amongst the faculties. If the preceding calculations are reworked giving "non-arts" students twice the weight of arts students, the positive association of proportionate increases in Treasury grants and proportionate increases in full-time students between 1953/4 and 1958/9 emerges more definitely than before. This double-weighting for non-arts men does not, however, make any difference to the lack of association in the earlier periods.

No sensible person would wish to contend that the size and composition of the student body should be the sole criteria for allocating additional grants to the universities. The increases in students expected in future are also relevant, as are actual and expected changes in the costs of maintenance of university facilities like equipment, libraries and halls of residence. Nor is "need" the only possible criterion for governing the distribution of public money amongst universities. Allocations in proportion to the help which can be secured from other sources and a university's proven capacity for useful expenditure are two other criteria which presumably influence the University Grants Committee (University Grants Committee, 1948, pp. 71–81). Yet, now that a rapid and state-encouraged expansion is in progress and the universities are so dependent on state funds, increases in numbers, though with regard to their composition, should be the single most important criterion. Even the aggregate figures here strongly suggest that it is. The main reason why the criterion was not followed as closely in the earlier periods was perhaps the very rapidity of the growth in the dependence of the universities on government grants. The allocation of a large proportion of the increase in the grants must have been made to meet the necessities of the moment whether or not they were closely related to increases in numbers of students. Oxford and Cambridge, despite the comparatively small increases in their student numbers between 1938/9 and 1948/9, were amongst the universities whose recurrent grants increased most. Did their relative independence of state funds before the war make the ancient universities particularly vulnerable to the erosion by inflation of the real income from their endowments and make it necessary to grant them relatively large increases in state aid after the war? It is also possible that the University Grants Committee paid less attention after the war to *actual* increases in students than would now be the case, in the belief that the pattern of the increases was not a good guide to how students would be distributed in more normal times.

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The mass of ex-service students was a transitory phenomenon. Now, on the contrary, expansion is expected to be permanent. At the same time there has consistently been amongst the English universities less variation in recurrent grants per student than in total expenditure per student. This feature is likely to persist. Quite apart from the effects of different proportions of students in different faculties some universities will always have readier access than others to funds from local and private sources of one kind or another.

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The Statistics of Accidents Collected by H.M. Factory Inspectorate, Ministry of Labour

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Summary

This note discusses certain features of the statistics collected by H.M. Factory Inspectorate as part of its work for the prevention of accidents, points out some of the limitations of the statistics and explains briefly the approach that has been adopted in planning their collection and processing.

1. Introduction

For the last ten years the number of accidents reported to H.M. Factory Inspectorate from premises subject to the Factories Acts has been between 160,000 and 200,000 each year and of these—except in 1959—more than 600 each year have been fatal. An accident is not notifiable to the Inspectorate unless it has disabled a person from carrying out his normal work for more than three days, so that the problems of industrial accidents are obviously of considerable social and economic importance to the community.

Accident statistics have been collected by the Inspectorate continuously since 1844, but as the field of coverage has been extended and the legal basis of collection changed on various occasions since then, no continuous comparisons of accident figures can be made. Estimates of numbers employed in premises under the Acts have been made periodically, however, so that an approximate comparison of accident rates can be derived.

There are in Great Britain several independent sets of national statistics of industrial injuries, namely those collected by the Ministry of Pensions and National Insurance in the administration of the industrial injuries insurance scheme and those collected by H.M. Factory Inspectorate, H.M. Inspectors of Explosives, H.M. Inspectors of Mines and the Ministries of Transport and Agriculture, of accidents within their respective spheres. The methods of compilation and the legislative backgrounds of these sets of statistics differ fundamentally and little comparison is possible between them even where they appear to cover the same or overlapping fields. The differences between the accident statistics of the Factory Inspectorate and those of the Ministry of Pensions and National Insurance are the subject of an article in the *Ministry of Labour Gazette* for April 1961.

2. The Data

The basis of the Inspectorate's statistics is the notice which must be sent to H.M. District Inspector of Factories when any reportable accident has occurred in any premises or place subject to the Factories Acts (i.e. factories, docks, building operations, works of engineering construction, warehouses, etc.). The notice is submitted on a prescribed form (there are two forms in use, one for constructional work, and

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one for all other premises) and the form is designed to give the District Inspector adequate information on the circumstances and result of the accident. It is not primarily designed for statistical purposes. The forms necessarily must not be complicated, and as they are used for all types of accidents, on a wide variety of work, the questions can rarely be specific. The types of answer, and the precision and detail given, vary enormously, so that great problems are involved in using the information for statistical analysis.

The problems arise in the choice of what material shall be treated statistically, in translating the bare and perhaps technically phrased or too brief description of an accident into statistical form, and in preparing an adequate code of instructions. A balance has to be kept in mind at all times between the desire to extract as much material as possible from the accident reports, and to achieve the maximum attainable statistical accuracy, with the necessity to make the minimum possible demands on the time of Inspectors. There is also a need to publish the statistics with the least practicable delay.

3. The Analysis of the Information

The planning and administration of comprehensive accident prevention measures require knowledge of where, how and why accidents happen, in as much detail as can be obtained. Information is also needed on the consequences of accidents and on the classes of persons involved in particular types of accidents. It may, for example, be desirable to know whether a statutory provision requiring the wearing of eye protection in a certain process would be likely to produce a reduction in accidents; or to find the factors resulting in an increase of accidents among certain young workers. Effective statistics should be able to produce answers in such cases.

To achieve these results numerous analyses of the total numbers of persons injured in the accidents reported in a specified period are made, including those mentioned below.

Some of the data are prepared in such a way that changes over time can be observed, to make possible the calculation of the likelihood of accidents per person employed and to provide a measure of the relative effectiveness of accident prevention methods.

The manner in which these problems are tackled to meet the statistical needs of the Inspectorate is described below, and fuller details can be found in the *Guide to Statistics Collected by H.M. Factory Inspectorate.*

3.1. *Analyses by Age and Sex*

Details of the age and sex of the injured person are required to be entered on the statutory accident report, primarily for the information of the persons scrutinizing the report or investigating the accident, rather than for statistical use. In the statistics the Inspectorate, since 1959, has analysed the age of the injured person in broad bands (under 16, 16–17, 18–19, 20–39, 40–59, 60–64, 65 and over); but in addition an "age unrecorded" category is provided, as the age is not always known by the person filling in the report and is not entered in about 4 per cent. of the notices. The numbers of accidents to persons in these age bands are tabulated for each sex and additional tabulations can be made as required (for example, for analyses of accidents to older workers), but the remaining published tables only distinguish between those in each sex under and over 18 years of age, this being the age below which special legal requirements apply.

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3.2. *Industrial and Process Sub-division*

For analyses by industry, the accidents have been classified since 1959 according to the Standard Industrial Classification (1958 Edition) allocated for general official statistical purposes to the workplace where they occurred. This permits some comparison with employment, production and other statistics and the publication of accident statistics for the separate industries. There are, however, difficulties in the use of the Standard Industrial Classification as it is based on the "establishment" as its unit, and this is then classified according to the main industry in which it is engaged. The establishment may well consist of several factories, in addition to premises which are not factories, such as headquarters offices or retail shops, and the factories may differ widely in the work they undertake; for example, one factory may spin cotton, a second weave it and another bleach it. Each is a separate factory within the legal definition, yet the three constitute an establishment. The kind of work being undertaken in the part of the establishment in which the accident occurred may also not be apparent from the Industrial Classification. For example, there may be a printing works in a bank or a vehicle-repairing works in a distributive trade establishment. For the identification of accident risks and hazards it is, however, necessary to be able to identify, more closely than is possible with the Standard Industrial Classification, the process or type of work where the accidents are occurring. In addition, particular hazards, for example those in the use of woodworking machinery, may be found in widely differing industries, and it is desirable to be able to group these hazards together in the statistics. A system of classification by process, devised specially for this purpose, is therefore used, and this is applied to the factory or section of the factory irrespective of the main industry of any concern of which it may form a part.

The process classification was also first introduced into the statistics in 1959, replacing an earlier "trade" classification introduced in 1921.

3.3. *Analysis by Occupation*

The occupation of the injured person is recorded on the report form for the information of the Inspector investigating the accident. Except for an analysis of accidents in the construction industry, no statistical analysis is undertaken by the Inspectorate. A general analysis would be lengthy and does not appear to be justifiable.

3.4. *Analysis of Accidents by Cause*

This is the most complicated aspect of an analysis of accidents and provides the most valuable information for accident prevention use. The major difficulty lies in devising a classification system which can reveal the maximum amount of useful information obtainable from accident reports, and at the same time be operated largely by personnel without a technical background and call for the minimum amount of assistance and supervision from the Inspector. A further difficulty is to find a suitable procedure for selecting for classification the cause of the accident, out of several which may have contributed to it, on which the most effective action can be based. Considerable judgement is needed for the choice and the decision is frequently not clear cut.

It is necessary in planning the classification by cause to arrange for much more detail to be obtained on one class of accident than another, and these needs may

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change from year to year. A general classification by cause is therefore given initially to each accident under one of twenty-three heads. Certain types of accidents, certain accident causes, and accidents in certain industries then receive further scrutiny and are given one or more sub-classifications. The latter change from year to year according to current needs. Accidents on building work, explosions, and accidents involving transport vehicles are among those at present receiving special examination and classification.

3.5. *Analysis of the Site and Nature of Injuries*

This is an essential piece of information for accident prevention purposes, where planning of personal protection against injury is to be undertaken, for example, by the provision of protective equipment.

The classification used by the Inspectorate has sixty-two headings. It was specially devised and is adapted from the nature of injury classification (the "N" code) of the International Statistical Classification of Diseases, Injuries and Causes of Death.

One difficulty in deriving the Inspectorate's statistics on the site and nature of injury is that the person submitting the report, shortly after the accident, may not know fully what injuries have been received; in fact they may not then have been diagnosed. For some (but not all) purposes a knowledge of the final "injury" would be more advantageous, taking account of later complications and the results of surgical treatment, but this cannot be obtained where an accident report, submitted shortly after the accident, is the only source of information. The statistics of the Ministry of Pensions and National Insurance and those made by private firms, which can be based on successive reports, can take into account the full and final assessment of injury.

3.6. *Analysis of Severity*

Knowledge of the severity of injuries, i.e. the period of incapacity from work, the nature of residual disabilities and the nature of any rehabilitative treatment needed, is essential for assessment of the social and economic cost of accidents. The Inspectorate's system of accident reporting distinguishes between fatal and other injuries, but it is not possible to provide other information of severity from the present system.

4. Time Series of Accident Figures

In the past, H.M. Factory Inspectorate has published a number of accident tables giving time series, usually covering the last five years or sometimes giving comparisons with selected earlier years. At the beginning of 1959 considerable changes were made in the Inspectorate's statistical methods and this has caused a break in most of the tables so that there is little comparison between pre-1959 tables and the subsequent ones.

5. The Method of Enumeration

In comparison with the total number of accidents, incidents where more than one person is injured occur relatively infrequently among the accidents reportable to the Factory Inspectorate. For statistical purposes, therefore, no special account is taken of such incidents and the unit of statistical enumeration is the "injured person" and not the "incident".

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6. Analyses of Trends

Though the absolute figures of accidents, variously analysed, are required for assessing the size of the accident problem and for showing the detailed accident pattern, information is also needed from which the effects of accident prevention measures and changes in the risk of the occurrence of an accident can be deduced. It is desirable, therefore, to be able to eliminate as far as possible the effect on the accident totals of changes in numbers at risk, changes in hours of work and other transient factors which affect accident totals. For this purpose, the most favoured approach is to derive the accident frequency rate. In this country the formula normally used is:

$$\frac{\text{Numbers of lost-time accidents} \times 100{,}000}{\text{Number of man-hours worked}}.$$

This rate is used by a large number of firms for voluntary returns to H.M. Factory Inspectorate and for their own statistics. Returns are received from about 4,500 factories covering a third of the persons working in manufacturing industry. It is not possible, however, to obtain a national rate using this formula as no national figures of "lost-time" accidents, as distinct from notifiable accidents, are available, nor are there reliable figures for man-hours worked in the fields covered by the Factories Acts.

However, information on the total number of notifiable accidents each year, analysed by industry, is available, and this can be put to good use if the figures can be associated with the numbers at risk in these industries.

Statistics of employment of persons subject to the Factories Acts, i.e. with the same coverage as the accident notifications, are not compiled regularly. The field covered by the Factories Acts is defined legally† and is related to the type of work undertaken. It cuts across the boundaries of the "establishment" used as the unit in the Standard Industrial Classification. It covers people not regarded by that classification as employed in the manufacturing industries and likewise does not cover all that are so classified. This, though satisfactory for purposes of factory inspection, badly complicates the statistics.

Estimates of population covered by the Factories Acts in the manufacturing and certain other industries were, however, obtained in 1959 by applying the results of a special survey of a sample of 5,041 establishments, made by the Inspectorate, to the official estimates of the total number of employees in these industries in mid-1959.

The sample of establishments selected in each industry was, as far as possible, representative of all establishments in the industry, having regard to size and geographical distribution, and was selected from the "L" returns submitted under the Statistics of Trade Act, 1947. Visits were made to these establishments and the number of employed persons subject to the Factories Acts was obtained and related to the total number of persons shown as employed in the current L return. Assuming that the sample was representative for each size range of factories in each industry, estimates were made, for each sex, of the percentage of the total number of employees in each industry who were subject to the Factories Acts in October 1960. It is assumed that these percentages will remain relatively stable and enable sufficiently reliable estimates of the factory population of these industries to be made for a number of years, from the estimates of employment made by the Ministry of Labour.

† Sections 123–127 and 175–176 of the Factories Act, 1961.

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A similar estimate of the numbers employed on constructional work covered by the Factories Acts was obtained by a survey of a sample of about 650 concerns (selected from the Ministry of Works register of contractors and from the list of local authorities) undertaken in 1960.

It is thus possible to derive, for the separate manufacturing and certain other industries of the Standard Industrial Classification, series of accident incidence rates giving estimates of the numbers of accidents per 1,000 employees, which are thought to give valid year-by-year comparisons for these industries. The first set (other than for construction) was obtained for 1959 and incidence rates for the constructional industry were obtained for 1960. Inter-industry comparisons can be made from these figures but they have less statistical validity.

It should be noted that an estimate has not been obtained of the total number of persons covered by the Factories Acts. Persons working in premises whose classification falls outside the industries surveyed are not included in these estimates, but about 90 per cent. of the population at present covered by the Acts is included.

Though the incidence and frequency rates give information not contained in the basic accident figures, in that both rates make allowance for changes in industrial employment levels and the frequency rate takes into account changes in hours worked, they only partly take into account changes in industrial activity. Increased production obtained by working more intensively will not affect the employment or hours figures, though such an increase in production, involving increases in the amount of materials and goods moved, manipulated and stored in factories, might be expected to result in an increased number of accidents. Thus neither the frequency rate nor the incidence rate can be regarded as giving a complete measure of the effectiveness of accident prevention methods.

7. The Processing of the Statistics

The processing of the statistics is undertaken by the Statistics Department of the Ministry using a mechanical statistical installation. Cards are punched from manuscript returns sent monthly by the 97 Districts of the Inspectorate. The returns give the details of the accidents of which reports have been received during the preceding month, irrespective of the actual date of the accident, except that where the accident occurred more than a year previously it is not included in the statistics.

Arrangements are also needed for correction of information already forwarded; for example, an injured person may subsequently die, or investigation of the accident may disclose circumstances not evident from the original report. In addition, an accident may be found not to be reportable and must then be deleted. Separate returns of corrections and deletions are forwarded monthly by the Districts.

Sorting the accident cards and the necessary tabulation is a lengthy procedure, so that the number of accident tables that can be prepared is strictly limited. This work will be undertaken in the near future by a digital computer and it will then be possible to arrange tabulation within the prepared programme very much more easily.

The demand for early publication of the annual statistics, combined with the period of between three and four months at present needed for the preparation of the tables, means that the starting of the work on the final tables cannot be delayed after the end of January of the following year, and the statistics are then closed to further correction. This early closure inevitably means that some discovered errors or inaccuracies in classification remain unremedied; if, for example, a person injured in an accident in December dies in February, no adjustment to the final figures can be made. In general

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the inaccuracies are not serious or numerous and there is at present no acceptable alternative.

8. Publication of the Statistics

Until about three years ago the Inspectorate's statistics were published only in the Annual Reports of H.M. Chief Inspector of Factories, with a monthly table of provisional fatal accident figures in the *Ministry of Labour Gazette*. Since 1958, however, provisional quarterly and final annual tables covering all accidents have also been printed in the *Gazette*. These tables give the numbers of fatal and the total numbers of accidents analysed by process and by the Inspectorate Division where they occurred.

The quarterly provisional tables are based on the monthly District returns. The annual tables cover the same fields but are the final fully corrected versions. Though the quarterly tables are not fully corrected, the difference between the sum of the total number of accidents from the four quarterly tables and the final corrected total has, so far, been less than $\frac{1}{2}$ per cent., though the numbers of fatal accidents differ by a larger amount.

9. Unreported Accidents

The statistics of the Inspectorate refer to reported accidents but, though there is a statutory requirement to report, not all non-fatal accidents are notified to H.M. District Inspector, and some may be notified too late for inclusion in the statistics. The exact proportion not notified is not known but it is not thought to be very large. The proportion will be less for accidents in factories, which are inspected periodically, than for accidents on building or constructional sites where the work may be completed soon after the accident or may be of such short duration that it does not come to the notice of an Inspector.

Acknowledgements

Thanks are gratefully given to my colleagues whose ideas have contributed to this paper. The help of Mr R. F. Fowler, C.B.E., is especially acknowledged.

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Reviews

PAGE

1.—*Cox, D. R. and Smith, W. L.* Queues 151
2.—*Gnedenko, B. V. and Khinchin, A.* An Elementary Introduction to the Theory of Probability 152
3.—*Mosteller, F. et al.* Probability with Statistical Application. *Mosteller, F. et al.* Probability: A First Course . . . 153
4.—*Kurth, R.* Axiomatics of Classical Statistical Mechanics . 154
5.—*Medgyessy, P.* Decomposition of Superpositions of Distribution Functions . . . 155
6.—*Francis, R. G.* The Rhetoric of Science—A Methodological Discussion of the Two by Two Table 156
7.—*Hill, A. Bradford.* Principles of Medical Statistics . . 157
8.—*Deming, W. Edwards.* Sample Design in Business Research . 158
9.—*U.N. Statistical Office.* A Short Manual on Sampling, Vol. 1: Elements of Sample Survey Theory 159
10.—*Syski, R.* Introduction to Congestion Theory in Telephone Systems 160
11.—*Von Handel, P.* (Ed.). Electronic Computers, Fundamentals, Systems, and Applications . 161
12.—*Bellman, R.* Adaptive Control Processes: A Guided Tour . 161

PAGE

13.—*Ackoff, R. L.* (Ed.). Progress in Operations Research, Vol. I . 162
14.—*Cabell, R. W. and Phillips, A.* Problems in Basic Operations Research Methods for Management . . . 163
15.—*Bowen, E. K.* Statistics—with Applications in Management and Economics . . . 163
16.—*Marshall, Alfred.* Principles of Economics 164
17.—*Grossman, G.* Soviet Statistics of Physical Output of Industrial Commodities: their Compilation and Quality . . 165
18.—*Nigam, K. and Chaudhuri, N. C.* The Corporate Sector in India: a Factual Presentation of Long and Short-term Trends . . 166
19.—*National Bureau Committee for Economic Research.* Demographic and Economic Change in Developed Countries . 167
20.—*Ministry of Pensions and National Insurance and the National Assistance Board.* Social Security Statistics . . 168
21.—*Furneaux, W. D.* The Chosen Few: An Examination of Some Aspects of University Selection in Britain . . 169
22.—*Rodwin, L.* Housing and Economic Progress: A Study of the Housing Experiences of Boston's Middle-income Families 171

1. Queues. By D. R. Cox and Walter L. Smith. London, Methuen; New York, Wiley; 1961. xii, 180 p. $7\frac{1}{4}''$. 21*s*. (Methuen's Monographs on Applied Probability and Statistics. General Editor, M. S. Bartlett.)

Over the past ten years our Society's *Journals* have, in common with other similar journals, contained a large number of papers on queues. It is not difficult to see the reasons for the phenomenon. Congestion problems occur frequently in practice and it is easy to set up models of queueing systems—not that the models usually correspond well with practice. For many of these models it is possible to write down the Chapman–Kolmogorov equation and,

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although it may be difficult to solve, at least a few transforms and moments can be found, and usually the calculations look complicated enough to give an air of respectability. So the handle-turning goes on and out pour the papers.

It would therefore have been easy for the authors of this monograph to have produced a book by stringing together the results from such papers. They are to be most warmly congratulated on not having done this. In fact they have given us an original and elegant book which should delight the applied statistician and interest the theoretician.

There is a long (32 pages) introductory chapter which discusses at an elementary level the fundamental ideas and their ranges of application. There is, for example, an account of random arrivals in some detail (though not as fully, or as attractively, as Khintchine: *Mathematical Methods in the Theory of Queueing*. Griffin, 1960; reviewed in this *Journal*, **124** (1961), 93). The next two chapters deal with queues with random arrivals and contain, amongst other topics, a careful account of the meaning of statistical equilibrium, a discussion of arrival rates and service times depending on the queue size and an original account of the non-equilibrium situation. Chapter IV is devoted to the machine interference problem, and specialized topics are mentioned in the final chapter. The latter includes sections on the integral equation approach, simulation and some original remarks on busy periods. The three appendices contain a brief bibliography, nineteen exercises, and three pages on probability and Laplace–Stieltjes transforms.

The person who will value this book the most highly will be the operational research worker faced with practical congestion problems, who is prepared to adopt what the authors call an "applied mathematician's" approach to his mathematics: that is, to regard the theory as a valuable tool but not as an end in itself. For example, consider the ingenious ideas on pages 65–68 on the "rush-hour" problem, wherein renewal theory is used in an approximate, but highly revealing, way to say something about the effect of a sharp increase in demands for service on the system.

This blend of elegant and powerful mathematics with sound practical thinking is found throughout the book. Although the mathematics may become "highbrow" it is never algebraically complicated. Sometimes I felt that the continued emphasis on examples rather than general methods was overdone. Thus the introduction of general (as distinct from exponential) service times on p. 51 is very brief and deserved more general discussion of the usefulness of the embedded Markov chain technique. In other words, it is a little deficient on describing general methods. There are several points of originality: the busy-period distribution, queues with priorities and the non-equilibrium situation.

This is an excellent book. The mathematician may prefer Khintchine (*op. cit.*) but most readers will prefer Cox and Smith for their modern outlook and sound practical sense.

D. V. LINDLEY

2. An Elementary Introduction to the Theory of Probability. By B. V. Gnedenko and A. Ya Khinchin. San Francisco and London, W. H. Freeman, [1961]. 139 p. 8″. 10*s*.

The first edition of this book was published in Moscow in 1945 and there were subsequent editions in 1949, 1952 and 1957. The present paper-backed edition has been translated by W. R. Stahl. Except for a few sections requiring sixth-form algebra, the mathematics is kept to an extremely elementary level, little more than simple algebraic symbolism being required. Much of the discussion is in terms of easy numerical examples.

The topics dealt with include: the laws of probability, conditional probability, Bayes's theorem, the binomial distribution, mean value and standard deviation of random variables, a simple form of the weak law of large numbers, the normal distribution. The book ends with a short history of the theory of probability up to the present time, some emphasis being placed on Russian contributions.

The illustrative examples cover a wide range of topics, military applications being the most common. Naturally the examples are rather idealized. However, they have been well

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chosen to emphasize that the subject is a serious practical one and the usual frivolous problems about playing cards and dice are not used.

The clarity of exposition is admirable throughout. Particularly good is the stress laid on the frequency interpretation of probability. Two minor criticisms are that the use of Chebyshev's inequality to set limits to the binomial distribution does not seem a good idea, and that there is a tendency in some examples to assume tacitly that events are independent when in practice they might not be.

The book can be strongly recommended.

D. R. Cox

3. Probability with Statistical Applications. By Frederick Mosteller, Robert E. K. Rourke and George B. Thomas, Jr. Reading, Mass., U.S.A.; London, Addison-Wesley Publishing Co.; 1961. xvi, 478 p. 9½″. 38*s*.

Probability: A First Course. By Frederick Mosteller, Robert E. K. Rourke and George B. Thomas, Jr. Reading, Mass., U.S.A.; London, Addison-Wesley Publishing Co.; 1961. xvi, 319 p. 9½″. 30*s*.

The second of these two books is contained in the first. The additional material in the first book comprises three chapters:—Joint Distributions and Continuous Distributions (Chapter 6), Theory of Sampling (Chapter 9) and Least Squares, Curve-fitting and Regression (Chapter 10); an appendix proving that, if X, Y and Z are random variables each taking a finite number of possible values, pairwise independence implies independence of $X+Y$ and Z; and answers to about half the exercises (which are numerous and mostly of a kind which should interest the student). This review will, for the most part, be written with reference to the larger book but, of course, comments on the common text apply equally to both books.

The titles of the chapters common to both books are (numbers refer to larger book):—

1. Probability and Statistics. The Study of Variability
2. Permutations, Combinations and the Binomial Theorem
3. A First Look at Probability: Equally Likely Outcomes
4. General Theory of Probability for Finite Sample Spaces
5. Numbers Determined by Experiments. Random Variables
7. Repeated Trials with Two Types of Outcomes. The Binomial Distribution
8. Some Statistical Applications of Probability.

There are also appendices on (very) elementary set theory, and on "summations and subscripts", four tables (random digits, $n!$ and $\log n!$, normal curve areas and binomial distribution) and a chart giving 95 per cent. confidence limits for a binomial proportion.

These titles reflect a rather thorough, and necessarily slow-moving, presentation of basic probability theory. The mode of development is well suited to a painstaking student without special mathematical interests but with broad general scientific curiosity. The remarkably low price should make the book popular, especially among students who are not statistical specialists. On the other hand, it is difficult to envisage a wide market (in England, at any rate) for the smaller book. It is only eight shillings cheaper, while the omitted portions are among the most interesting and most useful for practical applications.

The binomial distribution plays a central role in the book. It is a mark of the elementary level of the book that the Poisson distribution is not even mentioned. However, there is (in Chapter 7) an ingenious discussion of the central limit theorem, as applied to binomial distributions, which should give the reader a good understanding of the meaning of this important feature of probability theory. Considerable use is made of Chebyshev's inequalities in assessing limits within which a random variable may be expected to fall. In one such application there appears one of the occasional (though fortunately rare) lapses from clarity of expression in the text. On p. 291 [equation (7)] it is stated that "the probability is

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approximately 0·95 that $|\bar{p}-p| \leqslant 11\sqrt{n}$" without making it clear that this is so *only* when $p \fallingdotseq \frac{1}{2}$. Other examples of such minor lapses are the use of "value of $x+y$" for "(value of X)+(value of Y)" in Chapter 9, and the sentence "Hypotheses are rejected because we believe their alternatives have a good chance of being true in the light of the total evidence" (on p. 310) which is not followed by any explanation of the meaning of "good" in this context.

From a pedagogical point of view, the development of least squares techniques without using the calculus is of particular interest. The authors have succeeded in doing this without giving the text too pronounced an old-fashioned flavour. Also of interest are the analogies drawn between formulae relating variances of random variables with the variance of their sum, and formulae relating the sides and angles of a triangle. These and a number of less notable innovations should help many teachers of elementary statistics to freshen up their courses.

N. L. JOHNSON

4. Axiomatics of Classical Statistical Mechanics. By R. Kurth. Oxford, London [etc.], Pergamon Press, 1960. x, 180 p. 9″. 45*s*.

Those statisticians who delve into books on statistical mechanics cannot fail to be struck by the reluctance of their authors to use the word "probability". With the notable exception of *The Mathematical Foundations of Statistical Mechanics* by A. I. Khinchin (Dover, 1949) this reluctance is entirely justified, for in most books on the subject an extremely naïve view of probability is presented. As Khinchin himself remarks, in such books "the formulation of the fundamental notions of the theory of probability as a rule is several decades behind the present scientific level, and the analytic apparatus of the theory of probability, mainly its limit theorems, which could be used to establish rigorously the formulas of statistical mechanics without any complicated special machinery, is completely ignored".

The present volume is an attempt to remedy this situation by constructing "classical statistical mechanics as a deductive system founded only on the equations of motion and a few well-known postulates which formally describe the concept of probability". After a short introduction and a chapter summarizing the required mathematical tools (including sets, mappings, Lebesgue measure and integration, Hilbert spaces) the author introduces the idea of a phase space defining a mechanical system and of flow in that space. This leads in a fairly standard way to Birkhoff's theorem on Boltzmann's ergodic hypothesis, showing that the equivalence of a time average along a path in the phase space to the (phase-) average over an invariant subset of that space may be accepted if and only if that subset is metrically indecomposable. Faced now with the problem "under what conditions is there . . . an invariant subset of finite positive measure which is metrically not decomposable?", the author makes no direct answer.

Instead he introduces an axiomatic system of probability, justifying it by a frequency interpretation, in this case the Gibbs's ensemble. Bayes's axiom is also introduced as a way "to adopt a probability distribution without arbitrariness", to be used for defining an initial probability distribution in the phase space. Then follow chapters on the conditions under which mechanical systems of a stochastic nature may tend to statistical equilibrium, the behaviour of such systems in equilibrium and finally a chapter on statistical thermodynamics including the connection between entropy and probability.

While it is true that the ergodic hypothesis may be justified in terms of statistical equilibrium, such a justification implies a wholehearted acceptance of the probabilistic basis of statistical mechanics. It is a fault of this book that there is no evidence of such an acceptance by its author. Rather he gives the impression that he uses probability, not because he believes it to be appropriate to the mechanical systems discussed, but for want of anything better. A symptom of this is the virtual lack of any reference to standard

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results in probability theory (an exception is Tschebyscheff's inequality) and a complete lack of any reference to work done on stochastic processes in the past twenty years, much of which would be relevant to the author's aim of providing a rational basis to statistical mechanics. In fact such references as there are to probabilistic and statistical sources are concerned with the philosophy of probability, with the exception of a reference to Cramér's *Mathematical Methods of Statistics* as a source book for Lebesgue theory.

In spite of these shortcomings, however, this book may be recommended to students of statistical mechanics. It represents significant progress towards the time when the subject becomes a recognized branch of applied probability theory.

F. DOWNTON

5. Decomposition of Superpositions of Distribution Functions. By Pal Medgyessy. Budapest, Publishing House of the Hungarian Academy of Science, 1961. 227 p. $9\frac{1}{2}''$. $7.50.

The problem treated in this book and succinctly summed up in its title is not a statistical one. However, special forms of it underlie an important group of statistical problems and, indeed, according to Medgyessy it first arose in a statistical context (in Karl Pearson's 1894 paper). The general problem is simply stated; a function $G(x)$ is known to be expressible as

$$a_1 F_1(x) + \ldots + a_N F_N(x)$$

where the F's are distribution functions of known forms but depending on unknown parameters; the a's are unknown constants (which may be negative) and N may or may not be known. (It is sufficient that the F's have Fourier transforms which behave like characteristic functions; they are not necessarily associated with any random variable and, indeed, probabilistic notions are deliberately avoided.) $G(x)$ is supposed either known numerically for all relevant values of x (in principle) or to be tabled at small intervals over the whole range of x (in practice). The statistical forms of the problem are: where G is a regression function, which is therefore only known up to a random error and where G is a distribution function (the a's being commonly positive and summing to 1), from which a sample has been drawn. In both these cases N is as a rule assumed known (to be 2 or 3).

The present book is concerned with cases such as occur in physics where the random error is "negligible" (i.e. of the same order as the approximations to be used in any method of numerical computation), as, for example, in studies of diffusion or of the absorption spectra of gases. After considering the linear operator technique which has been applied to the normal case by Doetsch and others (which closely resembles Spectral Analysis of a Correlogram), the author turns to the treatment of a more general form of such an operator (for more general functions F). He then returns to the various special cases and even gives the results of a partial numerical application of the method although no complete numerical solution is given. (This is possibly due to the non-availability of an electronic computer, as a full solution of the example briefly treated here would seem to require assistance of this order.) The last chapter discusses other methods: graphical "stripping", Karl Pearson's method of moments (yielding algebraic equations for the unknowns) and special methods for special types of F. The references are impressively wide-ranging and representative.

The book is, fundamentally, very clear; the mathematics while rigorous is relatively easy and ideas are introduced by treating simple cases before they are generalized. But, in spite of the claims of the blurb that "It is written first of all for readers working in the field of experimental sciences", it remains a mathematician's book; the interest lies in the degree of generality and rigour rather than in any practical utility. It is not clear that the method of decomposition treated here can actually be applied by existing machines in a finite time and it is not clear that the author is at all seriously concerned about this aspect of the question. The degree of approximation is discussed but there is no real numerical study. The other methods are noted but not seriously compared.

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In practice, the book is very difficult to read because of its abuse of the English language. One appreciates it when the author acknowledges his debt to two senior colleagues for "their tiresome work" in "revising" his manuscript and one soon learns that Point 9, for example, must mean Chapter 9 (whilst Chapter III means Part III), but what is one to make of the section headed: "Determination of being a mixture by the counter current method"? Several passages are quite unintelligible and many others obscure, to say the least. Even a casual aside needs reading twice; e.g. "If the reader wishes to apply any of the methods to be treated he need not forget what has been said above concerning error estimates as well as, in all complicated cases, the request of a subsequent checking when a decomposition procedure etc. had been finished."

The printing is clear and the production is elegant but there are one or two (at least) rather disconcerting misprints (as in the equation on page 27).

D. E. BARTON

6. The Rhetoric of Science—A Methodological Discussion of the Two by Two Table. By Roy G. Francis. Minneapolis, University of Minnesota Press, 1961. [London, Oxford University Press.] viii, 183 p. 9″. 38*s*.

This book, the author of which is an American sociologist and statistician, is concerned with methodology and is directed at social scientists rather than statisticians. It contains nine chapters and a glance at some of their headings, "Categorical logic", "Problems in the proof of causality", "An attempt to measure causality", indicates that it has a strong philosophical bias, though a number of statistical techniques for dealing with qualitative data are described in it.

In the introductory chapter the thesis that "the goal of science is a theory" is defended (though not altogether convincingly) against other theses, for instance that the goal is prediction. Mere "operationalism" as a method of scientific procedure is frowned upon. However, the author does warn against theory for its own sake and against "ideal" formulations which tend to inhibit enquiry and research. In addition to theory the author reminds us that the research worker requires rules to enable him to draw conclusions from his data; this observation is the signal for a chapter on categorical logic and a review of syllogistic reasoning. In this context interesting parallels are drawn between the four forms of two-term propositions (the universal affirmative, the particular affirmative, etc.) and two-by-two contingency tables. Since no *necessary* conclusions can be reached from particular propositions, using the syllogism, the limits of categorical logic have to be admitted and attention is turned to statistical methods and probability statements.

Before dealing with these methods the author reviews the literature (Chapter 3) on causality. Amongst other things Karl Pearson's notion of association, which circumvents the problem of antecedent and consequent in a causal argument, is noted. But Pearson, like Hume, is criticized for implicitly assuming that "something like cause existed in the world of fact (sense perception)", whereas in the author's opinion effort should not be expended on trying to describe the essence of cause; instead, observations should be weighed and explained in the light of theory.

The next three chapters (numbers 4, 5 and 6) are statistical in content. They deal primarily with tests of independence and association where fourfold tables are concerned, and include an account of Fisher's exact test and the chi-squared test. But, in addition to measures of reality of association, an exhaustive account of measures of intensity of association (Yule's Q, Pearson's C, etc.) is provided.

Chapter 7, "An attempt to measure causality", would appear to be the *pièce de résistance* of the book. "If chance and cause are at least contraries", we are told, "then a table showing less than certainty and more than chance ought to generate a statistic with the same characteristics." This notion, due to T. C. McCormick, is elaborated, and a measure of causal efficiency, or goodness of prediction, called *kappa-prime*, is derived. The

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example given is one from voting: if D stands for Democrat and R for Republican, and the following observations are made:

	Intended to vote	
	D	R
Voted		
D	a	b
R	c	d
	n_1	n_2

then *kappa-prime* is given by the expression $(p_1 - p_2)$, where $p_1 = a/n_1$, $p_2 = b/n_2$. This measure is derived by defining maximum and minimum predictive efficiency. When prediction is perfect *kappa-prime* is clearly unity, but the case where its value is zero is hardly likely to arise since it will have been ruled out by an earlier test of association. We observe in passing that no information about the possible distribution of the statistic is given.

The penultimate chapter of the book deals with "correlated" dichotomies; the final chapter contains a summary and conclusions.

It is difficult not to be critical of this book. The style lacks crispness; arguments often lack point; and it is a surprise to find clumsy sentences, such as "practically no research is conducted in the social sciences but what a test is made of the null hypothesis", in a work of this type.

Since the book is directed primarily at social scientists one feels that the thesis that "the goal of science is a theory" has been taken over too uncritically from the exact sciences. Experience in psychology, for example, suggests that the last three decades, which were dominated by theories and by the hypothetico-deductive method, have been somewhat unproductive. One feels that, in the social sciences generally, a great deal of spade work in the collection and analysis of data and in correlation of observations has still to be done before any useful theories emerge. Indeed the author hints at this fact on page 6 and elsewhere.

The book suggests little communication between the social scientist and the statistician. Though few would dispute the value of tests of association in research work in a relatively young science, the value of measures of intensity of association is doubtful as their interpretation is obscure. Yet a large section of the book is devoted to a description of such measures, and little clarity is achieved.

It is interesting that, in the discussion of *kappa-prime*, the question to which McCormick sought an answer was (p. 125), "What kind of guarantee is there that the same table (that is fourfold table) would generate an equal reduction of error the next time it was used?" If I interpret the matter correctly, the problem is one of estimating the mean and confidence limits of a (or d) in the fourfold contingency table given above, in cases where the null hypothesis is discarded. A statistical answer to this question is available and is much more satisfactory than that offered by *kappa-prime*.

This book may be of interest to the social scientist with a philosophical bent, but is unlikely to be of much value to the practical worker.

A. E. MAXWELL

7. Principles of Medical Statistics. By A. Bradford Hill. 7th Ed. London, The Lancet, 1961. x, 367 p. $8\frac{1}{2}''$. 12*s*. 6*d*.

In the seventh edition of this classical text book of statistical methods applied in medicine further new material has been added, and numerous verbal changes have been made. The book retains unimpaired, however, all its characteristic flavour, and we can now enjoy over fifty new pages of vintage Bradford Hill, for an increase of financial outlay which is only just more than proportionate.

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The principal additions are three new chapters, on sampling methods, on enquiry forms and record forms, and on measures of sickness. Each makes a material contribution to the comprehensiveness of the book, and each has the simplicity, the clarity and the air of inevitability which persuade the reader that it is his own knowledge which is being extracted, set out clearly and made to point to the right conclusion: a flattering style, which has led many an innumerate physician to lose his distrust of statistics.

Besides these major additions there is now a section on the *t*-test (in which the common mis-spelling of W. S. Gosset's name is perpetuated), and a small table of the percentage points of *t* for degrees of freedom from 1 to 25. A way of testing the significance of a linear trend in a series of percentages is also added. Two interesting signs of the times are the final disappearance of an explanation of the term "probable error", which survived until the sixth edition, and the use of semi-algebraic expressions like "Sum $(x-\bar{x})^2$" instead of the entirely verbal forms previously used. It is probable that this indicates quite exactly the present location of the boundary of comprehensibility for the medically qualified; algebraic forms are acceptable, functional notation is not!

Principles of Medical Statistics remains unquestionably the best source of statistical knowledge for those working in the field of medicine. A strong case could be made for its being the best introduction to statistics generally; certainly few other text books establish so firm a foundation of proper sceptical enquiry into the nature and reliability of numerical data before building their edifices of mathematical theory.

P. D. OLDHAM

8. Sample Design in Business Research. By W. Edwards Deming. New York, London, Wiley, 1960. xx, 517 p. 9″. 96*s*.

The author of this book has probably had as much practical experience of designing sample surveys as anyone in the world. His book consists of three parts. The five chapters of Part I (about 80 pages) serve as an introduction to the subject for both statistician and non-statistician. They discuss the role of the statistician in a survey and introduce the reader to probability sampling, sampling frames, expected values, standard errors and non-sampling errors.

It is in Part II (about 270 pages) that the book differs most markedly from others in its approach. It confines itself to considering a certain type of replicated sample design, the author saying that he has used no other type of design since 1948. The advantage of these designs is the relative ease with which sampling errors can be calculated. In the simple case of selecting a sample of, say, 1 in *n* addresses from a single serial numbered list, the author would select ten independent systematic subsamples using ten random starts between 1 and 10*n*. The sampling errors are then readily calculated from the means of the ten subsamples which are tabulated separately. The estimated errors take account of any gains in efficiency due to stratification arising from the arrangement of the list. In the more complicated cases where primary units, such as administrative districts, are involved, the procedure boils down to the familiar process of selecting primary units with replacement and with probability proportional to the number of ultimate sampling units they contain or a measure thereof. In such cases there will generally be only two subsamples and the sampling errors are computed from the component parts of the two subsamples. The approach described is an extremely practical one in the American situation, where adequate up-to-date lists of dwelling units do not exist and have to be constructed, and where the only measure of the number of dwelling units in a primary unit may be very much out of date. For us in this country a somewhat similar situation arises in sampling from the electoral register, when the actual electors to be interviewed are selected from the current register but the first-stage units are selected with probability proportional to the electors in them at some earlier date. In such a case the number of electors to be taken from the selected first-stage units is adjusted proportionately up or down to allow for any increase or shrinkage in their populations since the earlier date.

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Many practitioners will turn first to Chapters 10 and 11. Chapter 10 gives details of a replicated sample of dwelling units in a small urban area, while Chapter 11 goes on to consider the design of national samples. The main difficulty for the reader will be to master the meaning of the probably unfamiliar terms, sampling unit, segment, block, thin zone and thick zone, and the difference between them. He is not helped by two unfortunate errors in the headings of Table 5 of Chapter 10. The third and seventh columns are headed "Number of *segments* in the block" and the fourth and eighth columns are headed "3 random numbers between 1 and the number of *segments*". In each case *segment* is incorrect: it should read *sampling unit*. This is very misleading as in most cases in the particular example the number of sampling units in a block equals the number of segments but the important thing is to know what to do when these are not equal. Quite apart from these errors, a little more care in the construction of the example would have brought out the distinction more clearly.

The next chapter gives details of the field procedure for dividing a block of buildings into segments, and for finally selecting one person at random within a dwelling unit. Short chapters follow on measuring interviewer variability and on how to sample populations about which little information is available. Part II concludes with a fairly lengthy consideration of the different ways in which stratification can be introduced into a sample design.

Part III (about 150 pages) sets out to supply some of the basic theory for what has been applied in Part II. Anyone teaching sampling theory may feel that in this respect it does not adequately replace some other and more comprehensive text books.

In summing up the book one feels that its strong point is the wealth of exercises and worked examples included and the practical advice resulting from so much experience that it gives. Furthermore, the author succeeds in communicating some of his enthusiasm for his subject. I am sure that practitioners and students will find the book both useful and stimulating.

P. G. GRAY

9. A Short Manual on Sampling, Vol. 1: Elements of Sample Survey Theory. Statistical Office of the United Nations, New York, 1960. vi, 214 p. 11″. 21*s.*; $3. (Studies in Methods Series F, No. 9.)

This manual is primarily written for the non-mathematical user of statistics. It presents a summary of well established sampling techniques and is divided into two sections. In the first the theoretical background is described; very little previous knowledge is required. In the second a series of numerical examples is given using the same body of data in different ways to exemplify each of the processes described. Twelve of the 22 examples illustrate various aspects of cluster or multi-stage sampling. This volume considers only the design and analysis of sample surveys: other aspects of a sample enquiry are reserved for a second volume.

The theoretical description, though adequate, is limited by the audience for which it is written. Even Student's test is omitted, by confining the discussion to large sample theory. There is an irritating tendency to refer ahead for definitions of unfamiliar terms.

The numerical examples are worked through in comprehensive detail, but the formulae presented as suitable for computation are unsatisfactory. Thus the corrected sum of squares is always given as

$$\Sigma y_i^2 - n\bar{y}^2 \quad \text{rather than} \quad \{n\Sigma y_i^2 - (\Sigma y_i)^2\}/n;$$

and the formula for the estimated variance of a ratio estimate, given on p. 102 as

$$\frac{N-n}{Nn}\left\{s_y^2 - 2s_y s_x r\frac{\bar{y}_n}{\bar{x}_n} + \frac{s_x^2 \bar{y}_n^2}{\bar{x}_n^2}\right\}$$

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(where r is a correlation coefficient), is more easily computed from

$$\frac{N-n}{Nn(n-1)}\{\Sigma y_i^2 - 2R\Sigma y_i x_i + R^2 \Sigma x_i^2\}$$

where $R = \Sigma y_i / \Sigma x_i$.

The quality of reproduction is poor (it appears to be duplicated) and some of the formulae lack clarity.

Despite the shortcomings of this manual, it is still a useful *vade mecum* for those whose knowledge of statistical theory is limited. If it does not supersede the recognized text books on the subject, it may at least provide an acceptable introduction to them.

H. R. Simpson

10. Introduction to Congestion Theory in Telephone Systems. By R. Syski. Foreword by Sir Thomas Eades. Chapter Two by H. H. G. Morris. London, Oliver & Boyd, 1960. xvi, 742 p. $9\frac{3}{4}''$. 105*s*.

The text starts on page 1 with a definition of the telephone ("an instrument by means of which its user, a subscriber, can converse with other subscribers, each of whom possesses a similar instrument") and by page 107 has progressed to a definition of a stochastic process ("any family of r.v.'s $\{x_t', t \epsilon T\}$"). This is an indication of the scope of the book, the purpose of which is to study the passage of telephone traffic through a switching system using the recent developments in the theory of stochastic processes.

An enormous volume of literature is devoted to telephone congestion theory. This is reflected in the great size of this book which brings together for the first time most of the major contributions to the subject. As such, it is itself a substantial contribution to the subject.

Congestion theory, of course, has its origins in the work of Erlang. More recently, these basic concepts in telephone traffic were used in the development of a theory of queues, and taken up by operational research, applications being found in widely different fields, such as road and air traffic, storage theory, machine maintenance, and so on. These studies have, in turn, produced contributions of importance in telephony, and the author also gives a full account of them.

The book is mathematical in character, and it is to be wondered whether the telephone engineers for whom it is intended will find it palatable. Although the author states that mathematical rigor is often sacrificed for simplicity, nevertheless the degree of abstraction in places, as for example in the chapter on probability theory, seems unnecessary. The mathematician interested in queueing theory, or, more generally, congestion theory, will find it an extremely useful source of references.

After an introductory chapter giving a survey of the whole book, Chapter 2 describes the telephone equipment and systems under study. Chapter 3 presents a mathematical account of probability theory and Chapters 4 and 5 proceed to apply the concepts of stochastic processes to the flow of calls and a study of systems with losses. Chapter 6 provides an account of queueing theory as usually understood. Chapter 7 studies the special problems of gradings. Although these are problems which have, up to now, been studied exclusively in the context of telephony, it is interesting to note that similar, or more general, problems of gradings could arise in situations where both specialized and common counters are provided for different types of customers. Chapter 8 is devoted to the study of link systems. Chapter 9 deals with problems involving special queue disciplines, bulk service, early departures, queues in series, and other special systems. The final chapter, 10, studies the telephone exchange as a whole and also networks of exchanges. A mathematical appendix gives a summary of basic formulae, and an account of the special functions. There is a list of some 470 references cited in the text. Papers which appeared in 1958 and thereafter are excluded.

This important book represents a prodigious effort of compilation and scholarship which deserves our admiration.

F. G. Foster

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11. Electronic Computers, Fundamentals, Systems, and Applications. Edited by Paul von Handel. Wien, Springer-Verlag, 1961. vi, 235 p. 9½″. 84*s.* 6*d.*

Though this book has been published by Springer in Vienna and the contributors are called von Handel, Tantzen, Jaenke and Gschwind, it is the product of American writers who work in the Institute of Defense Analysis and in an Air Force Development Center in New Mexico. P. von Handel says in his Preface that they "address the book to engineers and scientists who want to know the performance of computers, as well as to managers who are mainly concerned with their economic aspect". It is doubtful if the latter would profit from it.

The book contains five chapters: I. Introduction, II. Digital Computers, III. Analog Computers, IV. Digital Differential Analysers, V. Computing Control Systems. In Chapter II we read about number systems and codes, about programming, and about some applications. Chapter III contains a description of basic analogue components, of their combinations and uses. Chapter IV starts by explaining that "the digital differential analyser is probably not regarded as a very important type of machine at present", but that it might have a future. Some of the detail then following could have been replaced by references to the previous chapter. The last contribution starts with an explanation of the principles of control engineering, and of the role of computers within it.

At the end of the book we find a glossary of computer terms. Diagrams are clearly printed and there are some photographs of machines and, more usefully, of components.

A study of this book will not make anybody a competent computer engineer, programmer, or even user. But it has the merit of combining, within one volume of about 200 pages, a variety of subjects which are useful to a beginner in any of these fields, though it contains too much for him and too little for an expert. However, the computer community comprises a great number of people and it is not impossible that some will want precisely this sort of well produced and well printed text.

S. VAJDA

12. Adaptive Control Processes: A Guided Tour. By Richard Bellman. Princeton, University Press; London, Oxford University Press; 1961. xvi, 255 p. 9¼″. 42*s.* (A Rand Corporation Research Study.)

This is another book from the prolific mind and pen (or electric typewriter) of Richard Bellman. It is written in an entertaining style with, at times, the use of a rich vocabulary. On the first page of the first chapter the word "gallimaufry" appears—a word which, as far as I can remember, has never occurred in the gallimaufry of literature to which I have been exposed. On nearly the last page he refers to the polynomials of a mathematician, Cebycev —a spelling perhaps correct, perhaps soon to be universally adopted, but not as familiar to me as five or six other attempts at the name. Between these flourishes is an account of the application of the methods of dynamic programming to several types of problem leading up to control processes. The author is careful to point out that just one of several possible approaches is discussed.

The book contains an introduction and eighteen chapters, each of which has a list of further references and comments. These forty pages or so of comments give a good indication of how much work has been going on and how great a proportion Bellman himself has had some part in. All the chapters are short. The first four show how feedback control processes lead to problems which seem to appear as examples in the classical calculus of variations but which present difficulties when attempted by those methods; some differential equations and variational problems are presented in a functional equation form which leads naturally to a treatment by dynamic programming methods. The "curse of dimensionality" is continually stressed and three of the chapters discuss the difficulties and possible methods of avoiding them when the number of state variables is as large as three. The book turns from the treatment of deterministic control processes to those with a stochastic element, first dealing with the case where the distributions concerned are

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fully known. Bellman shows that the functional equation technique yields a unified approach for both the deterministic and stochastic processes. There are some suggestions for the numerical solution of some non-linear, first-order differential equations in terms of an equation derived from a control process. Brief chapters on stochastic learning models and game theory are followed by those on adaptive processes in which there is incomplete knowledge about the characteristics of the stochastic elements. The last chapter "Successive Approximations" reviews applications of the classical approach by solving sequences of problems which converge to the desired one and contrasts with it the method of deriving a succession of approximate policies which converge to the desired optimum.

Throughout the book the emphasis is directed towards expressing in a suitable form the problems considered and finding methods of computing the solution. It seems to the reviewer a big disadvantage in a book of this type that there are no numerical examples at all. There are illustrations of how problems need to be formulated—although in nothing like the profusion of the author's *Dynamic Programming*—in order to be able to compute a solution, but there the problem is left. It is perhaps out of place to make this objection when I expect that it is amply met by the contents of a new book by S. Dreyfus and the author on the *Computational Aspects of Dynamic Programming*, which I have not yet seen, but in the absence of such examples it is impossible to assess the success or importance of the techniques proposed. Apart from this criticism, the book is a useful addition to the literature on dynamic programming.

E. S. PAGE

13. Progress in Operations Research. Vol. I. Edited by Russell L. Ackoff. New York, London, Wiley, [1961]. xii, 505 p. 9″. 92*s*.

This is the first book of a review series sponsored by the Operations Research Society of America. It deals with developments of mathematical techniques. Other volumes will deal with other aspects of progress made, and they will be organized by other editors. The choice of topics reflects, of course, their interests and preoccupations, and their success in finding suitable contributors. Professor Ackoff regrets that he was unable to procure a chapter on business gaming, while game theory was deliberately excluded "because of the paucity of practical applications of the theory".

This volume I contains the following articles: The meaning, scope and methods of operations research. Decision and value theory. A survey of inventory theory from the O.R. viewpoint. Mathematical programming. Dynamic programming. Dynamics of operational systems: Markov and queuing processes. Sequencing theory. Replacement theory. The theory and application of simulation in O.R. military gaming. Progress in O.R.: the challenge of the future.

It will be noticed that the first and the last chapters treat principles, while the others contain more technical detail.

The authors were free to interpret their task according to their own inclination, and thus the word progress in the title does not necessarily mean recent progress. On the whole they appear to have been wise in going reasonably far back in time. This helps to present a continuous whole, and although there already exist text books that contain some of the topics dealt with here, there is no harm in that. The character of a review is maintained by leaving out some proofs or merely giving sketches of them, and by referring to extensive bibliographies at the ends of the chapters. As one reads on, one sees that all chapters were written by experts in their fields, with sound judgement of what results will remain useful. Of course, others might have come to other conclusions.

Your reviewer cannot claim to be an expert in all these fields, and his further remarks will be restricted to Chapter 4 on Mathematical Programming, which is the longest in the book. (Dynamic programming has an excellent chapter to itself.)

The point is repeatedly made, and it is worth repeating, that the search for an optimum may cost more than a possible improvement from a position already reached might

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warrant. This is a consideration in the true spirit of O.R. We also agree with the author's remark (pp. 115–16) about early times in linear programming when one "frequently encountered 'management mechanics', equipped with only a fancy 'screwdriver' (called linear programming), wandering around looking for 'screws' to turn. When confronted with a nail, rather than a screw, persons with such background would be inclined to cut a slot into the head of the nail so as to be able to use the screwdriver."

There is a slip on p. 114 in the characterization of Transportation Problems (it is not true that *all* a_{ij} are unity), and on p. 136 the requirements should be denoted by c_j in the table, to agree with the text. In lines 1–2 on p. 163 it should be made clear that the b_j have been assumed to be non-negative. Dantzig's method for integer programming, described on p. 189, was only tentative and he stated himself that it might not converge. But apart from such small matters the great care which the authors exercised to collect their material, and to exhibit it with fairness to the originals, and to the readers, is everywhere apparent.

The book can be thoroughly recommended and it is hoped that its successors will reach an equally high standard of scholarship and of presentation.

S. VAJDA

14. Problems in Basic Operations Research Methods for Management. By Randolph W. Cabell and Almarin Phillips. New York and London, Wiley, [1961]. xii, 110 p. $8\frac{1}{2}''$. 32*s*.

All those who have ever lectured on a subject whose essence becomes clear in its applications know the value of a collection of realistic problems which can be solved by methods discussed in class. This is particularly true of problems of operational research, because here the link between actual management situations and mathematical techniques for dealing with them is as yet tenuous and its precise location still subject to pragmatic, semantic and logical doubts. Therefore the aim of this booklet, viz. to make students—future business administrators—realize the need for model building "to fit problems rather than to search for 'cookbook' solutions", is to be recommended. What a pity that the authors chose to stop halfway on their path, and at a point where the going becomes particularly rough.

After an introduction to the nature and techniques of O.R. there are 36 chapters, each having a title such as "Mu Capacitor Company—sequencing". The first part of these titles makes one fear the facetiousness which is unfortunately so common in the more superficial accounts of O.R. cases. But actually the backgrounds to the problems are described quite seriously and adequately, with just that redundancy that is a feature of practical problems as they arise. Then the problem is either stated, or implied, e.g. "What is the probable explanation of the long lines and loss in business?" (in "Phi Oil Company —multiple channel queuing"). The cases refer to mathematical programming, inventory control, queueing, replacement, analysis of variance, and kindred subjects. No solutions, or even mathematical formulations, are given, but an appendix of 19 book titles is provided, and (for 35 out of the 36 problems) references to these books indicate where the student could learn how to proceed. The thirty-sixth case deals with "locations of facilities", an intricate matter where guidance would have been particularly welcome.

I do not understand why the authors have chosen this point to break off their assistance to the reader, who needs it precisely here, but the collection could be of value to teachers in O.R. courses.

S. VAJDA

15. Statistics—with Applications in Management and Economics. By Earl K. Bowen. Homewood, Ill., Richard D. Irwin, Inc., 1960. xi, 415 p. 9″. $6.50; 45*s*.

This book is written for those with no previous knowledge of the subject, and presupposes no formal training in mathematics. It deals very clearly with a useful number of topics, ranging from the traditional, but important, subject of data presentation to simple

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regression analysis and the use of large sample theory. The basic formulae of this are stated without justification. The use of the isosceles distribution in introducing the idea of a probability distribution is helpful and well worth noting.

The emphasis throughout is on the proper interpretation of figures. One example is the section, on page 239, on "same-month-year-ago" comparisons, where Mr Bowen points out the limitations of this very much used measure. The illustrations are taken from the kind of figures that may be met in business administration, and these illustrations point the relevancy of the various methods to business problems, without of course being able to deal with the way in which statistics may be used in business, or to give very much help in deciding the kind of problem where statistical methods are likely to give useful results.

The chapter on sample survey principles is also helpful, but some earlier remarks in Chapter 3 on postal surveys are perhaps more discouraging to their use than current opinions in the U.K. would suggest.

The American origin of the examples detracts little from the usefulness of the book, and it would make an admirable text book for a number of elementary courses. The price of 45s. is a disadvantage here.

J. A. Bound

16. Principles of Economics. By Alfred Marshall. Ninth (variorum) Edition with annotations by C. W. Guillebaud. Vol. 1, Text. Vol. 2, Notes. London, Macmillan & Co. Ltd, for the Royal Economic Society, 1961. 2 vols. $9\frac{1}{4}''$. 120*s*.

Mr Claude Guillebaud has produced a ninth (variorum) edition of that Old Testament for English economists, *The Principles of Economics* by Alfred Marshall. The edition consists of two bulky volumes, each running to about a thousand pages. Volume 1 consists of the text of the eighth edition with note-references to Volume 2: Volume 2 contains the alternative texts from earlier editions in Marshall's own words: it also contains a valuable introduction of 24 pages describing Marshall's life and the background and evolution of *The Principles*. This edition will inevitably be a standard work of reference and every economics library should display one or more copies of this superbly printed work.

Alfred Marshall (1842–1924) was a mathematical economist who passionately desired his work to be understood by and to influence a wide public with little aptitude for mathematics. He knew that the complexity of the economic subject matter, if it were to be treated mathematically at all, would require an apparatus far beyond the comprehension of his public and uncongenial even to himself, since as an outstanding economic historian he was acutely aware of those economic influences and phenomena which are not susceptible to numerical measurement. Yet he did not shirk the complexities of his subject: he wrapped them up in parables and attacked them from many angles, shifting his point of emphasis by means of a series of contrasted illustrative examples, so that his unmathematical readers could travel from page 1 to page 837 under the illusion that it was all quite easy. At the very end he provided a mathematical appendix which revealed just a little bit of the mathematical framework of his theories. But he was very sparing in this because he believed that anything which was stated simply and unambiguously in the form of a mathematical model would mislead the readers, since it must involve unrealistic simplifying assumptions: and so ink is still being spilt trying to discover exactly what he did mean in the more abstruse sections of his work.

His attitude towards statistics was no less austere. Expert in the handling and interpretation of the numerical evidence available in his day, he looked forward to the years when improvements in the available statistics, and developments in statistical methods would enable the elasticities and other concepts in his theories and examples to be provided with numerical estimates. But he would certainly have been horrified by the assurance with which economic statisticians nowadays churn out such estimates and even make predictions on the basis of what is now available. None but Marshall could have admonished the cautious Bowley in the following terms after the appearance of that author's *Elements of Statistics*:

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"In my view every economic fact, whether or not it is of such a nature as to be expressed in numbers, stands in relation as cause and effect to many other facts: and since it *never* happens that all of them can be expressed in numbers, the application of exact mathematical methods to those which can is nearly always a waste of time, while in the large majority of cases it is positively misleading; and the world would have been further on its way forward if the work had never been done at all. . . . I admit exceptions, and no doubt these are already more than I know of, and yet more will be discovered. . . . Surely *the* thing to do is to build the basis of our economic structure soundly and not to put a varnish of mathematical accuracy to many places of decimals on results the premises of which are not established within 20 or 50 per cent."

D. G. CHAMPERNOWNE

17. Soviet Statistics of Physical Output of Industrial Commodities: their Compilation and Quality. By Gregory Grossman. Princeton, University Press; London, Oxford University Press; 1960. [16], 151 p. $8\frac{3}{4}$". $5.60. (Nat. Bur. of Econ. Res. General Series, No. 69.)

The aim of this study is to examine the reliability of the primary reporting, compilation and publication of Soviet statistics of physical output of industrial commodities; the author stresses both in the introduction and in the conclusion that "this is not a comparative study of the quality of statistics in the USSR and other countries. Whether Soviet statistics in general, and the statistics of industrial physical output in particular, are or are not in some relevant respects better than those of another country, remains to be established by a separate investigation." However, the reader of this study inevitably feels, while reading it carefully from the introduction to the conclusion, that, by implication and because of the author's assumptions, comparison is being made all the time, not with particular statistics in a particular country other than the USSR, but with a somewhat abstract ideal statistic. The author works with the assumption that, because the Soviet economy is a "command economy", because statistics are used as "success indicators" in the fulfilment of plans, and because, "therefore, the planners and producers *cannot* [my italics] remain neutral towards them" (pp. 33–34), the function of statistics in the USSR necessarily differs from that in countries without a "command economy", with a neutral collector and publisher of statistics, who collects and publishes them either "for the use of scholars", or for the exercise of indirect controls in a market economy by a government which has "no special stake in the image of its country that is presented to the world at large". "The Kremlin has a monopoly of publication within the Soviet Union and has a compelling strategic, political and ideological interest in the image of the Soviet economy that it presents both abroad and at home. Therefore, we should not be surprised if it publishes statistics that are partial, selective, often deliberately ambiguous, perhaps falsified (in the strict sense of the word) and, as likely as not, misleading." Consequently, the question must always be asked in dealing with Soviet statistics, "What are they trying to prove? How are they trying to mislead the world? What follows from this about the usability of a specific statistical datum?" (p. 2).

The study is in two parts. Part One gives a brief history and a summary description of the Soviet statistical system. It is useful to the English reader as a *general* introduction to the existing institutional machinery for the collection of statistical information; for the seeker after a fuller insight into the function of the statistical system of the USSR, it would, of course, be necessary to consult the Russian sources. The reader must decide for himself whether the description given in Part One justifies the view of the author that, because "interested individuals participate in the generation and reports of the very statistical data on which their performance is judged", because "in a sense, the regional and national statistics of output are largely declared by parties who have a vested interest in them . . . although this effect is limited by fear of the strict penalties imposed for false reporting as well as by various institutional checks discussed below, this fact [vested interest–A. B.] is perhaps the greatest defect in the Soviet statistical system, and perhaps the strongest reason to

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suspect the reliability of the published physical output data" (p. 24). In my opinion, the author has not proved his assumptions about the characteristic behaviour of Soviet statisticians and general defects in the Soviet statistical system in spite of the fact that the Soviet press publishes a great deal of critical material about specific defects in the collecting of statistical data and in the functioning of statistical institutions, as well as reports of infringements (including criminal ones) of instructions on the reporting of statistical data by individuals.

Part Two of the study is devoted to a critical description of the process of collecting physical output data. The author analyses in detail various kinds of "motivation" for both "writing up" and under-reporting industrial output. He quotes statements from the Soviet press and from defectors from the Soviet Union, adducing individual cases of such behaviour, generalizing with such qualifications as "may be", "undoubtedly often", and so on, and coming to the conclusion: "In short, the Soviet record of suppression of production data and juggling of published categories does indeed suggest that these devices are employed as alternatives to outright numerical falsification at publication, although, needless to say, this certainly does not preclude the *possibility* [my italics] of such falsification" (p. 111) and "I should add that I know of no instance where a Soviet industrial *physical* output datum had been clearly demonstrated to be inconsistent with other information" (p. 112).

The author comes to the general conclusion that the evidence for the "presence of distortion, including falsification is largely non-quantitative and in considerable measure even aprioristic; in the few cases where it is quantitative, it is very fragmentary. Thus, while we can draw certain qualitative conclusions (that distortion exists, that in certain branches and at certain times it is likely to be greater or smaller than in other branches or at other times, that certainly it is in one direction or the other, and so forth), we are not able to estimate the quantitative extent of distortion in the information reported to the central authorities" (p. 128). To this conclusion I should like to add the rider that, if Mr Grossman's elaborate array of scholarly techniques leads to this conclusion as regards the impossibility of a quantitative assessment of the degree of distortion of information *reported* to the central authorities, how unprofitable from a scholar's point of view must be the attempts at recalculating *published summary* indices indulged in by some other "Sovietologists"! This particular study, in spite of all its scholarly merit as regards the techniques used in the analysing of available material, must necessarily be of very limited practical value—either to the statistician of industrial output in general or to the specialist in the study of the economy of the USSR. It gives useful methodological warnings about pitfalls in using statistics published in the Soviet Union for those unaware of their composition, but these are already known to serious students of its economy. It is difficult to avoid the doubtless commonplace reflection, on putting down this thoroughly competent work, that even the acutest minds and most serious scholars pay their unconscious homage to the climate of their day and age.

ALEXANDER BAYKOV

18. The Corporate Sector in India: A Factual Presentation of Long and Short-term Trends. By Raj K. Nigam and N. C. Chaudhuri. New Delhi, Government of India, Department of Company Law Administration (Research and Statistics Division), 1960. xiii, 194 p. 9½″. 15.50*R*.

This compilation brings together a mass of mainly statistical data relating to the growth, trends, organization, management and finances of Indian joint stock companies and corporations, in the public and private sectors alike.

There are 21 sections (or chapters) and 20 appendices. The latter consist of the detailed facts and figures which underlie the summaries given in the various sections.

The first two sections provide an illuminating picture of the position and importance of the corporate sector as related to national output, savings, capital assets, planned investment, etc., and illustrate very clearly the rapidly growing importance of corporate organization in India since 1938. Sections III to VIII classify and analyse in various ways

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the members of the corporate sector: e.g. by structural pattern, classes and types of companies, governmental *versus* non-governmental companies, and industrial pattern. Sections IX and X deal respectively with the State-wise distribution of companies, and trends in new company registrations (State-wise, size, types). Section XII picks out the 100 largest corporations, and ranks them in relation to gross sales, profits, assets, etc. Sections XIII to XVI deal with foreign companies at work in India and their subsidiaries; managing agencies; and trends in the sources and uses of company finances. Later sections deal with the various indices of industrial profits, the finance of governmental companies, stock exchange quotations, and the industrial and country-wise breakdowns of foreign business investments in India. Many of the summary tables and figures have their sources in extensive work undertaken previously by Dr Nigam.

The volume provides a most valuable and concise reference work for all interested in the economic and financial aspects of the domestic Indian corporate sector, and in the extent and nature of the operations of private foreign investors in India. The admirably brief comments on the statistical data highlight, most helpfully, both the characteristic features of, and trends in, each aspect of the corporate sector.

VERA ANSTEY

19. Demographic and Economic Change in Developed Countries: Conference of the Universities—National Bureau Committee for Economic Research, New York. Princeton, University Press, 1960; [London, Oxford University Press]. xii, 536 p. $8\frac{3}{4}''$. £4 16*s*. (National Bureau of Economic Research, Special Conf. Ser. 11.)

This volume contains a series of papers read at a joint conference of demographers and economists in 1958. It is edited by Professor Ansley J. Coale, the Director of Princeton's Office of Population Research, who also contributes an introduction. The book is divided into two parts, the first entitled "The Analysis of Population Change", written by demographers: the second "The Economic Effects of Population Change" by economists and econometricians. The contributions vary widely in nature and subject matter; from pure demographic analysis to technical econometrics interspersed with sociology and applied economics. It is impossible for any one reviewer adequately to assess the significance of all the papers, and this review will therefore largely confine itself to the first half of the book.

A few words may, however, be permissible about the book as a whole. A great deal is often said about the exciting possibilities of interdisciplinary meetings. Cross-fertilization of ideas, a redrawing of the map of knowledge, and similar phrases abound. If any such processes took place as a result of this conference, they are not apparent in the book. Demographers were the discussants of the demographic contributions, economists of the economic. It is, of course, possible that much discussion went unrecorded, but one cannot help being impressed by the dichotomy of approach between the two groups. It is presumably desirable for the economists to know what the demographers are working at, and *vice versa*, but it is difficult to believe that the conference achieved much more. With one or two exceptions, demographers and economists appeared to be talking a different language. This is almost admitted by Professor Coale, himself one of the few participants who appeared to have a foot in both camps, and it is only the exceptional individual who will wish to read the whole of the book.

On the economic side, there are papers in which various aspects of population change are related to demand, price and the level of employment (by Ansley J. Coale), the supply of labour (by S. Lebergott), the demand for food (by Jean Crockett), the demand for services (by Robert Ferber) and to aggregate output (by S. Kuznets). H. J. Barnett of "Resources for the Future, Inc." writes on Population Change and Resources, and Guy Orcutt and Alice Rivlin give a progress report on the construction of an econometric model of the household sector. In this section, all the factual data refer to the U.S.A., but the papers by Kuznets and Barnett are somewhat more theoretical than the others. It is interesting to note that neither of them seems particularly alarmed by high rates of growth

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in the U.S.A., either in relation to output or to the spoliation of natural resources. They are optimistic about future technical progress resolving at least some of the Malthusian dilemmas.

Of the nine demographic papers, seven are again devoted more or less exclusively to American problems. The two exceptions are contributions by two members of the U.N. staff: Halvor Gille's survey of recent European fertility trends, and Gwendolyn Johnson's assessment of European differential fertility surveys. Indeed, the problem of differentials is given much attention in the book. Miss Johnson shows that in many European countries there now exists a positive relation between fertility and educational status, and in another contribution Richard and Nancy Ruggles demonstrate that this also holds true of those American women who have had at least four years of high-school education. But there are other results. Clyde Kiser admits that urban-rural differences in fertility have narrowed in the U.S.A., but implies that this may be due to the changing composition of the population, for income and occupational differences persist. In another paper Norman Ryder is principally concerned with the problem of translating period rates into cohort rates on the assumption that the latter are a linear function of time, and when he comes to apply his method to U.S. data he concludes that the variance of family size distributions has declined.

Are we then to conclude that the pattern of differential fertility with which Western demographers have been familiar for the last 75 years is purely a phenomenon of the demographic transition? Professor Ronald Freedman comes near to taking this view, when he talks about "the disappearance of traditional fertility differentials" and their replacement by new types of differentials, e.g. between organization men and others. The implications of his remarks should be read in conjunction with David Goldberg's assessment of three of the principal recent American studies of fertility.

Other papers in this section include one by Dudley Kirk on "The Influence of Business Cycles on Marriage and Birth Rates", one by F. W. Notestein on "Mortality, Fertility, Size-Age Distribution and the Growth Rate", and an attempt by Gary S. Backer to explain fertility variations in terms of an analysis of the cost of and demand for children.

This short résumé will give an idea of the contents of the book and it should be emphasized that the individual contributions are all of a high order of merit. But the total effect is discouraging. I am reminded of the conclusion of a course of lectures on the theory of price, which I attended as an undergraduate. At the end of the course in which we were introduced to all the refinements of modern economic analysis, the lecturer concluded: "Do you know now what determines the price you have to pay for a bicycle? I don't." *Mutatis mutandis*, the same sentiments apply here.

E. GREBENIK

20. Social Security Statistics. Material collected by the Ministry of Pensions and National Insurance and the National Assistance Board. Interdepartmental Committee on Social and Economic Research. London, H.M. Stationery Office, 1961. viii, 171 p. 9½″. 8*s*. (Guides to Official Sources: No. 5.)

Britain has nothing to compare with the American monthly *Social Security Bulletin*, with its flow of current statistics and special surveys. But we now have what is, within its limits, a very useful guide to the main statistics, published or otherwise available, about our social security services between 1953 (war pensions) or 1948 (other services) and 1960 (stopping, that is, just before graduated contributions and pensions were introduced). The *Guide* comprises a detailed subject index to published statistics, a group of chapters describing the organization and content of the services and what statistics are collected and how, and appendices summarizing contribution and benefit rates and legislative changes of statistical import since 1939 (war pensions) and 1948 (other services).

The *Guide's* limits are somewhat narrow and not wholly consistent. In content the *Guide* does not cover the whole range of material collected by the agencies concerned. It

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concentrates chiefly on their serial statistics, though even here it notably omits any reference to statistics about such topics as staffing and organization, home visiting of receivers of sickness benefit or their examination by regional medical officers, prosecutions, or the war pensions welfare service. Nor are non-serial statistics wholly excluded, with puzzling results. The subject index records the Ministry's 1953 survey into reasons for retiring or not retiring at pension age, while the text mentions the *ad hoc* surveys of special topics which the Board publishes in its annual reports. But there is no mention of the equally notable *ad hoc* enquiries conducted for the Phillips Committee on pensions or for the National Insurance Advisory Committee on various questions (e.g. into earnings limits and widows' benefits). And though the forward estimates and special surveys contained in the Government Actuary's quinquennial insurance reviews are listed, his similar reports on Insurance Bills and his survey of occupational pensions (1958) are not. (Official surveys of health service, university and school, local government and other pension schemes, although containing relevant social security statistics, are omitted, presumably as beyond the *Guide's* scope.)

The *Guide's* time-span is not too happily chosen. The main information about war pensions runs only from 1953, when war pensions and national insurance were merged into one Ministry, but other information is given either from 1918 or from 1939 onwards. For national insurance and assistance, on the other hand, both text and sources start in 1948. There are no references to material on the 1946–48 transition years, to the financial and actuarial data on which the legislation following the Beveridge report was founded, or to the social security schemes which preceded that legislation, whether during the war or for a pre-war base year such as 1938. Nevertheless, the subject index singles out one pre-war survey, a paper by the Government Actuary published in this *Journal* in 1927, while excluding many other surveys which might have been mentioned.

Within its self-imposed limits, however, the *Guide* should help students of social trends to find their way to the rarer and little exploited sources, especially on sickness, industrial injury and pensions. It also brings to light some of the deficiencies of existing published statistics, as can be seen from the following two examples: (1) Not much is published about the extent to which benefit recipients (especially the sick) have child or adult dependants, and there is little or no co-ordination of the dependency statistics derived respectively from the insurance, assistance, war pension and family allowance systems; (2) these services commonly provide age-group statistics about their clients and not uncommonly regional statistics too, but scarcely ever a combination of age and regional grouping. It is not possible, for instance, to compare trends in relieved poverty at different ages in Sheffield and Birmingham (or the East and West Ridings and the Midlands) by relating the numbers of assisted persons of different ages in these areas to the relevant age groups in the local populations. Nor is it possible to ascertain, in addition, what proportions of various age-groups in different cities or regions are not on assistance but are drawing widowed mothers' allowances or retirement pensions, with or without supplements for dependants.

FRANÇOIS LAFITTE

21. The Chosen Few: An Examination of Some Aspects of University Selection in Britain. By W. D. Furneaux. London [etc.], Published for the Nuffield Foundation by the Oxford University Press, 1961. 245 p. 8½″. 25*s*.

The use of statistics and the social sciences in critical assessment of social and economic affairs is an established feature of English public life. The reasons for the considerably increased emphasis on education in this type of research since the war are not far to seek. We are living through a period of unprecedented educational expansion which began with the transformation of the intake to grammar sixth forms from a trickle to a flood in the fifties and which will continue with the expansion of higher learning in the sixties and seventies. Study of the first phase of expansion gave us the *Early Leaving* and *Crowther* Reports as well as an accumulation of private research in the political arithmetic tradition. The problems of the second phase promise us the *Robbins* Report, have given us the *Kelsall*

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enquiry and have now delivered a first book on Mr Furneaux's investigations into university selection which began in 1948 under the patronage of the Nuffield Foundation.

Post-war expansion has completely changed the nature of admission to the British universities. Before the war a niggardly provision of secondary education and of scholarships to higher education resulted in the universities having no selection problem at all. They stipulated their matriculation requirements and watched anxiously at the gate every autumn.

Today there are, with multiple applications, anything from two to twenty applicants for every place—and here is the beginning of Mr Furneaux's problem which, in this book, he gives a general if patchy treatment. Some valuable discussion of the validity of selection techniques in common use is included and answers are offered to such questions as that of the weight to be attached to social and educational factors in selection before the end of the secondary stage of schooling, the possibility of expanding the number and quality of applicants for university places, the effect of the growing practice of multiple application and the organization of a central clearing house.

The patchiness of the treatment is apparently due to the fact that this is not the book that Mr. Furneaux intended to write. The Nuffield enquiry was designed to discover how far intelligence and similar tests might be useful in selection for British universities, but the results of this investigation have yet to appear. What we have here are some by-products of the main enquiry. This is not to discount their value both to those interested in the practical problems of selection and the urgent issue of the clearing house and, more generally, to those who must face the intricate mysteries of using inadequate official statistics for research purposes (e.g. the Ministry of Education's infuriating custom of publishing statistics from which it is impossible to discern the indispensable base line of the exact intake of pupils into the various types of school in a given year), and the pitfalls and obstacles that attend direct enquiry by private survey (e.g. the difficulties of getting co-operation from Oxford and Cambridge which Mr Furneaux found insuperable and which cast considerable doubt on the appropriateness of his book's title).

Unfortunately, however, these unavoidable embarrassments have been further compounded by an unbelievably bad organization of the book itself. It is almost as if Mr Furneaux never completely accepted the fact that he was writing it. He begins with the conclusion: he then writes a useful little book of a hundred pages on the selection problem: he adds another summary and conclusion which partially overlaps the first and then starts all over again, changing chapter numbers from Arabic to Roman form. And finally, for good measure in a *tour de force* of confusion, he distributes appendices, acknowledgements and other courtesies from start to finish. "Ideally," he writes (p. xv), "a book should be directed to a clearly defined audience and should have a definite objective." He might well have taken his own advice and written a straightforward account of the problem, his method, his findings and their interpretation for an audience defined as those interested in university admissions. In its present form both the admirably meticulous analysis of his data and the often bold pertinacity of his conclusions will be lost by the reader between page and thumb.

Leaving aside its form of presentation, the book may be looked on as a contribution to two main problems. It deals first with the general context of university selection as the final stage of a process of social and educational sifting which impinges on the individual from birth. Second, it offers a critical analysis of current practices in university selection.

For the first problem Mr Furneaux uses his own data to improve on the findings of the *Early Leaving* Report. He does not use the more recent *Crowther* Report and ignores nearly all the relevant private research into the social determinants of educational achievement. He confirms the well-known association of the selective process with social class, demonstrates that this broad influence has done its work by the time that children reach the upper sixth form and presents new findings concerning the relation of sex and social class to motivation towards further education. He shows that the provision of courses leading to five G.C.E. papers for the most able half of the entry to secondary schools, together with

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the elimination of premature and early leaving among the able, could lead to a situation where 7 per cent. of all boys and 4 per cent. of all girls would be potential applicants for university places. Some readers will dispute these proportions as too low, and further argument would turn on a discussion of the weight of remediable environmental (including educational) as opposed to innate limitations on the quality of the British population into which Mr Furneaux does not enter. He is fairly confident that the supply from the schools will be adequate to meet future requirements for university-trained manpower. But for the demand side of the equation he accepts current forecasts without discussion. If American experience is any guide (and it shows that supply and demand are not independently determined), some critics will want to raise the quantities on both sides of the equation.

The basic figure for beginning an appraisal of the selection methods used by the universities is Mr Furneaux's calculation that 84 per cent. of all qualified applicants to universities in the United Kingdom are probably able to gain admission. The whole problem, therefore, turns on the validity of the methods used to eliminate the minority of border-line candidates. On this Mr Furneaux has some findings and observations of forthright relevance. He makes a highly successful attack on the amateur incompetence of university selectors and on the low validity and reliability of the interview and the headmaster's letter. It is clear that G.C.E. marks are the best of a poor lot of predictors of university success. No doubt there will be complaisant resistance to his firmly based suggestion for better record keeping and for the availability of expertise on selection procedures in all universities. On the other hand, he surely overstates his case in arguing that G.C.E. marks are typically given little or no weight in admissions decisions. It is true that the decision in most cases has to be made before G.C.E. results are available: but do not most admissions tutors fix a minimum G.C.E. performance on the basis of last year's experience such that, in the annual August agony, those provisionally accepted compete at the margin with the "pool" candidates specifically on their "A" level results?

Finally Mr Furneaux makes out a persuasive case for a central clearing house and suggests an organization in detail which merits thorough scrutiny alongside the other proposals which are at present under discussion in the universities.

A. H. Halsey

22. Housing and Economic Progress: A Study of the Housing Experiences of Boston's Middle-income Families. By Lloyd Rodwin. Cambridge, Mass., Harvard University Press and the Technology Press, 1961. [London, Oxford University Press.] x, [6], 228 p. 9¼″. 60*s*.

If they are to be of much practical value, studies of housing must deal with a tangle of related economic, social, political and technological problems; if they are to be reliable they must deal only with the few topics on which accurate data are available. Professor Rodwin's attempt to resolve this dilemma consists of a series of self-contained essays on various aspects of the development of housing in one city: the growth of housing for Boston's "middle-income" families, the relation between incomes and rents for these families (from 1846 to 1959), an analysis of the effects of rent control derived from a comparison of Boston's experience after the first world war (without rent control) and the second world war (with rent control), a social and economic history of the development of housing in Boston, and some solid appendices on theoretical and methodological problems.

The statistical data for these studies are fuller and run further back in time than the data available on comparable British cities. Although Professor Rodwin has occasionally to resort to some heroic estimations, the path by which he arrives at them is always clearly and honestly explained. He argues that the study of the middle-income groups throws light on crucial issues that tend to be neglected in studies of the extreme groups—particularly the growth of owner-occupation, the decay of prosperous neighbourhoods and the development of new forms of low-cost building and new institutions for the

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finance of house purchase. If attention must be focused on limited sectors of the market, this choice is a good one; but many will find that the later chapters which deal with the whole city are the most revealing in the book. The findings of these studies are sufficiently intriguing to make the reader regret the gaps in them—particularly the lack of data on age, sex and marital status, and the relation between changes in demographic structure and the city's residential development—but such gaps probably reflect the limitations of the data available.

The essays are linked, implicitly rather than explicitly, by two threads. One is the search for a general theory to explain the development of cities. Professor Rodwin successfully demolishes much of the theory erected so far, but he is far too honest an empiricist to produce a comprehensive alternative to it. Oddly, he seems convinced that this should be possible. His own brand of sceptical liberalism is the second thread that runs through the book. It enables him to strip away the veneer of scientific-sounding exposition with which others have cloaked their essentially political commitments to maintain "free enterprise" or racial segregation.

This is the second publication of the Joint Center for Urban Studies of the Massachusetts Institute of Technology and Harvard University. We look forward to further instalments of the Center's work.

D. V. Donnison

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Current Notes

United Kingdom Official Statistics

1. *Input-Output Tables for the United Kingdom, 1954*

Input-Output Tables for the United Kingdom, 1954 (H.M.S.O., 7*s.* 6*d.*) was published on July 27th, 1961. The booklet has been prepared jointly by the Board of Trade and the Central Statistical Office, and is based largely on an analysis of the data contained in the detailed *Census of Production* for 1954 and the *National Income Blue Book* for 1960.

The input-output tables provide details of the purchases and sales of 46 different industry groups, of which 38 are in manufacturing industry. Convenient summaries of the detailed figures are also given for 12 major industry groups. Some of these summary tables are similar to those previously published in the *National Income Blue Book* for 1958, but the large detailed tables giving information for 46 separate industry groups are new. For each industry group, figures are given of its purchases from each of the other industry groups and from abroad, of its sales to each of the other industry groups, and of its sales for final consumption by personal consumers and public authorities and its sales for investment and exports.

The various input-output relationships, both direct and indirect, are set out in 17 tables and are accompanied by 30 pages of text, which describe them and explain their uses and limitations and the sources and method used in compiling them.

The figures in the tables relate to 1954 as this is the latest year for which detailed information about both output and input is available for manufacturing industries. Similar information will again be collected in the next detailed *Census of Production* for 1963.

2. *Protective Duties*

As noted in an earlier issue of this *Journal* (Volume 123, Part 4, 1960), the Commissioners of Customs and Excise are issuing a new series of annual volumes of statistics entitled *Protective Duties* 19..: *Values or Quantities of Imported Goods Entered for Home Use and Receipts of Duty.*

The 1960 volume will follow the same lines as the 1959 volume, but its contents will be amplified to set out the information mentioned in the previous note in the following detail:

(a) The value or quantity admitted free under Commonwealth Preference;
(b) The value or quantity duty-paid at Commonwealth Preference rates, and the related net receipts (this information was in fact indicated by footnotes in the 1959 volume; see, for example, page 145);
(c) The value or quantity duty-paid at E.F.T.A. rates, and the related net receipts;
(d) The value or quantity admitted free under E.F.T.A. arrangements. (Until the items concerned become more numerous, this information will be specially indicated by footnotes);
(e) The value or quantity duty-paid at full rates of duty, and the related net receipts.

3. *Ministry of Power Statistical Digest, 1960*

The 1960 edition of the *Ministry of Power Statistical Digest* (H.M.S.O., 27*s.* 6*d.* net) follows broadly the pattern of the last issue. Points of difference are: (a) Changes have been made to the treatment of statistics of imports and exports of petroleum. Previously the quantities of oil recorded in gallons by H.M. Customs and Excise in the published *Trade and Navigation Accounts* have been used throughout, being converted to tons by means of standard conversion factors associated with measurements taken at a temperature of 50° F. In the 1961 edition, the Customs and Excise figures have no longer been used

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for summary tables, being replaced in Tables 106, 107 and 108 as from 1948 with figures of arrivals, shipments and bunkers provided by the importers and exporters of oil. In the more detailed Tables 109 to 120 the Customs and Excise figures are still used, but they have been converted from gallons to tons by conversion factors supplied for the purpose by the United Kingdom Petroleum Industry Advisory Committee; (b) Information about the use of pulverized fuel, last published in the 1955 *Digest*, has again been collected and is summarized for the year 1960 in Tables 62, 63 and 64. The last table, which shows the numbers and types of boilers using this fuel, is a new feature; (c) Figures of disposals for domestic purposes of specially re-active smokeless solid fuels are included for the first time; (d) The analyses of industrial coal consumption and of industrial use and stocks of liquid fuels for burning are now based on the revised Standard Industrial Classification; (e) A distribution of industrial establishments classified according to size of coal consumption in 1960 is given in Table 48; a similar table relating to the year 1955 and earlier was published in the 1955 *Digest* (Table 78); (f) For the first time, a detailed breakdown of deliveries of petroleum products has become available in respect of England and Wales combined, and Scotland and Northern Ireland separately. This information is reproduced in Table 123 for the years 1956 to 1960; (g) The new edition omits a few tables in the last edition which are not thought of sufficient interest to warrant inclusion every year and includes a few such tables not in the last edition. The table (40 in the 1959 edition) showing the value of production of minerals other than coal has been discontinued.

4. *Scottish Agricultural Economics*

Scottish Agricultural Economics, Vol. XI, 1961, published by the Department of Agriculture and Fisheries (H.M.S.O., 4*s*.), contains, in addition to current estimates of the value of agricultural output and of the main outlays by Scottish farmers, a new index of net output using the years 1954/55 to 1956/57 as a base and deducting from gross output all inputs bought from outside the agricultural sector (not, as previously, merely imported feed, store cattle and seeds). This new series is therefore akin to the index of industrial production and the U.K. index of agricultural output published in the *Annual Review White Paper*.

A note entitled "Scottish Farm Population" analyses data collected from a substantial representative sample of farms. Of the 317,000 people living on Scottish farms in 1960 (approximately one-sixteenth of the total population), 113,000 live in farmers' households and the balance in cottages, etc. Two-thirds of the adults living in the farmers' households were engaged in, or dependent on, farming while the proportion for those living elsewhere on the farm was four-fifths.

Data from the same sample show the extent to which occupiers of farms are engaged in farming. Of the 54,000 occupiers of farms of all sizes, some 28,000 occupiers are engaged full-time in farming; 10,000 occupiers are engaged solely in farming but not on a full-time basis; 1,000 are mainly engaged in farming but have some other activity, while 15,000 are mainly engaged in some other work. In the latter group over 70 per cent. of the occupiers were primarily engaged in work which had no connection with farming.

An article on farming rents and estate expenditure is based on a large body of information collected mainly by survey in the field during 1958/59. Many tables set out data on rents per acre, reasons for rent changes, methods of fixing new rents and expenditure on maintenance and improvements. The analysis is chiefly by type and size of farm.

United Nations Statistical Publications

1. *Standard International Trade Classification, Revised.* English (French and Spanish in press). Statistical Papers, Series M, No. 34. 135 p. U.S. $2; 14*s*. stg.; Sw. fr. 8.50.

The Standard International Trade Classification (SITC) was recommended to Governments by the United Nations Economic and Social Council in 1950 as a means

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of achieving international comparability in trade-by-commodity statistics on the basis of categories of economic importance. By 1960, countries accounting for about 80 per cent. of world trade were compiling data according to the SITC. In 1960, the United Nations Statistical Commission authorized a revision of the SITC to take account of structural changes since 1950 in the commodity composition of external trade and to present the SITC as a re-arrangement of the items of the 1955 Tariff Nomenclature (BTN) of the Customs Cooperation Council (Brussels), which had been adopted for customs purposes by a number of countries, particularly in Europe. This new publication sets forth the SITC, Revised, showing its relationship to the BTN and to the original SITC. An appendix lists the BTN headings and gives the code number of each in the SITC, Revised.

2. *Report on the International Training Course on Vital and Health Statistics for the Western Pacific Region, Manila, Philippines, 17 October to 25 November, 1960.* English (French, in press). Statistical Papers, Series M, No. 35. 49 p. U.S. $0.75; 5*s.* stg.; Sw. fr. 3.

This is the final report on a training course sponsored and organized by the World Health Organization (Regional Office for the Western Pacific) and the United Nations (Bureau of Technical Assistance Operations and the Statistical Office). It contains sections on the background, objectives and activities of the course and on the appraisal of results. The appendices have information on the teaching staff and special lecturers, books and documents distributed, participants and observers, weekly programmes of work and the demographic and health survey carried out during the course.

3. *International Standard Industrial Classification of All Economic Activities.* Statistical Papers, Series M, No. 4, Rev. 1. (English, French and Spanish.) U.S. $0.30; 2*s.* stg.; Sw. fr. 1.25.

A Russian translation of this work (97 pp., mimeographed) was issued in June 1961; copies may be obtained, free of charge, from the Statistical Office of the United Nations, New York.

United Nations Input-Output Conference

An International Conference on Input-Output Techniques was held at the Palais des Nations, Geneva, from September 11th to 15th, 1961. The Conference, with approximately 250 participants from almost 40 countries, was organized by the Economic Research Project of Harvard University, in association with the United Nations Secretariat. This was the third international conference on input-output problems and techniques, the first having been held in the Netherlands in 1950 and the second in Italy in 1954. The increase in attendance and in geographical representation at these periodic but independent meetings reflects an ever-widening interest in input-output as an economic and statistical tool.

Approximately thirty papers were presented on the following four subjects: economic prognostications—economic planning; use of input-output techniques in partial analysis; dynamic models; problems of standardization of input-output statistics—classification and aggregation. A selection of the papers will be published after revision by an editorial committee consisting of Tibor Barna, National Institute of Economic and Social Research, London (Chairman); William I. Abraham, Statistical Office of the United Nations; W. Duane Evans, United States Bureau of Labor Statistics; Zoltán Kenessey, Hungarian Central Statistical Office.

United Nations Industrial Seminar

The Seminar on Industrial Statistics for Asia and the Far East was held in Bangkok, Thailand, from September 7th to 23rd, 1961. The Seminar was sponsored and organized by the Economic Commission for Asia and the Far East (ECAFE) and the Statistical Office of the United Nations, in cooperation with the Bureau of Technical Assistance

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Operations, and was attended by 29 participants from 18 member and associate member countries of ECAFE. The papers and the discussions at the Seminar dealt with the objectives, contents (e.g. field to be covered, items of data to be sought and definitions for these items, tables to be prepared) and methods of collection and compilation of the different enquiries (i.e. monthly or quarterly, annual and less frequent, into small and large mining, manufacturing, construction and electricity- and gas-producing units) that should make up a system of industrial statistics. The Seminar was primarily organized to provide an opportunity for an exchange of experience and views among technical personnel from the various countries of the ECAFE region working in the field of industrial statistics, as one way of assisting the development of the industrial statistics that are required by the countries for planning and assessing their economic development and for other purposes, and that are needed, on a comparable basis, internationally. The Seminar reached a consensus on a number of those questions and, in particular, on an adaptation of the United Nations Statistical Commission's recommendations on the 1963 World Programme of Basic Industrial Statistics to the circumstances of the countries of the ECAFE region. All countries represented at the Seminar are starting or extending enquiries in the field of industrial statistics and practically all of them are already planning to make comprehensive enquiries as parts of the 1963 World Programme.

International Statistical Institute

The thirty-third session of the International Statistical Institute was held in Paris, at the invitation of the French Government, from August 29th to September 7th, 1961. About 440 persons participated, of whom 160 were members of the Institute; the British contingent was about 50. The proceedings were opened by M. Debré, Président du Conseil, and followed by an address from the President of the Institute (M. Boldrini) in which he dwelt on the contribution of French scholars to the theory of probability.

A large and varied programme had been drawn up. Over 140 papers were submitted which were allotted to a series of "meetings" as follows:

(1) Model building; (2) Time series analysis; (3) Social statistics; (4) Economic statistics; (5) Distribution functions in biology; (6) Data processing; (7) Statistical theory; (8) Food availability and nutrition statistics; (9) Life testing and reliability; (10) Applied statistics training for industry in European countries; (11) Design of experiments and variance analysis; (12) Traffic congestion; (13) Statistical aspects of urban research; (14) Production, income and financial flows; (15) Miscellaneous application of statistical methods; (16) Present status of statistics in the sciences; (17) Industrial applications of statistical methods; (18) Smoking and lung cancer and associated questions.

The first meeting was a joint one with the Econometric Society, and the fifth and eleventh were joint meetings with the Biometric Society.

During the session, the centenary of the Société Statistique de Paris (founded 1860) was celebrated; three communications were presented by prominent French statisticians and speeches of welcome were made on behalf of other statistical societies, Dr M. G. Kendall speaking on behalf of the Royal Statistical Society as its President. The celebration concluded with an evening excursion on the Seine.

At the administrative meeting, Sir Harry Campion was elected to fill one of the vacancies of Vice-President of the Institute, and an invitation from the Canadian Government to hold the next session in 1963 in Ottawa was accepted.

Stochastic Analysis Group

The Stochastic Analysis Group (Chairman, Professor G. E. H. Reuter, Durham University) was formed by the 20 participants in a meeting on probability theory held in Oxford on December 20th, 1961, with the following objects:

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(i) to promote the study of those branches of pure mathematics having application to statistical science;

(ii) to promote cooperation between mathematicians and statisticians in the British Isles.

The programme at the December meeting consisted of six one-hour papers, as follows:

Dr S. J. Taylor, "Sample paths of stable processes";
Mr J. F. C. Kingman, "Skeletons";
Professor S. Orey, "Semi-Markov processes";
Mr J. G. Mauldon, "Extreme points of some sets of doubly-stochastic matrices";
Mr D. Williams, "Random time substitution in denumerable Markov processes";
Mr D. Vere-Jones, "Recent work in the U.S.S.R.".

It is proposed to hold further meetings at approximately six-monthly intervals, and it is hoped that some of these will be associated with meetings of other mathematical and statistical bodies. By the kind permission of Professor E. T. Davies, the second meeting of the Group will take place at Southampton, on Saturday morning, April 14th, 1962, immediately after and in association with the Southampton meeting of the British Mathematical Colloquium.

The following programme of invited papers has been arranged:

9.30 a.m. Professor H. E. Daniels (Birmingham): "Permutation expansions".
10.30 a.m. Professor G. Baxter (Minnesota and Aarhus): "Recent results in the combinatorial analysis of sums of random variables".
11.30 a.m. Dr L. Mirsky (Sheffield): "Results and problems in the theory of doubly-stochastic matrices".

All those interested will be welcome, but it is hoped that as many as possible will take part in the British Mathematical Colloquium as a whole; lectures and "splinter-groups" on stochastic analysis will form part of the Colloquium programme.

Those who want accommodation on the Friday night (whether or not they intend to take part in the Colloquium meetings) should register with the Honorary Secretary of the Colloquium, Dr F. Rhodes (Mathematics Department, The University, Southampton), saying that they hope to attend the meeting of the Stochastic Analysis Group, and stating whether they will require lunch on the Saturday.

Those who do not want accommodation on the Friday night should register with the Honorary Secretary of the Group, Mr D. G. Kendall (Magdalen College, Oxford), stating whether they will require lunch on the Saturday.

Honours

We congratulate the Fellows named below who were included in the New Year's Honours List:

R. M. Fraser	Knight Bachelor
G. H. Daniel	C.B.
C. T. Brunner	C.B.E.
H. Kennewell	O.B.E.

Appointment

Mr R. L. Plackett has been appointed to a newly created Chair of Statistics in the Newcastle Division of the University of Durham.

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List of Books received for Review

(Available to Fellows in the Society's Library)

Alexander, Howard W. Elements of mathematical statistics. New York, London, Wiley, [1961]. xii, 367 p. diagrs. 9″. 64*s*.

Australia. *Bureau of Agricultural Economics.* The coffee industry in Papua—New Guinea: an economic survey. Canberra, 1961. [8], 143 p. tables. 13″. n.p.

Australian Mathematical Society. Research reports of the Summer Research Institute of the Australian Mathematical Society, 1961. Part I. Probability and statistics. [Canberra, 1961.] 73 ls. 13¼″. n.p.

Bailey, Norman T. J. Introduction to the mathematical theory of genetic linkage. Oxford, Clarendon Press, 1961. x, 298 p. 9¼″. 55*s*.

Barclay, George W. Techniques of population analysis. New York, Wiley, London, Chapman & Hall, [1958]. xiv, 311 p. 9″. 38*s*.

Bartlett, M. S. Probability, statistics and time: an inaugural lecture delivered at University College, London, 15 May, 1961. London, H. K. Lewis, 1961. 20 p. 10″. 2*s*. 6*d*.

Bennett, Merrill K. & Peirce, Rosamund H. Change in the American national diet, 1879–1959. *Stanford University Food Research Institute Studies* (1961), *II*, 2. 95–119 p. tables, diagrs. 10″. $1.

Bhatt, V. V. Employment and capital formation in underdeveloped economies. Bombay, [etc.], Orient Longmans, 1960. [8], 127 p. tables. 8½″. *RS*10.

Cayley, Arthur. An elementary treatise on elliptic functions. 2nd ed. New York, Dover, [1961]. London, Constable. xiv, 386, 15 p. 8″. 16*s*.

Charnes, A. & Cooper, W. W. Management models and industrial applications of linear programming. New York, London, Wiley, [1961]. xxiv, 467, 4 p. diagrs. 10″. 94*s*.

Clement, A. G. & Robertson, Robert H. S. Scotland's scientific heritage. Edinburgh, London, Oliver & Boyd, 1961. [8], 151 p. 8½″. 18*s*.

Council of Europe. Statistical data: population, employment, agriculture, industry, trade, transport, external trade, finance, 1960. [Strasbourg], 1961. 459 p. 9½″. 30*s*.

Cox, D. R. & Smith, W. L. Queues. London, Methuen, 1961. xii, 180 p. diagrs. 7¼″. 21*s*.

Cox, Richard T. The algebra of probable inference. Baltimore, Johns Hopkins Press, London, Oxford University Press, [1961]. xii, 114 p. 9″. 40*s*.

David, F. H. & Pearson, E. S. Elementary statistical exercises. Cambridge, University Press, 1961. viii, 108 p. 10¼″. 13*s*. 6*d*.

Dewhurst, J. Frederick, & *others*. Europe's needs and resources: trends and prospects in eighteen countries; [by] J. Frederick Dewhurst, John O. Coppock, F. Lamartine Yates, and associates. New York, Twentieth Century Fund, 1961. London, Macmillan & Co. xxvi, 1198 p. tables, diagrs. 10″. 84*s*.

Dresher, Melvin. Games of strategy: theory and applications. London, Prentice-Hall International, [1961]. xii, 186 p. diagrs. 9″. 42*s*.

Eisenhart, Luther Pfahler. Continuous groups of transformations. New York, Dover, [1961]. [London, Constable.] xiii, 301, 15 p. 8″. 15*s*.

Fano, Robert M. Transmission of information: a statistical theory of communications. [Cambridge], Massachusetts Institute of Technology Press New York, London, Wiley, [1961]. x, 389 p. diagrs. 9″. 60*s*.

Feigenbaum, A. V. Total quality control: engineering and management: the technical and managerial field for improving product quality, including its reliability, and for reducing operating costs and losses. New York, [etc.], London, McGraw-Hill, 1961. xii, 627 p. diagrs. 9″. 85*s*. 6*d*.

Ferguson, Robert O. & Sargent, Lauren F. Linear programming: fundamentals and applications. New York, [etc.], London, McGraw-Hill, 1958. xiv, 342 p. tables, diagrs., chart. 9″. 77*s*. 6*d*.

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Finland, *Central Statistical Office.* 1953, General economic census, vol. III. Construction, banking, insurance, transport and communication services, agriculture, forestry, fishing, real estate. Helsinki, 1960. 68 p. 11¾″. n.p.

Francis, Roy G. The rhetoric of science: a methodological discussion of the two-by-two tables. Minneapolis, University of Minnesota Press, [1961]. [London, Oxford University Press.] viii, 183 p. 9″. 38*s.*

Garvin, Walter W. Introduction to linear programming. New York, [etc.], London, McGraw-Hill, 1960. xiv, 281 p. diagrs. 9″. 68*s.*

General Agreement on Tariffs and Trade. International trade, 1960. Geneva, 1961. 109 p. tables. 9½″. $1.50.

Gibbs, J. Willard. Elementary principles in statistical mechanics: developed with especial reference to the rational foundation of thermodynamics. New York, Dover, [London, Constable], 1960.

The scientific papers of J. Willard Gibbs . . . in two volumes. Vol. 1. Thermodynamics. Vol. 2. Dynamics, vector analysis and multiple algebra, electromagnetic theory of light, etc. New York, Dover, [London, Constable], 1961. 2v. 8½″. 16*s.*

Goodman, Richard, *editor.* Annual review in automatic programming: 2. Oxford, London, [etc.], Pergamon Press, 1961. viii, 394 p. diagrs. 9¾″. 70*s.*

Greece, *National Statistical Office.* Household survey: carried out in the urban areas of Greece during 1957–58. Athens, 1961. 438 p. 11¾″. $2.50.

Résultats du recensement de la population: effectué le 7 Avril, 1951. Vol. 1. Aperçu historique—Rapport méthodologique—Analyse des résultats, Tableaux par superficie et altitude. Athens, 1961. cxcx, 312 p. 12¾″. $3.

Hall, Margaret, *& others.* Distribution in Great Britain and North America: a study in structure and productivity; by Margaret Hall, John Knapp and Christopher Winston. Oxford, University Press, 1961. xiv, 231 p. tables, diagrs. 11″. 63*s.*

Handel, Paul von, *& others, editors.* Electronic computers: fundamentals, systems, and applications; edited by Paul von Handel . . . with the co-operation of Hans W. Gschwind, Martin C. Jaenke and Robert F. Tantzen . . . with 160 figures. Wien, Springer-Verlag, 1961. vi, 235 p. plates, diagrs. 9½″. 84*s.* 6*d.*

Hartman, S. & Mikusinski, J. The theory of Lebesgue measure and integration . . . enlarged edition, translated from Polish by Leo F. Boron . . .. Oxford, London, [etc.], Pergamon Press, [1961]. diagrs. 8½″. 30*s.* (International series of monographs on pure and applied mathematics, vol. 15.)

Hohn, Franz E. Applied Boolean algebra: an elementary introduction. New York, The Macmillan Company, [1960]. xx, 139 p. diagrs. 9¼″. 17*s.* 6*d.*

Hungary, *Központi Statisztikai Hivatal.* 1960. Évi népszámlálás (Population census). 2. Személyi és családi adatok képviseleti minta alapján. Budapest, 1960. 158 p. 11″. n.p.

Iceland, *Statistical Bureau.* Statistics of Iceland, II, 21. Population census 1703. Reykjavik, 1960. 59 p. 9½″. *Kr.* 20.

India. Characteristics of family households: 1951 census—West Bengal. Delhi, 1960. vi, 535 p. 11″. 26*s.* 6*d.* Census of India; paper no. 1. 1958.

Institute of Practitioners in Advertising. National readership surveys: 1961. London, 1961. 2v. 8″ × 14″. n.p.

International Labour Office. Family living studies: a symposium. Geneva, London, 1961. [4], 280 p. 9½″. 15*s.* (Studies and reports, new series, 63.)

Jeffreys, Harold. Theory of probability . . .. 3rd ed. Oxford, Clarendon Press, 1961. x, 447 p. 9¼″. 84*s.*

Jonge, H. de, *editor.* Quantitative methods in pharmacology: proceedings of a symposium held in Leyden on May 10–13, 1960. Amsterdam, North-Holland Publishing Co., 1961. xx, 391 p. 9″. 86*s.*

Kauffman, A. & Cruon, R. Les phénomènes d'attente: théorie et applications. Paris, Dunod, 1961. xiv, 274 p. diagrs. 9½″. 56*NF.*

Kemperman, J. H. D. The passage problem for a stationary Markov chain. Chicago, University of Chicago Press, [1961]. [8], 126 p. 9½″. $5.

Kenya, *East African Statistical Department.* Capital formation in Kenya: 1954–1960. Nairobi, 1961. [2], 14, [2] p. tables, diagrs. 10¾″. 3.50*s.*

Reported employment and wages in Kenya, 1948–1960. [Nairobi], 1961. iv, 27, 16 p. tables. 11½″. 3.50*s.*

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Kestelman, H. Modern theories of integration. 2nd rev. ed. New York, Dover, [1960]. [London, Constable.] x, 309 p. 8½″. 16*s.*

Kopal, Zdenek. Numerical analysis: with emphasis on the application of numerical techniques to problems of infinitesimal calculus in single variables. 2nd ed. London, Chapman & Hall, 1961. xvi, 594 p. tables. 9″. 70*s.*

Krupp, Sherman. Pattern in organization analysis: a critical examination. Philadelphia, New York, Chilton Company, 1961. xiv, 201 p. 8″. $5.

Lenti, Libero. Saggi di macroeconomica. Milano, Dott. A. Guiffre, 1961. viii, 635 p. tables. 9¾″. *L*4,000.

Levy, Paul M. G. La querelle du recensement. Bruxelles, Institut Belge de Science Politique, 1960. 166 p. 8¾″. n.p. (Nouvelle série 3.)

Linnik, Yu. V. Method of least squares and principles of the theory of observations. Translated from the Russian by Regina C. Elandt . . . edited by N. L. Johnson . . .. Oxford, London, [etc.], Pergamon Press, 1961. xii, 360 p. 8½″. 84*s.*

London County Council. London statistics: volume III, new series, 1947–1956. London, 1960. xii, 255 p. pl., map, tables, diagrs. 10¾″. 40*s.*

London Transport Executive. London transport in 1960. London, 1961. 97 p. 4 plates, maps, diagrs. 8½″. n.p.

Lopez, Alvaro. Problems in stable population theory. Princeton, N.J., Office of Population Research, Princeton University, 1961. x, 107 p. 9″. $4.

Marshall, Alfred. Principles of economics: ninth (variorum) edition, with annotations by C. W. Guillebaud. Volume 1, Text; Volume 2, Notes. London, Macmillan & Co., for the Royal Economic Society, 1961. 2v. diagrs. 9¾″. 120*s.*

Miller, David W. & Starr, Martin K. Executive decisions and operations research. London, Prentice-Hall International, 1961. x, 448 p. diagrs. 9″. 42*s.*

Moser, C. A. & Scott, Wolf. British towns: a statistical study of their social and economic differences. Edinburgh, London, Oliver & Boyd, 1961. xii, 169 p. tables, charts. 10″. 30*s.* (Centre for Urban Studies, report no. 2.)

National Bureau of Economic Research. The growth of public expenditure in the United Kingdom; by Alan T. Peacock and Jack Wiseman, assisted by Jindrich Veverka. Princeton, University Press, [London, Oxford University Press], 1961. xxxii, 213 p. diagrs. 9″. 30*s.*

The postwar residential mortgage market; by Saul B. Klaman. Princeton, University Press, 1961. London, Oxford University Press. xxxii, 301 p. tables, diagrs. 9″. 60*s.*

Productivity trends in the United States; by John W. Kendrick . . . assisted by Maude R. Pech. Princeton, University Press, [London, Oxford University Press], 1961. lii, 630 p. tables, diagrs. 9″. 100*s.*

Signals of recession and recovery: an experiment with monthly reporting; [by] Julius Shiskin. [New York, 1961.] xii, 196 p. tables, diagrs. 9″. $3. (Occasional paper 77.)

Organization for European Economic Cooperation. Demographic trends, 1956–1976 in Western Europe and in the United States. Paris, 1961, London, H.M.S.O. 151 p. 10¾″. 12*s.* 6*d.*

Pantony, D. A. A chemist's introduction to statistics, theory of error and design of experiment. London, Royal Institute of Chemistry, [1961]. ii, 38 p. diagrs. 9½″. 5*s.*

Pask, Gordon. An approach to cybernetics. London, Hutchison, [1961]. 128 p. plates, diagrs. 7¾″. 12*s.* 6*d.*

Peterson, William. Population. New York, London, The Macmillan Company, 1961. xx, 652 p. tables. 9¼″. 60*s.*

Queensland, *Commonwealth Bureau of Census and Statistics.* Official year book of Queensland, 1960, no. 21. Brisbane, 1961. xii, 465 p. map, diagrs., tables. 8½″. 10*s.*

Rapoport, Anatol. Fights, games, and debates. Ann Arbor, University of Michigan Press, [1960]. xvi, 400 p. diagrs. 9″. $6.95.

Reichman, W. J. Use and abuse of statistics. London, Methuen, 1961. 336 p. diagrs. 8¼″. 22*s.* 6*d.*

Rhodesia & Nyasaland, *Federation of. Central Statistical Office.* Balance of payments, 1954–1960. Salisbury, 1961. [4], 78 p. 13″. 3*s.* 6*d.*

Income tax statistics. Income years 1953/54–1958/59 . . .. Salisbury, 1961. 4v. 8″ × 13″. 3*s.* 6*d.*

National accounts of the Federation of Rhodesia and Nyasaland, 1954–1960. Supplement; territorial tables. Salisbury, 1961. 2v. 13″. 10*s.*

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Preliminary report on the federal European family expenditure survey, October, 1960. [2], 23, xxii p. 13″. n.p.

Robertson, D. J. The economics of wages and the distribution of income. London, Macmillan & Co., 1961. xii, 242 p. 8½″. 21*s.*

Rumania. *Directia Centrală de Statistică.* Receňsamîntul populatiei din 21 Februarie 1956: structura social-economică a populaţiei ocupaţii. Bucarest, 1961. lxxi, 1469 p. 12″. n.p.

Slonim, Morris James. Sampling in a nutshell. New York, Simon & Schuster, 1960. xiv, 145 p. illus., tables. 8¼″. $3.50.

Smirnov, G. D. Electronic digital computers . . . translated from the Russian by G. Segal . . .. Oxford, London, [etc.], Pergamon Press, 1961. vi, 97 p. diagrs. 8½″. 42*s.*

Smith, Vernon L. Investment and production: a study in the theory of capital-using enterprise. Cambridge, Mass., Harvard University Press, 1961. [London, Oxford University Press.] xii, 340 p. diagrs. 8¼″. 60*s.*

Société de Statistique de Paris. "Paris, 1960": Edité à l'occasion du centenaire de la Société de Statistique de Paris. Paris, Imprimerie Municipale, 1961. 328 p. tables, diagrs. 9½″. n.p.

Solomon, Herman, *editor.* Studies in item analysis and prediction . . .. With contributions by: R. Sitgreaves, G. Elfving, P. F. Lazarsfeld, R. R. Bahadur, H. Raiffa, M. V. Johns, E. Paulson, H. Solomon, D. Teichroew, and A. H. Bowker. Stanford (Calif.), Stanford University Press, 1961. [London, Oxford University Press.] xii, 310 p. diagrs. 9¼″. 70*s.*

Spencer, Milton H., *& others.* Business and economic forecasting: an econometric approach; [by] Milton H. Spencer, . . . Colin G. Clark . . . and Peter W. Hoguet . . .. Homewood (Ill.), Richard D. Irwin, 1961. xii, 412 p. tables, diagrs. 9″. $7.95.

Spurr, William A., *& others.* Business and economic statistics; by William A. Spurr, . . . Lester S. Kellogg . . . and John H. Smith . . .. Rev. ed. Homewood (Ill.), Richard D. Irwin, 1961. xii, 560 p. illus., tables, diagrs. 9″. $7.95.

Stone, Richard. Input–output and national accounts. Paris, Organization for European Economic Cooperation, 1961. London, H.M.S.O. 204 p. tables. 9½″. 21*s.*

Stone, Richard & Stone, Giovanna. National income and expenditure. London, Bowes & Bowes, [1961]. 118 p. tables. 7¼″. 15*s.*

Sweden, *Statistiska Centralbyran.* Konsumentpriser och indexberakningar, aren 1931–1959. Stockholm, Kungl. Socialstyrelsen, 1961. 158 p. tables, 9½″. *Kr.* 4.75.

Tartara, Marina. L' "Isolotto" a Firenze: indagine sui problemi di adattamento in un quartiere di edilizia pubblica. Firenze, Scuola di Statistica dell'Università, [1960]. 100 p. diagrs. 9½″. n.p. (English summary: Problems of citizens' adaptation in a district of popular housing.)

Tortrat, A. Principes de statistique mathématique. Paris, Dunod, 1961. xiv, 165 p. diagrs. 6½″. 16*NF.*

Tuck, R. H. An introduction to the principles of agricultural economics. London, [etc.], Longmans, 1961. vii, 259 p. tables. 8½″. 25*s.*

Turnbull, H. W. & Aitken, A. C. An introduction to the theory of canonical matrices. New York, Dover, [1961]. [London, Constable.] xiv, 200, 15 p. 8″. 12*s.*

United Kingdom. *Board of Trade.* The machine tool industry: a report by the sub-committee of the Machine Tool Advisory Council appointed to consider Professor Melman's report to the European Productivity Agency. London, H.M.S.O., 1960. 36 p. 9¾″. 2*s.*

Board of Trade & Central Statistical Office. Input–output tables for the United Kingdom, 1954. London, H.M.S.O., 1961. vi, 32 p. 11¼″. 7*s.* 6*d.* (Studies in official statistics, 8.)

General Register Office. Census 1961. England and Wales; preliminary report. London, H.M.S.O., 1961. iv, 80 p. 13″. 7*s.* 6*d.*

Home Department & others. The fishing industry. Presented to Parliament by the Secretary of State for the Home Department, the Secretary of State for Scotland and the Minister of Agriculture, Fisheries and Food . . .. London, H.M.S.O., 1961. 11 p. 9¾″. 1*s.* (Cmnd. 1453.)

Home Office. Home Office studies in the causes of delinquency and the treatment of offenders, 4. Murder; by Evelyn Gibson and S. Klein. London, H.M.S.O., 1961. iv, 43 p. tables. 9¾″. 4*s.*

Interdepartmental Committee on Social and Economic Research. Census of production reports. London, H.M.S.O., 1961. vi, 86 p. 9¾″. 5*s.* (Guides to official sources, 6.)

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Medical Research Council. Cerebral infarction: the role of stenosis of the extracranial cerebral arteries; [by] Peter O. Yates and Edward C. Hutchinson. London, H.M.S.O., 1961. viii, 95 p. plates, tables, diagrs. 9¾″. 14*s.* (M.R.C. special report series, 300.)

Refraction and its components during the growth of the eye from the age of three; by Arnold Sorsby, B. Benjamin and Michael Sheridan . . .. London, H.M.S.O., 1961. viii, 87 p. 4 plates, diagrs. 9¾″. 9*s.* 6*d.*

Meteorological Office. Handbook of meteorological instruments: part II, instruments for upper air observations. London, H.M.S.O., 1961. x, 209 p. plates. 9½″. 25*s.* (M.O. 577.)

Ministry of Agriculture, Fisheries & Food, & others. Output and utilization of farm produce in the United Kingdom, 1956–57 to 1958–59; [by the] Ministry of Agriculture, Fisheries and Food, [the] Department of Agriculture and Fisheries for Scotland, [and the] Ministry of Agriculture, Northern Ireland . . .. London, H.M.S.O., 1961. 2v. 9½″ × 13″. 3*s.*

Ministry of Defence. Defence specification DEF-131: sampling procedures and tables for inspection by attributes: (batch by batch inspection where a series of batches is to be offered for acceptance). London, H.M.S.O., 1961. 151 p. 10¾″. 17*s.* 6*d.*

Ministry of Labour. Family expenditure survey: report for 1957–59. London, H.M.S.O., 1961. vi, 88 p. 11″. 12*s.* 6*d.*

Ministry of Power. Statistical digest, 1960. London, H.M.S.O., 1961. x, 172 p. 11″. 27*s.* 6*d.*

Scotland. General Registry Office. Preliminary report on the sixteenth census of Scotland, 1961. Edinburgh, H.M.S.O., 1961. 47 p. 13″. 5*s.*

United Nations. Commodity survey, 1960. New York, 1961. xii, 240 p. tables, diagrs. 11″. 21*s.*

Current economic indicators. A quarterly statistical review of developments in the world economy. 1961. **2**, 1. New York, 1961. 11½″. 28*s.* p.a.

Department of Economics and Social Affairs. World economic survey, 1960. New York, 1961. xii, 237 p. tables, diagrs. 11″. 21*s.*

Yearbook of national accounts statistics, 1960. New York, 1961. xxiv, 284 p. 11″. 25*s.*

United States. *Department of Labor.* Wholesale prices and price indexes, 1959. Washington, 1961. viii, 268 p. 10¼″. $1.50. (Bulletin 1295.)

National Bureau of Standards. Fractional factorial designs for experiments with factors at two and three levels; [by] W. S. Connor and Shirley Young. Washington, 1961. vi, 65 p. tables. 10¼″. 40*c.*

National Science Foundation. Scientific and technical personnel in American industry: report on a 1959 survey. Prepared by the U.S. Department of Labor, Bureau of Labor Statistics. Washington, 1960. vi, 66 p. tables. 10¼″. 45*c.*

Vickery, B. C. Classification and indexing in science . . .. 2nd ed. London, Butterworths. xx, 235 p. 8½″. 30*s.*

On retrieval system theory. London, Butterworths, 1961. x, 159 p. diagrs. 8½″. 30*s.*

Weiss, Lionel. Statistical decision theory. New York, [etc.], London, McGraw-Hill, 1961. viii, 195 p. diagrs. 9″. 58*s.*

Williams, G. Prys, & Goodwin, Harold S. Road risks of the elderly: the dangers to which they are exposed, and how they may reduce the risk of death and serious injury: a report to retired and retiring people . . .. London, Magazines and Publications, 1961. 36 p. 9½″. 3*s.*

Wilson, Edwin Bidwell. Vector analysis: a text-book for the use of students of mathematics and physics founded upon the lectures of J. Willard Gibbs . . .. New York, Dover, [London, Constable, 1960]. xviii, 436 p. 15 p. 8″. 16*s.*

Wonnacott, Ronald J. Canadian–American dependence: an interindustry analysis of production and prices. Amsterdam, North-Holland Publishing Co., 1961. xiv, 143 p. tables. 9″. 32*s.*

Woods, Frederick S. Higher geometry: an introduction to advanced methods in analytic geometry. New York, Dover, [1961]. xii, 423, [11], 15 p. diagrs. 8″. $2.

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Learning and Inference

By Violet R. Cane

Newnham College, Cambridge

[Read before the Royal Statistical Society on Wednesday, January 17th, 1962, the President, Dr M. G. Kendall, in the Chair]

Summary

The results of some learning experiments are used to test the assumptions upon which linear mathematical models of learning rest, and these assumptions are found to be invalid. It is found that, if the problem confronting a subject in a learning situation is regarded as a decision problem, in the sense used in statistical theory, it is possible to regard his response as consistently determined in a way similar to a statistical decision procedure, provided proper account is taken of the effect of his previous experience on the sort of hypotheses he considers and the prior probabilities he attaches to them. Some suggestions are made for a physiological model that would produce behaviour of this kind.

1. Introduction

It has been suggested often enough that the process of inference, or inductive logic, is equivalent to learning from experience; and statisticians certainly seem to regard statistical induction as in some way leading to a best method of behaving in the light of a given amount of information. Only Savage, I think, discusses behaviour directly, in that he bases his theory on preferences among acts; but all statistical theories, by the use of one principle or another, supply methods of evaluating the data at our disposal so as to determine exactly (or almost always exactly) how to choose between various competing beliefs about the state of the world; and it is usually assumed that, once this choice is made, we shall know how to act.

Thus any statistical theory may be described as offering a rational method of determining behaviour in terms of knowledge of past events. As to whether it is rational to try to do this, I should perhaps remind this Society that at a previous meeting Irwin pointed out (discussion on Anscombe, 1948) that inductive inference is based on faith in the proposition that we can learn from experience. That human beings have this faith may be judged from their behaviour, and the same may be said of most other creatures; in fact, experimenters have demonstrated time and again that, by suitable manipulation of a subject's experience in a given situation, they can get him to respond to it in almost any way they choose. Thus we may infer—by a circular argument—that the propensity to learn, so generally found, is biologically useful, that is, it produces behaviour which conforms with events in the external world. Whatever is thought of this argument, we can certainly accept that learning occurs and statistical inference occurs, and ask the question "What is the relation between them?"

It is a question that covers an enormous field. I must, through ignorance, confine myself to a small corner of it. I propose only to consider experiments on learning in a two-choice situation, to ask whether the type of mathematical models called linear

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models (Bush and Mosteller, 1955; Bush and Estes, 1959) represent adequately the results obtained in such experiments, and to see if the ideas that seem necessary to interpret these results are paralleled by concepts formulated by statistical theorists.

A great deal of work has already been done on a similar problem, that of testing whether subjects respond in the way recommended by decision theory or games theory. This is, however, mainly devoted to analysing "asymptotic" behaviour, when the subject is supposed to have finished learning and settled down into a "steady state"; whereas I wish to consider the process of learning, that is, what occurs in the earlier trials in an experimental situation. It seems, indeed, extremely difficult to decide when asymptotic behaviour has been reached, and the conflicting results of various experimenters may be mainly due to the different numbers of trials assigned to the learning stage.

2. Three Learning Experiments

In a two-choice learning situation the subject is repeatedly put into a position where he must make one of two possible responses, and where there are only two outcomes, which may be described as "success" and "failure". Many of these experiments are of one of the following types:

(i) for human beings; a subject sits before a machine (a one-armed bandit) which has two light bulbs, one on the left and the other on the right, and a button below each. He is told that, at each trial, he has to predict which light will come on and to signify his choice by pressing the appropriate button. If he chooses the correct button he scores a point, for which he is usually paid.

(ii) for human beings; a similar machine is used, but in this case if the correct button is chosen the light above it goes on, if the wrong button is chosen there is no light (this is a two-armed bandit). It differs from experiment (i) in that neither button, or both buttons, may be correct at a given trial. In these two experiments one trial immediately follows another.

(iii) for rats; a rat which has been starved for something like twenty-four hours is put into a runway from which arms lead left and right. When he has moved into one of these arms he is kept there and, if he has made a successful choice, finds a small amount of food. Sometimes the trials in such an experiment are a day apart, sometimes two or three are run on the same day, usually less than an hour apart. In the latter case it is assumed that any food consumed during the experiment does not materially affect the rat's appetite.

This experiment is similar to (ii) in the sense that the rat only gets information about the arm he visits; I have labelled it as a separate type because it is impossible to explain to the rat what he is required to do, and it is known that instructions affect behaviour (e.g. Edwards, 1961).

I shall consider only those experiments in which the reinforcement schedule—the scheme which determines what response (or responses) shall be rewarded at each trial —is decided in advance; in the first experiment this will be some sequence of the terms "left" and "right"; in the other two it may include "both" and "neither". The sequences are usually determined in a random, or quasi-random, manner and a schedule is described as $x : y$, where x and y are, respectively, the probabilities of reward on the right and left sides. If learning is to take place the experimenter must arrange that one side, which I shall take to be the right, occurs more often than the other in this schedule. Then $x > y$, and for experiment (i), $y = 1 - x$. The combination

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of the ith term in the schedule with the ith response of a subject determines the outcome, for him, of the ith trial. Thus a sequence of trials can be represented by symbols $R(s), R(f), L(s), L(f)$, where R and L stand for right and left and s and f stand for success and failure. This is by no means a complete record of the experiment: one might, for instance, note the time elapsing before a response is made, or the sort of hesitations and uncompleted responses that occur, and some theories of learning deal with these (e.g. Audley, 1960; Bower, 1959). The linear models for learning, however, are formulated solely in terms of the choice made and its outcome.

2.1. *The Meaning of Probability Statements in the Description of Learning*

If a large number of subjects do one of these experiments, an obvious way of summarizing the changes in behaviour that occur in the course of the experiment is to calculate the proportion, $p_i(R)$, of subjects who choose R at the ith trial; $p_1(R)$ would be about $\frac{1}{2}$ and $p_i(R)$ should, in a successful experiment, tend to 1 as i increases. Provided we can regard these subjects as a random sample of the population, $p_i(R)$ can be taken as an estimate of the probability that a subject chosen at random will make the correct response at the ith trial. Conditional probabilities, for instance $p_2(R \mid R(s)$ at 1), can be estimated in the same way and will have the same sort of interpretation. Such an approach is usually good enough for deciding whether a change in the experimental conditions—say, the sort of instructions or amount of reward given—has any effect.

In most mathematical theories of learning, however, it is assumed that it is meaningful to talk about the probability of correct response at the ith trial for a single subject, this probability being conditional on his experience in the first $(i-1)$ trials. If a subject has only a probability of responding in one way rather than another in these experiments, however, he can hardly be basing his actions on inference, since any system of inference would lead—almost always—to determinate behaviour. It is therefore worth considering in what sense we might speak of the probability of a response in relation to a single subject.

First of all, I do not see that this idea, that a subject might vary his response at a given trial, can be verified directly by experiment; for any repetition with the same subject in the same situation simply gives us more of the learning experiment; while, if a group of subjects assumed to be identical behave differently, we cannot know that their responses are not in fact determined by events in their earlier life. (It might be thought that rats reared under laboratory conditions could be treated as similar to each other; in fact, hand-reared laboratory birds show much greater variation in their behaviour than wild birds (Vince, 1960).) All we can do is to discuss what this idea might mean.

Imagine, therefore, that we could repeat the ith trial; if the subject made a different response, how could this variability be interpreted? One suggestion (see Restle (1959, p. 421); this is a modification of Estes' theory) is that any given situation contains a number of stimulating elements, some indicating one kind of response and some another, and that the subject acts upon whichever of these chances to catch his eye; what happens in the course of learning is that more and more of the elements in the situation indicate the correct response. Thus the response is random in the same way that the result of tossing a coin is. Alternatively we might suppose that the subject arrives at a definite decision based on all the information, past and present, that he considers relevant but allow for the possibility of his making errors—as de Finetti has pointed out, an individual might act in accordance with the probability

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axioms and yet make mistakes in arithmetic. This also involves a random element, but it seems to me of rather a different kind, and it can sometimes be detected. For example, there is a best strategy for experiment (i), that is, to predict the side on which the light has occurred more often; I have found that some subjects use this strategy consistently until the number of trials is rather large, when they begin to make mistakes which are clearly due to errors in counting. There is one further case in which the response at the ith trial would not be invariant, and that is if the two sides were not discriminably different. In experiment (i), for a subject who counted accurately, this would occur only when the light had appeared equally often on the two sides; but if a subject relied upon a mere impression of frequency then there would probably be a discrimination threshold, i.e. he would not treat a frequency f_2 as discriminably greater than a frequency f_1 until $f_2 > f_1(1+c)$, where c is a positive constant (assuming that Weber's law would hold).

Thus there are various ways in which a random element could occur in a subject's decision procedure. However, I feel that what learning theorists chiefly mean in talking about the probability of a correct response at the ith trial is that the experimenter can only make probability statements about what a subject will do next, given his responses so far. This seems to be the same as describing a group of subjects having the same experience on the first $(i-1)$ trials.

3. Linear Models

Mathematical linear models of learning are based on the following assumptions:

(i) since the situation in which the subject is placed is always the same, the change in behaviour observed on successive trials must be the result of the responses and reinforcements that occur; thus a record of these comprises the relevant data of the experiment;

(ii) if the experimenter knows the first i terms of such a record he can make some statement—a probability statement—about the response that will be chosen at the $(i+1)$th trial. These probabilities will depend on the preceding responses and reinforcements and on some parameters whose values will depend on the conditions of the experiment, i.e. $p_{i+1}(R) = f(\text{record}, \theta)$. (The conditions of the experiment include any known previous experience common to the whole group of subjects, e.g. the administration of a drug. In theory, the parameters θ might be different for different subjects within the group but in practice estimates of the parameters are usually made from the whole group);

(iii) the whole effect of the i preceding trials is summed up in $p_{i+1}(R)$. In consequence of this, $p_{i+1}(R)$ can be expressed in terms of $p_i(R)$, the response and outcome of the ith trial, and θ. A linear relationship is taken as a first approximation. Thus $p_{i+1}(R) = \theta_1 p_i(R) + \theta_2$ if $R(s)$ occurs at trial i, and there are three other equations of the same type corresponding to $R(f), L(s), L(f)$.

Put $p_1(R) = p_i$, so that $p_i(L) = 1 - p_i = q_i$.

Since perfect learning occurs when one side, say R, is continually rewarded we must have

$$\lim_{i \to \infty} p_i = 1, \quad \text{so that} \quad p_{i+1} = (1-\theta)p_i + \theta;$$

also if the reward is the same on whichever side it occurs we must have

$$q_{i+1} = (1-\theta)q_i + \theta$$

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for the case $L(s)$. If we assume that $R(f), L(f)$ have symmetrical effects we arrive at the set of equations

$$\text{(A)} \qquad \begin{aligned} p_{i+1} &= (1-\theta)p_i+\theta && \text{if } R(s) \text{ at trial } i \\ &= (1-\phi_1)p_i+\phi_2 && \text{,, } R(f) \text{ ,,} \\ &= (1-\theta)p_i && \text{,, } L(s) \text{ ,,} \\ &= (1-\phi_1)p_i+\phi_1-\phi_2 && \text{,, } L(f) \text{ ,,} \end{aligned}$$

where θ and ϕ_1 lie between 0 and 1 and $0 \leqslant \phi_2 \leqslant \phi_1$. In experiment (i), $R(f)$ and $L(s)$ convey the same information to the subject so it is usual to take $\theta = \phi_1$ and $\phi_2 = 0$ for such experiments (occasionally also for experiments (ii) and (iii)). It will be seen that the first of the equations (A) implies that the success of a response increases the probability that it will occur at the next trial.

For a reinforcement schedule $x : y$ we have

$$E(p_{i+1}) = (\phi_1-\theta)(x-y)E(p_i^2)+[x(\theta-\phi_2)+y(2\phi_1-\theta-\phi_2)+1 -2(\phi_1-\phi_2)]E(p_i)+(\phi_1-\phi_2)(1-y)$$

and if $E(p_i)\,E(p_i^2), E(p_{i+1})$ all tend to 1 as i tends to infinity, either $x = 1$ or $\phi_1 = \phi_2$. However, since extinction can occur, the equations for $R(f)$ and $L(f)$ must have limiting solutions for which $p_{n+1} = p_n = \frac{1}{2}$, and then $\phi_2 = \frac{1}{2}\phi_1$ and $p_{n+1} = (1-\phi_1)p_n+\frac{1}{2}\phi_1$ for both cases. (A learnt response is said to be extinguished if the subject no longer chooses it in preference to other possible responses; in two-choice experiments extinction can usually be accomplished by changing to a 0 : 0 reinforcement schedule, and the subject generally reverts very quickly to random choice, i.e. $p(R) = \frac{1}{2}$.) Thus the population as a whole cannot learn to give consistently the better of two responses except in the special case where one response is always successful—nor can a single subject invariably be relied upon to do so. If these models do represent the process of learning, then subjects of learning experiments do not behave like statisticians.

Galanter and Bush (1959) have tested such models experimentally and find them totally inadequate to deal with certain aspects of learning—the slowness of initial acquisition, the effect of over-learning, and the rapidity of extinction. Therefore something must be wrong with the assumptions, and I propose to consider them in some detail.

3.1. *Enquiry into Some Assumptions*

The third assumption includes the first two, so I shall begin by asking "Does $p_i(R)$ sum up the situation at the beginning of the ith trial?" This question was, I think, first put by La Berge (1959a, b) and tested by means of experiment (i). He gave his subjects n trials with a reinforcement schedule of 0·5 : 0·5, and then changed this, without a break in the sequence of trials, to one of 0·9 : 0·1. For four groups of subjects A, B, C and D, n was 0, 20, 100 and 200 respectively. The trials with the second reinforcement schedule form the main experiment, and for this $p_1(R)$ was about $\frac{1}{2}$ for each group. The rate of learning of the four groups was, however, different; 80 per cent. of the subjects were responding correctly by trial number 27, 35, 49, 49 in groups A, B, C and D respectively. A linear model could, of course, deal with this situation by using different parameters for the four sets of results. However, if the parameters change with the previous experience of the subject then in any learning experiment different subjects should be assigned different parameters

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and a linear model cannot represent the learning of a whole group. For instance in the case of experiment (i), where $\theta = \phi_1, \phi_2 = 0$, and $x = 1-y$, if we have

$$E(p_{i+1}) = x-(x-p_1)(1-\theta_j)^i$$

for the jth subject, over a group of k subjects we shall have

$$k^{-1}\Sigma E(p_{i+1}) = x-k^{-1}(x-p_1)\Sigma(1-\theta_j)^i$$

and this cannot be put into the form $p_{i+1} = x-(x-p_1)(1-\theta)^i$ for all i. The discrepancy between the learning curves for a uniform group and a mixed group would be most noticeable for the early trials, and it is here that linear models are notably unsuccessful.

La Berge's result can, however, be described in terms of statistical inference, qualitatively at least. One would say that the subject entertained the hypothesis that the reinforcement schedule was 0·5 : 0·5 with a degree of belief (or prior probability) that depended on his previous experience; and the fact that there is little, if any, difference between the results for groups C and D suggests that he must also entertain the hypothesis that the reinforcement schedule may change.

A necessary implication of a linear model if learning is to occur at all is that, if a response is reinforced at one trial, it should be more likely to occur at the next; in particular, that $p_2(R|R(s)) > p_1(R)$. This follows from the fact that, if $p_{i+1} = (1-\theta_1)p_i+\theta_2$ when $R(s)$ occurs at trial i, then $p_{i+1} > p_i$ if $\theta_2 > \theta_1 p_i$; θ_2/θ_1 is the limiting value of p_i as $i \to \infty$ and must be noticeably greater than $\frac{1}{2}$ if any learning occurs, whereas p_1 should be very near $\frac{1}{2}$. Experimental evidence does not support this. Data quoted by Sternberg (Sternberg, 1959; p. 362, Table 4) give, for 243 subjects doing experiment (i), a value for $p_1(R)$ of 0·636 and a significantly lower value for $p_2(R|R(s))$ of 0·546. The results obtained by Galanter and Bush in some experiments of type (iii) give corresponding values of 0·49 and 0·46 (their experiments I and II combined) and of 0·50 and 0·45 (their experiments III and IV combined). (In the latter two experiments the rats were given some pretraining; they were allowed to go into the first part of the maze—not the arms—and could find food there.)

Since Galanter and Bush report their experiments in full, that is, give the response and outcome for each subject at each trial, it is possible to investigate the effect of reinforcement at later trials. In their experiment I they had 24 rats to whom they gave three trials a day, for thirty-six days, with a 0·5 : 0 reinforcement schedule. They find, among other things, a daily recovery effect, so that an interval between trials of about $22\frac{1}{2}$ hours cannot be equated to one of about 45 minutes. For this reason I have calculated $p(R)$ only for the first trial of each day, and $p(R|R(s))$, etc. refers to the first two trials of a day. The results are shown in Table 1. It will be seen that for the first nine days $p(R|R(s))$ is never greater than $p(R)$; also $p(R|L)$ and $p(L|R)$, although they fluctuate a good deal, do not show any trend away from 0·50 for the first twelve days at least. It appears that there are two sorts of learning occurring in this experiment; the subject first learns to choose the more successful response at the first trial of the day—this begins at about the sixth day—and then learns to suppress alternation after an R response; this begins at about the tenth day. The response to the third trial appears to be dependent on the result of the second in much the same way as the second is on the first. If we work out for each rat the last day on which he gives the response L at the first trial, the median number of days is 15; the medians for the second and third trials are $24\frac{1}{2}$, $26\frac{1}{2}$. For the three other experiments, in which the reinforcement schedule was 1 : 0, the medians are

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4, 5, 4; 7, 8, 7; 8, 7, 8; in these experiments, the two kinds of learning seem to proceed at much the same rate and it is difficult to disentangle their effects. (The effect of pretraining, incidentally, has been to delay learning.)

Galanter and Bush think that a linear model cannot account for the rapidity of extinction. I am not sure that this is true. In the extinction situation, $p_1 = 1$ (or very nearly) and we should have $p_n = \frac{1}{2}(1-\phi_1)^{n-1}+\frac{1}{2}$, which would tend to $\frac{1}{2}$ very rapidly for ϕ_1 near 1. The only question then is whether this value of ϕ_1 can be incorporated

TABLE 1

Days	1	2	3	4	5	6	7	8
$p(R)$	·50	·71	·46	·42	·42	·67	·71	·79
$p(R \mid R(s))$	·42	·71	·45	—	·30	·38	—	—
$p(R \mid R(f))$	—	—	—	·60	—	—	·59	·50
$p(R \mid L)$	·50	·29	·54	·71	·71	·50	·43	·33

Days	9	10	11	12	13	14	15	16
$p(R)$	·75	·75	·71	·75	·83	·71	·58	·79
$p(R \mid R(s))$	·50	·83	—	·44	—	—	1·0	·79
$p(R \mid R(f))$	—	—	·59	—	·65	·88	—	—
$p(R \mid L)$	·50	·33	·57	·67	·50	·43	·60	·60

into the equations for the learning situation which precedes extinction. If the effects of $R(s), L(s)$ are assumed equivalent to those of $L(f), R(f)$, respectively, the initial rate of extinction would be about twice the initial rate of learning; it is usually found to be much greater than this.

What the linear model cannot deal with is the effect of over-learning on extinction and the fact that a partially reinforced response extinguishes more slowly than one invariably reinforced. If subjects doing experiments (ii) or (iii) under a reinforcement schedule $x : 0$ ultimately reach a stage when they invariably choose the correct side, then the only relevant fact, if a linear model is used, is that $p_1(R) = 1$ at the beginning of the extinction experiment. Yet it is generally found that the rate of extinction is slower if n, the number of learning trials, is increased (x constant) or x decreased (n, suitably large, constant). The first of these results is similar to that of La Berge, and can be interpreted in terms of the strength of the belief that R is the correct response. The second result is more interesting because, in fact, there has been less evidence during the learning experiment in favour of R. It has been suggested that, during the learning experiment, subjects become accustomed to runs of failure if $x < 1$ and therefore are not affected by the failures that occur under extinction until the run has become unexpectedly long; and something of the sort must surely be true. This means, however, that the subject is in effect estimating the value of x; he is not dealing simply with the two hypotheses that R is the better response, or L is, but considering a whole set of hypotheses defined by the possible values that x might take (probably a finite set, corresponding to discriminable differences in x).

Bakan (1953), in suggesting that learning might be described in terms of inverse probability, seems to me to deal only with two hypotheses (of the form, "If I go right, reward follows"), though he has not in fact considered the situation that arises in experiment (ii) with an $x : y$ reinforcement schedule; and I do not myself see how to deal with it in his terms. In any case, I feel that he designs hypotheses from the point of view of the experimenter, who knows what the experiment is about, and not from that of the subject, who does not.

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3.2. *The Effect of Experience on the Formulation of Hypotheses*

Let us consider experiment (iii) from the point of view of a rat; he does not know that it is the same maze at each trial (experimenters in fact often do their best to conceal this), he does not know that there is likely to be food in the maze, nor does he know that the experimenter is using a random reinforcement schedule. He looks for food in the maze because he is hungry. If he happens to turn left and finds no food there, it is clearly better for him to try the right at the next trial if it is the same maze, and it does not matter what he does if it is a different one. If, however, he happens to turn right at the first trial and finds food there, he eats it, and at the next trial it is still better to try the other arm because he knows there was no food on the right when he left. In particular, rats who were accustomed to 20-hour (or more) food deprivation might well expect that, once food had gone from a place, it would be a long time before it appeared again. I suspect that the reason why the rats that had pretraining in Galanter and Bush's experiment, with 1 : 0 reinforcement schedule, learnt more slowly was that they had had several days of such deprivation before the experiment proper. For these, $p(L|R(s))$ remained at about 0·50 for four days, whereas for those without pretraining the probability dropped from 0·50 on the first day to 0·25 on the second, and remained low.

For trials soon after the first the rat should still choose the opposite arm from the one last visited, because each was empty when he left it but one has been left unexplored for longer and therefore there has been more time for replenishment. Roughly we might say that, if the time interval between trials is equal, and the probability of replenishment after one such interval is $1-\pi$, then for two such intervals it is $1-\pi^2$. If there are three trials a day, then at the first trial a day the probabilities for the two sides will be $1-\pi^k$ and $1-\pi^{k+1}$, $k = 30$ approximately, which are probably not discriminably different. Thus the correct response should be learnt more rapidly for the first trial each day because there is no incentive to alternate. What the rat learns, in fact, is that one side is replenished more rapidly than the other.

We might write down an approximation to this state of affairs as follows: previous experience has led the rat to classify situations by the rate of recurrence of food; if we call such a rate λ, the probability that food will have recurred by time t (assuming a Poisson process) is $1-\exp(-\lambda t)$. The rat must start with some prior distribution of probability for λ, and this will be the same for each arm (unless he has a "side preference"). After a certain number of trials on experiment (iii) suppose the distribution of λ for the two sides is given by $p_L(\lambda)$ and $p_R(\lambda)$ and suppose that at the last trial he responded R and at the one before L. Then his expectation of food on the left and the right is $\int p_L(\lambda)\,[1-\exp(-2\lambda t)]\,d\lambda$ and $\int p_R(\lambda)\,[1-\exp(-\lambda t)]\,d\lambda$, respectively, and we may suppose that he chooses in accordance with the larger. If he chooses L and fails, the posterior distribution for λ, on that side, becomes $p_L(\lambda)\exp(-2\lambda t)\Big/\left[\int p_L(\lambda)\exp(-2\lambda t)\,d\lambda\right]$ while $p_R(\lambda)$ remains the same; similarly, if he chooses R and succeeds, the probability distribution for the right becomes $p_R(\lambda)\,[1-\exp(-\lambda t)]\Big/\left[\int p_R(\lambda)\,(1-\exp(-\lambda t))\,d\lambda\right]$. This is not, of course, an easy formulation to handle because each change in the distribution depends on the time interval between visits to an arm; moreover we must suppose that each rat has not only a personal prior distribution but also, probably, a personal threshold as to what

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he finds discriminable. Many experimenters have reported, however, that rats tend to alternate even when food is available in both arms at each trial, and that the probability of alternation drops with the time interval between trials (see, for instance, Dember and Fowler (1958)), and no explanation that is not equivalent to "there is something about rats that makes them alternate" has been put forward; so it seems worth suggesting one that would imply reasonable and consistent behaviour on the part of the rat.

Because any theory that predicts that learning will occur can be tailored to fit observed learning curves fairly well, it is usual to test linear models by the extent to which they can reproduce the "fine-grain structure" of the response sequences; predictions are made from the model of the average number of errors before the first success, the total number of errors, the number of alternations and other similar statistics, and these are compared with the averages from a group of subjects. It would I think be difficult to test the model I have proposed in this way. I am convinced, however, that this is not the proper method; what one should do is check the assumptions made, or simple deductions from them, directly. As was mentioned above, perhaps the most important deduction from the linear model is that $p_2(R \mid R(s)) > p_1(R)$. If this deduction is not verified it is hardly worth investigating the fine-grain structure since something is bound to go wrong near the beginning of the learning experiment; so far as I know it has never been checked with a large number of subjects, and such evidence as I have been able to find suggests that it is not necessarily true.

Moreover, a theory of learning should be able to predict, at least roughly, how parameters in the model will change when particular changes are made in the experimental conditions. This, I think, can be done if we consider the previous experience of the subject and suppose he works with hypotheses based on it. For instance, we should predict (i) that, when food is always available in both arms, alternation should occur more frequently from the first to second trial than it does later, if the time-interval between trials is short; and we should predict (ii) the effect found in extinction experiments, that the more trials there are on one reinforcement schedule, the slower the subject will be in adapting his behaviour to a new schedule. Other possible predictions are (iii) that a rat should learn more quickly (in terms of the number of trials) if he has only one trial a day, and (iv) should alternate less if, at normal feeding times, he is fed pellets of food one by one; also, since the probabilities calculated above are conditional on the maze being the same maze at each trial, we should expect initial learning to be faster if as many sensory cues as possible are provided for the rat.

These predictions are all based on the fact that alternation occurs when food is provided in both arms, and on the assumption that this is so because the rat recalls that there was no food in an arm when he left it. Qualitatively, the arguments for them are as follows:

(i) suppose the proportion of the rats choosing R first that choose L second is $\frac{1}{2}+\delta$; if the trials follow each other quickly δ should be fairly large, say about $\frac{1}{4}$, because the R side was empty when the rat left and the L side is unexplored. Suppose now a proportion $\frac{1}{2}+\delta^1$ of these return to R; $\delta^1 < \delta$ probably because now the rat is choosing between an arm just left and one left not so long ago; those which chose R at both first and second trials have already had some experience of the rapid rate of replenishment there, therefore the proportion of these turning to L next will be, say, $\frac{1}{2}+\delta^{11}$, $\delta^{11} < \delta$. The proportion alternating from the second to third trials will be

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$(\frac{1}{2}-\delta)(\frac{1}{2}+\delta^{11})+(\frac{1}{2}+\delta)(\frac{1}{2}+\delta^{1})<\frac{1}{2}+\delta$. The same argument applies to those choosing L first;

(ii) for an extinction experiment, say a reinforcement schedule of 1 : 0 followed by one of 0 : 0, the more trials on the first schedule, the more certain it is that λ is large; that is, $p(\lambda<\lambda_0)$ for the right side decreases as n increases. In the extinction trials the evidence goes to show that λ is small; the smaller $p(\lambda<\lambda_0)$ originally, the more evidence is required to make $p(\lambda<\lambda_0)$ suitably large;

(iii) if the interval between trials is large, there is less reason for the rat to avoid the arm last visited because there has been more time for replenishment, even if λ is small. In particular, if he is on a 24-hour deprivation schedule he may learn from this that food recurs about every 24 hours;

(iv) if food is replenished at short intervals in his ordinary life, he should assume it more likely to happen in the maze also.

It is the existence of this alternative hypothesis, that the maze is different, which really, to my mind, accounts for the rapidity of extinction. The rat is, in effect, being posed the difficult problem of testing continuous production—compare for instance the problem discussed by Anscombe *et al.* (1947)—and, as explained in that paper, can consider only the cases of a sudden change in the situation or a gradual trend. I do not know of any experiments in which the reinforcement schedule is changed gradually, so I shall explain only how he might deal with a sudden change. Suppose that the rat has to learn to accept that the maze is the same at each trial; let π represent the probability that a maze which looks the same, is the same (i.e. is such that the posterior distributions for λ on the two sides should be used), then π will change trial by trial and will have the value π_i, say, after the $(i-1)$th trial. Let p_{Ri}, p_{Li} be the probabilities of finding food on the right and left, respectively, if the maze is the same and let p_0, the same for each side, be the probability of finding food if the maze is different. The probability of finding food on the right is then $\pi_i p_{Ri}+(1-\pi_i)p_0$, and there is a similar expression for the left side. The decision between the two sides depends on the sign of $p_{Ri}-p_{Li}$; as usual, I assume a discrimination threshold, so that the choice is random if $|\pi_i(p_{Ri}-p_{Li})|<c$, for some given $c>0$. If $R(s)$ occurs,

$$\pi_{i+1}=\frac{\pi_i p_{Ri}}{\pi_i p_{Ri}+(1-\pi_i)p_0},$$

which is $>\pi_i$ if $p_{Ri}>p_0$, which must be ultimately true, also the distribution for λ for the right side, conditional on the maze being the same, changes in the usual way; there are similar results if $R(f)$ or $L(f)$ occur. After a long sequence of trials on an x : 0 reinforcement schedule, p_{Ri} should be approximately $x, \gg p_{Li}$, and π_i will be near 1, say $1-\delta$. If now the reinforcement schedule is changed to 0 : 0, when $R(f)$ occurs the probability attached to the hypothesis that the maze is the same becomes approximately

$$\frac{(1-\delta)(1-x)}{(1-\delta)(1-x)+\delta(1-p_0)};$$

thus, if $x=1$ it drops to zero, and in general the rate of decrease will depend on the size of $1-x$ and on δ, which will in turn depend on the number of reinforcement trials; ultimately, $|\pi_i(p_{Ri}-p_{Li})|<c$.

The sort of hypotheses that subjects adopt in experiments (i) and (ii) are bound to be very different from those above; they may again, however, be considered as based on the subjects' previous experience. (Such experience includes the instructions

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given at the beginning of the experiment; see Edwards (1961).) In experiment (i), a subject told to predict which light would appear might possibly assume that there was a random reward schedule, in which case he might either work with a uniform prior distribution for x or, from previous experience of experimenters or gambling machines, a non-uniform distribution. (After experience of gambling machines this would probably be of the form $Cx^m(1-x)^m$, m large; after experience with experimenters it would more likely be proportional to $[x^r(1-x)^s+x^s(1-x)^r]$, $r \neq s$.) He might equally well, however, suppose that he had to look for a temporal pattern and then we should have to assume that he assigns prior probabilities to initial sequences such as *RLRLRLR* or *RRRLRRR* (I have taken these of length seven since immediate memory is effective for about this number of symbols). He might also make the "gambler's error" of supposing that, because the light had appeared on the right for a long sequence of trials, it ought soon to appear on the left. I am not sure that this is an error if it is considered from the point of view of a subject's finite experience. Suppose that in n occurrences of a situation a particular outcome had been observed about $\frac{1}{2}n$ times, and that in the next n' occurrences it was observed n' times. For some not too small value of n', the subject would interpret this as a change in the situation (cf. extinction experiments); that is, what he means by a stable situation is one for which successive estimates of probability do not vary much. In consequence of this, if he is committed to thinking the situation stable, he expects some sort of balance within blocks of a given size. In fact, to ensure that a subject learns the right thing, experimenters often use sequences which are not random but made up of balanced randomized blocks; and, for all I know, manufacturers of gambling machines may do the same. Thus the "error" could have been learnt from experience.

One can eliminate hypotheses about patterns either by giving suitable instructions (Nicks, 1959) or by using a situation such as drawing a card from a freshly shuffled pack containing cards of two kinds (Neimark and Shuford, 1959). The second of these experiments has the additional advantage that it visibly displays the random character of the sequences; since the number of possible hypotheses is thereby limited, we should expect learning to be faster in this case. Unfortunately it is not possible to discover from the data reported whether this is so. What Neimark and Shuford did find, however, was that if subjects were asked to estimate the proportions of the two kinds of card, on the basis of those which had already been drawn, before making their prediction, they gave the correct response more often. Whether this requirement simply made them count more carefully or whether it actually drew their attention to the existence of a best strategy is not clear.

The best strategy for experiment (i), if the possibility of patterns is excluded, is to predict the light which has so far occurred more often. It is by no means obvious what the best strategy is for experiment (ii). The distinction between the two would be clearer—at any rate for the subject—if urns were used for the experiment. In experiment (i) we should use one urn containing a proportion x of red balls, the remaining balls being white, and ask the subject to predict the colour of the next ball in a sampling with replacement procedure. He would score a point for a correct prediction. In experiment (ii) we should use two urns, containing proportions x and y of red balls, respectively, and ask the subject to choose the urn from which we should take a ball (again sampling with replacement). He would score a point if the ball selected were red. This sort of experiment is rather like real life where we can usually only know the outcome corresponding to the response we actually make, and not the outcome of any other we might have made. Each response is a terminal decision for

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that trial but the outcome is an experiment as far as later trials are concerned, and no real terminal decision (to stick to one urn) need ever be reached. Thus at each trial the subject has two aims, (a) to give the best decision on that trial, and (b) to gain information to aid him in making future decisions. The same considerations apply in experiment (iii); the rat is usually supposed to be so hungry that he acts in accordance with (a); if he acts in accordance with (b) he is said to be displaying exploratory behaviour. De Valois (1954) finds that there is more alternation, i.e. exploratory behaviour, when a rat has been deprived of water for 6 hours than when he has been deprived for 22 hours.

A subject of experiment (ii) has to maximize his score over a number of trials; he can obviously only do this if he knows, or estimates, how many trials he will have. This problem (the two-armed bandit with a given number of trials) has been discussed in a number of statistical papers (see, for instance, Vogel (1960) and the papers he refers to) but a strategy which will maximize the expected score has only been found for various special cases: if the proportion in one urn is known, or only a finite number of preceding trials is remembered, or a terminal decision is to be made after k trials with each urn (k to be determined from the observations). The solutions (apart from the finite memory case) do not, however, reduce to simple rules that could be used in practice. A conjecture, given by Bradt, Johnson and Karlin, is that the choice should be made as if there were only one trial to come; they say this has been verified to give a best strategy when there are up to 8 trials ahead. Suppose, however, that the subject assigns a uniform prior probability on [0, 1] to both x_1 and x_2 (where x_1, x_2 are the proportions of red balls in urns U_1, U_2 respectively). After r_1 red, w_1 white balls from U_1 he will change to U_2 if

$$\frac{r_1+1}{r_1+w_1+2} > \tfrac{1}{2};$$

this may never happen if the true value of x_1 is appreciably above $\frac{1}{2}$, yet, if there are a large enough number of trials ahead, it must be worth trying U_2 after a white ball has been drawn from U_1 because the second urn may contain only red balls.

If the prior distributions are uniform it is clearly worth staying with one urn so long as red balls are drawn from it since the other urn could not produce better results. A possible strategy, therefore, would be for the subject to make a new decision only after a white ball were drawn, and then to choose the urn for which the expected gain up to the occurrence of the next white ball (or up to the end of the experiment) was greater. This is a consistent policy (see Appendix) and would lead the subject to try each urn at least once if the number of trials were sufficiently great, but it suffers from the same sort of disadvantage as the policy of looking only one trial ahead. Suppose that the first ball drawn from U_2 were white; then the subject would never choose U_2 again if

$$\frac{r_1+1}{w_1} > 1$$

always (if the number of trials ahead is very large), and the amount of evidence from U_2 seems very slender for such a decision.

I have tried these two experiments, using urns containing five balls; subjects were told the number of balls and the total number of trials they would have, which was twenty. Each subject did both experiments, in randomized order, though never with

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the same two reinforcement schedules (of which all possible kinds were used) and was paid a penny for each point scored. I compared the relevant results (when the reinforcement schedule was, or appeared to be, 1 : 0) with the results from a one-armed bandit given by Sternberg. The results for the single urn were very similar to his (learning was, perhaps, slightly more rapid but as I had only twenty subjects this is not conclusive). For the experiment with two urns, however, learning was very much faster; 90 per cent. or more of the responses were correct from the third trial whereas with the single urn this did not happen until the eleventh trial. Even those subjects who made incorrect responses, in the two-urn experiment, generally did so deliberately, explaining that it was worth a penny to find out more about the other urn. I suspect that the speed of learning was mainly due to elimination of the "gambler's error".

4. Discussion

There is a canon of psychological theory due to Lloyd Morgan which runs: do not attribute an animal's performance to the operation of a more intellectual process such as reasoning, if it can be understood in simpler terms. It is, I suppose, in obedience to this that learning theories tend to be formulated in terms of rules linking a stimulus to a response. In my opinion it is better to formulate them in terms of subjective probabilities and hypotheses, and I should like to justify this.

Inference is not an intellectual process, as I said earlier, although thinking about the best way of making inferences may be. It is a physiological process, and one which may reasonably be supposed much the same for other mammals as for man. "Hypothesis" and "probability" are terms invented by human beings to describe certain sensations; but sensations can be present without being named, and we can certainly act on an hypothesis without being conscious of it. Thus it seems quite sensible to describe any animal's behaviour in these terms.

The results of learning experiments indicate that we must take account of subjective prior probabilities, and must assume that subjects can entertain more than two hypotheses about a given situation. These hypotheses, however, are suggested by the subject's own experience and are not necessarily those the experimenter would adopt. It may be argued that, by inventing a suitable hypothesis for a subject, we can explain anything at all that he does. However, I do not envisage this approach as an explanatory one merely. If we can locate the sort of experience that leads to an hypothesis or to a particular level of subjective probability, then we can try to change it experimentally; but this requires that we should look at an experiment from the subject's point of view.

De Finetti has suggested that psychologists should ask whether human beings naturally, without special training, act in accordance with the axioms of probability. A more interesting question, I think, is whether we can envisage a physiological system that would act thus. Any answer is, of course, purely speculative. I suspect, however, that such a system cannot—or at any rate does not—handle both multiplication and addition, so that one axiom at least must be sacrificed; the addition rule seems less useful than the multiplication one so we might see how far we can go without it. For, say, the experiment with two urns we can suppose that the subject has an array of hypotheses H_{ij}, where i and j relate to the states on the two sides, with prior probabilities π_{ij}. After a success on one side these would be proportional to $\pi_{ij}p_i$; alternatively, if the success were on the other side they would be proportional to $\pi_{ij}p_j$. The factor of proportionality could be disregarded if we supposed that

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he acted in accordance with the maximum posterior probability, that is, he would select the H_{ij} for which this had the greatest value and make a decision depending on the values of i and j. Because Weber's law so often holds (that is, we can only apprehend differences which in the exact measures we choose to adopt are expressed as ratios) it seems worth trying the assumption that his "sensations" are in terms of the logarithm of probability. After s successes and f failures with the second urn, the hypothesis H_{ij} would have attached to it a weight

$$s\log p_j+f\log q_j+\log \pi_{ij}.$$

In writing q_j for $1-p_j$ I have of course assumed the addition law of probabilities and also that probabilities must sum to one. However there seems no reason why we should not just give arbitrary weights to the numbers of successes and failures, and so attach to H_{ij} the weight $sw_j+fw'_j+c_{ij}$, where c_{ij} is a prior weight; the size of w_j and w'_j must reflect the likelihood—in an everyday sense—of a success or failure on that hypothesis. We could thus think of each outcome as adding to or subtracting from the weight attached to a given hypothesis, and of comparisons being made between these weights; a mechanism of this sort, for a finite number of hypotheses, seems quite conceivable and it would produce decisions similar to, though less efficient than, ones determined by probabilities in the usual ways.

As a very simple example of this approach, suppose that a rat tested in a T-maze once a day considers only two rates of recurrence of food—slow and fast—and consequently works with a set of four hypotheses, H_{ij}, $i,j=1,2$, where i and j refer to the left and right arms of the maze and 1 and 2 indicate the slow and fast rates. Suppose that the prior weight attached to H_{ij} is c_i+c_j, where $c_1>c_2$; and that, if food is found on the left side, weights w_1, w_2 are added to H_{1j}, H_{2j} respectively, whereas, if food is not found, weights w'_1, w'_2 are added ($w_1<w_2, w'_1>w'_2$); under the same conditions, weights w_1, w_2 or w'_1, w'_2 are added to H_{i1}, H_{i2} if the right side is visited. If a zero threshold is assumed it is not necessary to allow for changes in the probability that the maze is the same. In order to avoid complications with the first few trials, I shall also assume that the first visit to each side is treated as if it were a visit after an interval of one day. For a 1 : 0 reinforcement schedule, after l turns to the left and r turns to the right, the weight attached to H_{ij} is $(c_i+c_j+lw'_i+rw_j)$; thus H_{12} will always have a greater weight than H_{21} or H_{22} and will have a weight less than or greater than that of H_{11} according as $r\lessgtr(c_1-c_2)/(w_2-w_1)$. If H_{11} has maximum weight we may say that the most likely hypothesis is that replenishment is slow on both sides and so, since there is a long interval between trials, choice will be made at random and $p(R)=\frac{1}{2}$. Correspondingly, if H_{12} is maximum, $p(R)=1$. (If it should happen that $(c_1-c_2)/(w_2-w_1)$ were an integer I should assume that, for this value of r, the rat had a chance of $\frac{1}{2}$ of acting on either hypothesis; but, for simplicity, I shall suppose that this is not the case and put $r_0=(c_1-c_2)/(w_2-w_1)+\delta$, $0<\delta<1$.)

Then $$P_i(R)=\tfrac{1}{2} \quad \text{if} \quad r<r_0 \quad \text{after } (i-1) \text{ trials};$$

$$=1 \qquad \geqslant r_0;$$

thus the rat will invariably give the correct response after $n=l+r_0$ trials. It will be seen that n is a random variable, having a negative binomial distribution and that the rat might appear to have learnt by some trial earlier than n if the last few random choices were to the right. For a group of rats operating with the same system of

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hypotheses and weights

$$P_{Li} = \tfrac{1}{2} \quad \text{for} \quad i = 1, 2, \ldots, r_0$$
$$= \sum_{r=0}^{r_0-1} \binom{i-1}{r} \Big/ 2^i, \quad i > r_0$$

(for example, if $r_0 = 2$, $p_{Li} = i/2^i$ for all i) and there would be no learning for the first r_0 trials, but after that a learning curve of much the usual shape. This would certainly account for the initial slowness of acquisition.

A similar method could be used with more categories for the rate of replenishment and more than one trial a day; there must then be different weights for different time-intervals between visits to an arm and also an order of preference between states; for example, if the hypothesis of greatest weight corresponded to slow replenishment on the left and a medium rate on the right, the rat might have to choose between the cases of slow replenishment over a long interval and a medium rate of replenishment over a short one. He would presumably decide in terms of the weights w, w' since these represent the likelihood of food being, or not being, found in these circumstances; this requires an obvious sort of consistency, that if $w_i > w_j$ then $w'_i < w'_j$. Other kinds of limitations on the weights (for instance, if we require consistency in the statistical sense) are considered in the Appendix.

From this very limited enquiry into some learning experiments I would conclude that learning from experience is a much more complex matter than linear mathematical models imply; that, although complex, the behaviour of a subject in a learning situation can reasonably be regarded as consistent and determinate, if we allow for such inexactness as discrimination thresholds and errors in recording; and that it is, in fact, similar to statistical inference, at least when such inference takes account of a range of alternative hypotheses and the prior probabilities attached to them.

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APPENDIX

1. THE TWO-URN PROBLEM

The subject is required at each trial to choose from which of two urns, U_1 and U_2, a ball is to be drawn (and replaced) and he scores a point if the ball is red. He knows that there are red and white balls in the urns but not the total number or the proportions of each colour.

Suppose that at some stage r_i red balls and w_i white balls have been drawn from U_i and that there are to be N further trials. Let $f(r_i, w_i, N)$ be the expected addition to the score if the subject goes on choosing U_i until either a white is drawn from it or all N trials have occurred. Then, if, for each urn, the subject assigns a uniform prior distribution on $[0, 1]$ to p, the proportion of red balls, and if he adopts the procedure of choosing the same urn again, if a red ball is drawn from it, and of choosing U_1 or U_2 according as

$$f(r_1, w_1, N) \gtrless f(r_2, w_2, N) \tag{1}$$

if a white ball has been drawn, then this procedure is consistent in the sense that consideration of (1) after a red ball would lead him to repeat his choice. (If the two terms in (1) are equal, either urn may be chosen.)

$$f(r, w, N) = \int_0^1 p^r q^w \sum_{s=1}^{N} p^s \frac{(r+w+1)!}{r!\, w!}\, dp$$

since, for a given value of p, the probability of an additional score of s or more is p^s.

The following results can be deduced immediately or after a little algebra:

(i) for fixed r and w, $f(r, w, N)$ increases as N increases and tends to $r+1/w$ or infinity as $N \to \infty$, for $w \neq 0$ and $w = 0$ respectively;

(ii) $\Delta f(r, w, N) = f(r, w, N+1) - f(r, w, N) = E(p^{N+1})$ is positive and decreases as N increases;

(iii) $f(r+1, w, 1) > f(r, w, 1)$ and $\Delta f(r+1, w, N) > \Delta f(r, w, N)$;

(iv) $f(r, w, N) = f(r, w, 1)\,[1 + f(r+1, w, N-1)]$.

From (i), (ii), (iii), if $f(r, w, N)$ is plotted against N, the curve formed by joining successive points by straight lines is concave to the axis of N and the curve for $f(r+1, w, N)$ lies wholly above that for $f(r, w, N)$.

In comparing two curves $f(r_1, w_1, N)$ and $f(r_2, w_2, N)$ suppose without loss of generality that $w_1 > w_2$.

If $f(r_1, w_1, 1) \leqslant f(r_2, w_2, 1)$, i.e.

$$\frac{r_1+1}{r_1+w_1+2} \leqslant \frac{r_2+1}{r_2+w_2+2},$$

then

$$\frac{r_1+N+1}{r_1+w_1+N+2} < \frac{r_2+N+1}{r_2+w_2+N+2}$$

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for all N; therefore $\Delta f(r_1, w_1, N) < \Delta f(r_2, w_2, N)$ and the first curve lies completely below the second, except perhaps at the first point.

If
$$\frac{r_1+1}{r_1+w_1+2} > \frac{r_2+1}{r_2+w_2+2},$$

then
$$\frac{r_1+N+1}{r_1+w_1+N+2} < \frac{r_2+N+1}{r_2+w_2+N+2}$$

for sufficiently large N; thus we may have $\Delta f(r_1, w_1, N) > \Delta f(r_2, w_2, N)$ for small N and $\Delta f(r_1, w_1, N) < \Delta f(r_2, w_2, N)$ for large N and the curves may cross; they will cross once at most, and whether or not they do so can be decided by whether or not

$$\frac{r_1+1}{w_1} < \frac{r_2+1}{w_2}.$$

Consistency requires that if (for $n \geqslant 2$)

$$f(r_i, w_i, n) \geqslant f(r_j, w_j, n)$$

then
$$f(r_i+1, w_i, n-1) > f(r_j, w_j, n-1).$$

This is obviously true if $w_i = w_j$, for then $r_i \geqslant r_j$ and so $r_i+1 > r_j$.

If $w_i \neq w_j$ and the curves for $f(r_i, w_i, N), f(r_j, w_j, N)$ do not cross, then

$$f(r_i+1, w_i, n-1) > f(r_i, w_i, n-1) \geqslant f(r_j, w_j, n-1). \tag{2}$$

If the curves cross between N_0 and N_0+1 then either (i) $w_i > w_j$ and $n \leqslant N_0$ or (ii) $w_i < w_j$ and $n \geqslant N_0+1$. If the curves cross at N_0+1 we may have (iii) $n = N_0+1$ and a random choice between the urns.

In case (i) argument (ii) applies; similarly in case (iii) if $w_i > w_j$. In case (ii) and case (iii) with $w_i < w_j$ we must have

$$f(r_j, w_j, 1) > f(r_i, w_i, 1),$$

since the curves have crossed; if also

$$f(r_j+1, w_j, n-1) \geqslant f(r_i+1, w_i, n-1)$$

then by (iv) above
$$f(r_j, w_j, n) > f(r_i, w_i, n)$$

which is a contradiction. Therefore

$$f(r_j, w_j, n-1) < f(r_j+1, w_j, n-1) < f(r_i+1, w_i, n-1).$$

Since $f(0, 0, N) \to \infty$ as $N \to \infty$, $f(r, 1, N) < f(0, 0, N)$ for sufficiently large N, whatever r. Therefore if the number of trials ahead is great enough, the subject should certainly try both urns.

Clearly a similar procedure may be used if the prior distribution is proportional to $p^r q^w$, for some integral values of r and w. Also, the same procedure may be applied to any number of urns.

2. The Weighting of Hypotheses

Suppose that H_x stands for the hypothesis "the proportion of red balls is x"; assume that x can be any real number in $[0, 1]$ and that weights $u(x), v(x)$ are attached to H_x if a red or a white ball, respectively, is drawn.

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If the subject acts on the hypothesis with greatest weight, after r red and w white balls he will choose the H_x for which $ru(x)+wv(x)$ is maximum. If, in the long run, he is to choose the right hypothesis, then this weight must be a maximum for

$$x = \frac{r}{r+w} = \frac{r}{n}$$

say. Using a continuous approximation, this implies

$$xu'(x)+(1-x)v'(x) = 0$$

and

$$xu''(x)+(1-x)v''(x)<0$$

and a solution to this is

$$u(x) = \int_{\frac{1}{2}}^{x}(1-\theta)g(\theta)\,d\theta, \quad v(x) = \int_{x}^{\frac{1}{2}}\theta g(\theta)\,d\theta,$$

where $g(x)$ is positive and continuous in $[0, 1]$ and $u(\frac{1}{2})$, $v(\frac{1}{2})$ are arbitrarily taken to be zero.

Suppose that the subject finds the hypothesis with maximum weight by a scanning process, and that there is a threshold for discrimination between weights. If the maximum weight is in fact at $x = r/n$, then the weight at $x+\epsilon$ is approximately

$$ru(x)+wv(x)+\tfrac{1}{2}\epsilon^2[ru''(x)+wv''(x)] = ru(x)+wv(x)-\tfrac{1}{2}n\epsilon^2 g(x);$$

so that if the threshold constant is c, $H_{x+\epsilon}$ will not be distinguished from H_x if $\epsilon^2<2c/(ng(x))$. Thus $g(x)$ serves to determine a sort of confidence interval.

If $g(x) = 1/x(1-x)$, $u(x) = \log 2x$ and $v(x) = \log 2(1-x)$ and the "confidence interval" is given by $\epsilon^2<2cx(1-x)/n$. This is the simplest form for $g(x)$ if logical necessity is to be satisfied, i.e. $H_1<H_x$ for all n if one white ball has been drawn, $H_0<H_x$ for all n if one red ball has been drawn.

Discussion on Miss Cane's Paper

Dr Mervyn Stone (moving a vote of thanks): Miss Cane has given us a characteristically pleasant mixture of mathematics and life. However, the undertones of the life she describes—people pressing buttons and rats running mazes—are not all pleasant. As the theory becomes more sophisticated, it is not certain that all its applications will be beneficial. In a sense, it may be fortunate that, as Miss Cane lucidly shows, mathematical learning theory is still in its infancy.

In her discussion of La Berge's experiments, Miss Cane seems to rule out subject variation of the parameters θ and p_1. I do not understand her grounds for doing this. If such variations were allowed, the theory would still have predictive power for an experiment of reasonable length like La Berge's. Bush and Mosteller (*loc. cit.*, p. 202) have emphasized the difficulties of estimating the parameters for each subject but, as they point out, maximum likelihood estimation of the parameters in a complicated likelihood function is well within the powers of an electronic computer. For La Berge's data, (θ, p_1) could be estimated for each subject in groups B, C and D, using some or all of the results in the 0·5:0·5 reinforcement schedule. (If all n trials were used, an incidental test of the linear model would be possible, since the empirical distributions of $(\hat{\theta}, \hat{p}_1)$ in the three groups should agree statistically.) The estimates would predict the results obtained under the changed reinforcement schedule. Miss Cane remarks that $p_1(R)$ was roughly $\frac{1}{2}$ for each of the three groups, so it is likely that the analysis I have proposed would still produce evidence against the linear theory. However, if we are to weigh the linear theory against

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some statistical decision theory, since the latter will generally allow subject variations (for example in the subjective probability distributions), it seems only fair to allow them in the linear theory too.

Professor Cox (*J. R. statist. Soc.* B (1958), **20**, 215–232) has argued that if the Bush and Mosteller models "aim at linking the observations to a neurophysiological mechanism" we should "wrestle as vigorously as possible with the resulting statistical complications". I think it would be ungenerous to deny the linear models a possible physiological basis; in fact, the assumption that the previous record is summarized in $p_1(R)$ has a physiological ring about it.

Miss Cane has not drawn attention to the choice there is when the La Berge data and the Galanter and Bush data force the rejection of the simple linear model. Should we try to patch up the linear model or not? This must be largely a matter of taste. Sternberg (*loc. cit.*) has kept the linearity but not the Markovian status of $p(R)$ in order to explain alternation or persistence of response. Luce (*ibid.*) has introduced non-linearity with his Beta model and has had limited success with the overlearning phenomenon. I guess that Miss Cane's approach has been and will be more fruitful because of its greater flexibility. However, there is a distinct danger of anthropomorphism, that is, a little statistician or *rat-stat* inside every rat. We see that the rat-stat is an accomplished mathematician. He can calculate

$$\int p_L(\lambda)\,[1-\exp(-2\lambda t)]\,d\lambda$$

and must therefore be familiar with characteristic functions; he takes Bayes' Theorem in his stride. Another danger lies in the variety of statistical decision procedures to choose from. To reduce this variety, Miss Cane enjoins us to "consider the experiments from the point of view of the rat". Although a highly anthropomorphic recommendation, it is also a rewarding one since, as Miss Cane shows, it gives an ingenious and satisfying explanation of the alternation effect, based on the specific assumption that the rat-stat sees the experiment as two independent Poisson processes. However, I have the impression that Miss Cane is reluctant to be very specific about the mathematical structure of her models—at least in connection with actual data—and would prefer to say, "My model is of this general type and I can make the following qualitative predictions." But a case can be made for employing a precise mathematical model to examine fine-grain structure. The chances of hitting the jack-pot are small but the pay-off would be considerable. In addition, it will be essential to be specific if any physiological tie-up is envisaged. However, I think Miss Cane's attitude is almost justified by the complexities of any fine-grain analysis.

For example, consider the data from the encounter of a human subject with a one-armed bandit. By one model, the subject has a prior distribution $p(\alpha)$ where α is the probability that the right-hand light comes on. After observing a lights on the right and b on the left, he chooses according to the sign of $A(a, b)-\frac{1}{2}$ where

$$A(a, b) = \int p(\alpha)\,\alpha^{a+1}(1-\alpha)^b\,d\alpha \Big/ \int p(\alpha)\,\alpha^a(1-\alpha)^b\,d\alpha.$$

Given an actual record of the rat's behaviour, $R(f)$, $R(f)$, $L(s)$, $L(f)$, ..., how may the model be tested? The model is possible if a $p(\alpha)$ can be found for which sign $[A(a, b)-\frac{1}{2}]$ agrees with the subject's choice for all the a, b pairs presented. If it happened that min $(a-b)$ for the right-hand choices exceeded max $(a-b)$ for the left-hand choices, there are Beta distributions giving a suitable $p(\alpha)$. Also, for any $p(\alpha)$, sign $[A(a, b)-\frac{1}{2}]$ is increasing with a for fixed b and decreasing with b for fixed a, so that the model is invalidated by any $R(s)$ L's or $L(s)$ R's in the record. In general, one could search for discrete distributions for α ($p_i = P(\alpha = i/k)$, $i = 0, \ldots, k$) and agreement with the model would require $\Sigma p_i c_i > 0$ where

$$c_i = \pm\left(\frac{2i}{k}-1\right)\left(\frac{i}{k}\right)^a\left(1-\frac{i}{k}\right)^b$$

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according as the right-hand or left-hand choice is made. Linear programming techniques might be useful here.

For the two-armed bandit, the prior distribution is $p(\alpha)\,p(\beta)$ and, at the stage at which the bandit's behaviour is given by r, w, r', w', one model might prescribe that the subject chooses according to sign $[A(r, w) - B(r', w')]$, where $p(\beta)$ defines B as $p(\alpha)$ defines A. An obvious inconsistency would be if the subject changed arms immediately after a reward but, if this is avoided, the test procedure is even less clear than for the one-armed case.

In both cases, the situation is even more hopeless if estimates of the prior distributions are required, say, for prediction in other parts of the experiments. We cannot rely on the length of the data giving consistent estimates, as is the usual case in statistical problems, for the prior distributions effectively operate only for the early part of the data.

No doubt some models could be easily tested. For instance, a model might say that a subject, facing a two-armed bandit, changes arms at the first failure. Robbins (reference in Vogel (*loc. cit.*)) has shown that, even asymptotically, this procedure is not outrageously bad. For uniform prior distributions for α and β, it is optimal for up to three choices but breaks down for the four-choice case after $R(f)$, $L(s)$, $L(f)$. The best way of testing it might be to ask the subject whether he has adopted it. One can hardly ask such a question for more complicated models.

Finally, I should like to make the cynical observation that even if it turns out that any mathematical superstructure based on Miss Cane's approach proved irrelevant in psychology there are good chances that it would be useful in statistics.

This Society has learnt to expect rewarding thoughts from Miss Cane. I venture to suggest that it has not been disappointed at this trial. I have much pleasure in proposing a vote of thanks.

Dr A. R. Jonckheere (seconding the vote of thanks): It gives me great pleasure to second the vote of thanks to Miss Cane for her stimulating paper, containing both a critique of currently employed linear statistical models for the explanation of simple learning, and some rather bold speculations concerning the possible use of subjective probabilities. I only want to make one or two points of a general nature.

One can take two views concerning the parameters involved in these stochastic learning models. If they are considered as purely descriptive, merely summarizing in compact form some of the salient features of a given sequence of learning trials, it is not really relevant to object that in experiments such as those of La Berge, and Galanter and Bush, one finds that parameter values must be modified according to prior experience and the conditions in which learning takes place. Further, psychologists are by now well aware of the danger of making fallacious inferences concerning the learning process from a study of the characteristics of the "average learning curve". To avoid this, parameter values must be estimated for each learning sequence obtained from each subject, and a group result represented by substituting the mean of these parameter values in the equation expressing the stochastic process employed; and if there are several different parameters involved, they must be shown to be independent. Only in this manner can one avoid Miss Cane's stricture that " . . . if the parameters change with the previous experience of the subject then in any learning experiment different subjects should be assigned different parameters and a linear model cannot represent the learning of a whole group".

It is only if the parameters in these processes are given a rather deep interpretation in terms of the experimental conditions, and the past experience of a subject, that one is entitled to expect their values to remain constant or change in a predictable manner when the learning situation is altered. This approach has been adopted by Estes with his models developed in terms of "stimulus sampling".

I should like here to take the opportunity of drawing the attention of statisticians to the problem of devising a satisfactory test of goodness-of-fit, applicable to these non-stationary stochastic processes which are now being developed by learning theorists. In general, given the unique series of choices, from two or more alternatives, made by a

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particular individual, we require not only to fit a non-stationary process by estimating the parameters involved, but also to be able to assess the adequacy of this fit. Only if this problem is solved will the present-day proliferation of stochastic learning models be held in check.

I come now to the speculation concerning the use of subjective probabilities which Miss Cane has used in developing her own learning models. As has been said, they seem to depend upon the assumption that an individual will act rationally in accordance with certain elementary statistical principles. There are, however, several phenomena which psychologists have observed in the type of situation described by Miss Cane which ought perhaps to be taken into account. When, for example, an experiment is so arranged that at any trial one of two alternative responses is rewarded with probability p, and the other with probability $1-p$, it is usually found that subjects distribute their successive choices so that the probabilities of their choosing each of the two alternatives match the probabilities of their reinforcement. In this situation, using the notation given in the paper, the limit of p_i as i increases is not 1 but p. There seems here to be evidence that subjects are not acting in accordance with ordinary statistical practice, since in order to maximize one's gain, one should of course always choose the side where the reward occurs with greatest probability. This phenomenon of response probability matching has proved difficult to explain, though the stochastic models developed by Estes and others predict its occurrence.

I should like again to thank Miss Cane for her paper, and hope that she will develop further her own type of explanation to the point where it may be tested in the laboratory.

The vote of thanks was carried unanimously.

Dr R. J. Audley: Miss Cane's paper combines a number of interesting speculations with a considerable body of controversial material. It is difficult in five minutes to tease out those statements which especially demand a critical evaluation, so I have selected two points which have aroused the strongest emotional response in me and I then turn in an uncritical way to another matter that is raised in the paper.

First, the preference for subjective probabilities and hypotheses. I do not think learning theories tend to be formulated in terms of rules linking a stimulus to a response because of an application of Lloyd Morgan's Canon, which is after all only the Occam's razor of a particular scientific discipline. The reason for this kind of stimulus–response formulation is to ensure that one's theory can be related to observable events. In cases where the hypotheses that Miss Cane advances can be put in a testable form I do not consider that her approach differs in principle from that of other learning theorists. Whether or not one calls the constructs of a particular theory "subjective probabilities" seems to me to be quite irrelevant, although I do feel that in this paper there is dangerous ambiguity in the way in which the term "subjective probability" is used.

What Miss Cane would really seem to be arguing against is the simplicity of the decision processes which the linear models assume. This is to attack a straw man. The so-called mathematical learning theories are a recent development in psychology and must be fairly regarded as preliminary attempts to derive rigorously the properties of a system satisfying certain assumptions. They are as yet largely essays in methodology, Estes' work excepted. They represent only a small fraction of psychological speculation, and rarely do they aspire to the state of a general theory of learning. Thus it seems to me that the comparison of the notions of this paper with the linear learning models is also irrelevant. The paper therefore demands appraisal as a particular psychological theory.

This brings me to the next point concerning the application of the theory to experimental material. It is here that Lloyd Morgan would begin to spin in his grave. For example, the account of alternation on p. 191 is fantastic speculation. The hypothesis, although couched in dangerously subjective terms, might in principle be reasonable. That is, if it were not for the body of experimental literature on alternation, which is very improperly dismissed

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in this paper as leading only to the conclusion that there is something about rats that makes them alternate. All the evidence points to novelty as being the relevant variable.

Furthermore there are motivational factors which affect the phenomena. Hungry rats alternate more than thirsty rats, or if one controls for the various factors which are also changed by food and water deprivation, this turns out to mean that rats losing weight alternate more than rats gaining weight. Are the rats supposed to be using different principles of inference in the two cases? By analogy perhaps, yes, but not in the way in which the paper uses the term inference.

The third point I want to consider concerns the discussion of the use of probability statements in the description of learning. I accept with some reservations the views on this problem which are expressed in the paper and I am very pleased that the matter was raised. The problem also seems to have important bearing on what kind of data one should employ when testing certain kinds of hypotheses, and what kind of inferences one can draw from data. The problems appear in a wide range of phenomena outside learning, and I should like to exemplify it in the context of choice behaviour.

Several theories have been concerned with the transitivity of preferences. For example, with 3 alternatives *A*, *B*, and *C*, rules have been given for deriving the probability of *A* being preferred to *C*, given the preference for *A* over *B* and *B* over *C*.

Stated as a psychological theory these rules apply to the individual subject. In practice, once a preference has been expressed for discernibly different objects, the subject is likely to repeat this in future so that no individual probabilities can be observed.

If we use the proportion of people possessing a given preference for the purposes of testing the theory, what relevance does this have to the initial hypothesis? The data are statements about the group as a whole and cannot therefore bear directly upon the hypothesis which is supposed, as a psychological hypothesis, to be true for all members of the group. I feel there is a great need for a critical discussion of the kind of legitimate inference which can be made in this kind of situation.†

In concluding I should like to say that I have some sympathy for the kind of approach that Miss Cane advocates, especially since I infer that this is in part a demand for a more realistic approach to decision processes, but in this short note I can only indicate an extremely critical view of the way in which the hypotheses have been expressed, and the manner in which certain psychological experiments have been interpreted.

Dr A. E. Maxwell: Psychologists, and others interested in learning processes, are in great need of two lucid papers. One of these would tell them what really happens when a rat "runs" a maze (for I understand that the word *runs* is much too graceful for describing the antics which take place), and the other would tell them what mathematical models of learning aim at. Miss Cane has made a number of attempts to supply us with the latter and we are indebted to her, but she tends to assume that the reader is already immersed in the subject, with the result that her jewels are sometimes hard to digest.

The end of the first stage in the development of mathematical models of learning seems to have been reached. Until now we have all been rather tolerant of the pioneers, but it now appears that, as Horace so aptly put it a long time ago, "the earth was in travail and a tiny mouse was born". The trouble, as Miss Cane has pointed out, is that the linear models—until now so popular with learning theorists—cannot deal with many of the known facts. She mentions the effect of over-learning on extinction, and the fact that a partially reinforced response extinguishes more slowly than one invariably reinforced. To these might be added several problems which arise when experimental subjects perform

† During the discussion it was suggested that if one knew what assumptions one was making, no mistakes in inference would occur. This is obviously true, but if one has to make the assumption that all individuals are alike in certain respects, when this is known to be untrue, one is again in a quandary. From a statistical point of view it may be better to abandon a problem which involves these difficulties, but I still think it would be worth while to consider what would be the best strategy to employ in approaching this kind of situation.

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under different degrees of motivation and linear models again are inadequate for dealing with the observed behaviour.

But even when we admit that linear models are inadequate and that our models must be made more elaborate we are not out of the wood. The main trouble as I see it lies in the first of the assumptions which Miss Cane quotes in the section of her paper headed "Linear Models". I refer in particular to the sentence, "the change in behaviour observed on successive trials must be the result of the responses and reinforcements that occur". If we assume that the responses referred to here are entirely overt, and thus measurable, we are greatly over-simplifying the facts and it would be easy to present experimental evidence to support this statement. Miss Cane appears to be very much aware of this when she appeals, in the latter part of her paper, to the concepts of decision theorists with their prior probabilities and latent hypotheses weighted this way or that. This section of her paper seems to be an excellent bit of escapism. Nevertheless her treatment of learning problems in decision theory terms is ingenious and makes very pleasant reading. It is, as she admits, purely speculative but the deceptive attraction of decision theory is that it invariably supplies the best strategy for indecision, for the number of unknowns which have to be introduced makes further action virtually impossible. In her "Discussion", however, Miss Cane is unnecessarily worried about her choice between the "addition" and the "multiplication" axioms of probability theory. As Schrödinger (1947) has shown, and perhaps others have, once we adopt the conventions that our measure of probability, p, must lie between 0 and 1 and that the sum of the probabilities of an event and its complement is unity, the *addition* rule follows from the *multiplication* rule. Schrödinger further points out that the habit of introducing the multiplication rule as an axiom into probability theory is quite unsatisfactory. It is anything but self-evident and it is his contention that it could not be so. He goes on to show that by stating a further convention, the axiomatic basis of the rule can be reduced to next to nothing. In view of this it seems doubtful whether de Finetti's challenge to psychologists, which Miss Cane quotes, would be a valuable one to pursue.

Dr I. J. Good: It occurs to me that the tendency of rats to alternate their behaviour during early stages of an experiment might be attributed to two causes not mentioned by Miss Cane. One would be the exploratory instincts of the rat. The other depends on the fact that the amount of food given on any one trial was small, so that the rat might try the other arm of the maze on the next trial in the hope of finding more food on that side. The rat would not know at first that the amount of food to be found would be either nothing or the same small amount as before.

On another point, G. Russell and A. M. Uttley (N.P.L. Symposium, 1959, p. 144) have conjectured that the logarithms of probabilities might be represented in the nervous system by means of delays, one idea being that the logarithms of conditional probabilities could then be obtained by subtraction and so could be represented by simple neural circuits. I have myself extended the conjecture, very speculatively, to "weights of evidence", log-odds, amounts of information, and "tendencies to cause" (Cherry, 1961, p. 128).

Mr N. L. Webb: It occurs to me that one could arrange to be presented with rather more information than is given by a series of 1's and 0's. It is sometimes possible to isolate "decision times" in psychological experiments, and it may be possible to do so here.

If so, would it not then be possible to arrange a trial to associate length of decision time with the complexity of integration sum or whatever it is which is postulated that the rat has to do? At the beginning of the experiment there would be very little information for the rat to go on, but there would be some point in the experiment when whatever one postulated was a more difficult decision to arrive at could be observed to be made within a given time, so that during the experiment variations of difficulty could be associated with variations in decision time. I am not saying that it would happen with rats, because during

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the intervals between trials they would have time to chew over their food and speculate about the information available but it might be done with human subjects where the trials immediately followed one another. This approach also holds out the possibility of eliminating "between subject" variation.

Dr AUDLEY: Observations of decision time provide useful information about the state of an organism on any individual trial, and experiments have demonstrated a relationship between decision time and the probabilities of selecting the various alternatives. Also, most organisms display vacillatory behaviour in any choice situation and this too may be used to augment the rather meagre summary of behaviour given by the sequence of successes and failures.

I should like to describe briefly an experiment which I carried out to test an hypothesis about alternation behaviour which was rather like that advanced by Miss Cane. There were two groups of rats run in a T-maze. For one group, there was a single pellet in each arm of the maze; for the other group, a whole heap of pellets on either side. After choosing, the rat was to be allowed to eat a pellet, and was then to be replaced at the beginning of the maze. Would the rats in the second group tend to repeat their first chance, whilst those in the other group alternate theirs? Broadly, the answer was yes. But, to indicate the difficulties of this kind of experiment, no rat ever ate a pellet. Blocks of wood might just as well be used as pellets and do in fact yield the same results. It is surely difficult to introduce "subjective probabilities" in any guise to account for this kind of behaviour.

Professor G. A. BARNARD: I am not sure whether Buridan's ass was a figment of scholastic imagination or whether Buridan was one of the pioneers in experiments of the type Miss Cane is discussing. But in any case I think the arguments centring around that animal are relevant to the present discussion. You will remember that the ass was supposed rational and actuated by the principle of insufficient reason, nowadays called the principle of invariance. Confronted with a problem invariant under reflections in its ventro-dorsal plane, the ass insisted on a similarly invariant solution and so went hungry. In so doing he made no choice and I think this possibility of making no choice is one which Bayesians tend to overlook. One would have liked to know how many of the rats displayed an intelligent indecision.

I should like to voice a protest against Miss Cane's statement that "inference is not an intellectual process". I do not grasp Miss Cane's distinction between intellectual processes and physiological processes. I should have thought that any intellectual process had a physiological aspect. I am very much with her, however, in thinking that rats are entitled to intellectual respect as are human beings.

Professor BARTLETT: I should like to raise one question connected with alternation. It is to what extent animals—certainly human beings tend to exhibit it and there is evidence that animals do, too—have a predetermined pattern of behaviour, which, presumably, would tend to be eradicated in time. It would be nice if we all had open minds and it would be nice if rats had; but perhaps they have preconceived ideas or patterns of behaviour. That is not nonsensical from the biological point of view because primitive creatures tend to work entirely on instinctive methods, which can be changed only by rather severe conditions. I feel that this is rather important.

One other point concerns the problems of statistical inference in connection with data of this kind. You must be rather careful about the sort of assumptions you are prepared to make from a statistical point of view. If you are not prepared to make statistical assumptions, you are not prepared to make statistical deductions. If you have sequences different from one rat to another, you may get a limited amount of information from the sequences, as such, because there is certainly an amount of repetition, but in each case there is a limit and you will not get very much unless you are prepared to assume that a group of rats are tending to behave somewhat similarly in a statistical sense. The variations

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as well as the similarities can be studied, unless it is assumed that a rat is completely unique and cannot be studied in relation to another rat. The problems are very difficult in that sort of situation, but if clarified in that way, they should not be insuperable.

Mr M. Bud: Professor Bartlett's remarks are borne out by observing players at the Monte Carlo Casino. People start with a predetermined faith that something will follow. Unless there is a slight modification which brings success, a realization that there may be a maze-running process involved results in completely erratic behaviour from then onwards and the risk of their losing faith in rational choice. In other words, they behave randomly without gaining faith in any better hypothesis. In *The Mint* T. E. Lawrence relates how after World War I the R.A.F. endeavoured to make useful soldiers by first demolishing the personalities of recruits in order to substitute new patterns of adaptive behaviour.

Mr A. Battersby: I want to ask a question about alleged optimum behaviour. Miss Cane and Dr Jonckheere both mentioned the fact that if food is put very often on one side, the optimum behaviour is always to go to that side. But is that correct? For example, it was suggested that if the food were on one side for 70 per cent. of the time and on the other for 30 per cent., the optimum behaviour for the rat would be to go always to the side where the food occurred 70 per cent. of the time. However (excluding the case where it was 100 per cent. available on one side), is it not possible that the rat is checking that the experimenter has not changed the conditions? Suppose, for example, that the frequencies were changed from 70/30 to 70/100, the rat would be pursuing a better course if it occasionally explored the 30 per cent. side to see what was going on. That is optimal behaviour in a situation which is likely to change rather than remain static. May not the rat's apparently sub-optimal behaviour in fact be "super-optimal", so that it is learning on a second plane about how the rules change rather than what the rules are?

Dr Good: Even that would not justify the rat's alleged tendency to visit the two sides with relative frequencies equal to those of food placement.

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Miss Cane, in reply: I admit that Dr Audley's criticism seems fairly overwhelming, although I am not clear that novelty means any more than "exploratory behaviour".

Dr Stone thought that I was being rather anthropomorphic and thinking of the statistician inside every rat, but I am, in fact, thinking of the rat inside every statistician.

One objection to linear models, even when you regard them as devices for fitting curves, as Dr Jonckheere prefers, is that they do not seem very good at dealing with initial acquisition. If one looks at learning data, one finds a fairly random response, at least near the beginning of the trial sequence. As far as probability matching is concerned, I have looked at a large number of experiments on this and I am convinced that this effect is not found if the experiment goes on long enough. That was certainly true in the experiment I did. With a 70:30 reward schedule I obtained considerably more than 70 per cent. correct responses after a time.

I was interested in Dr Audley's point about choice behaviour, but I am not convinced that individuals would make the same decision if they were given the same choice of alternatives on different occasions. Anyone presented with a really complicated choice problem such as that of deciding between three jobs, one of which allows more leisure

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time, another provides more money and a third is in a pleasant place, surely finds that he veers towards one or the other as his frame of mind, or circumstances, vary.

I agree with Professor Barnard that I was wrong to say that inference is not an intellectual process. I do not know what the right word is; I was thinking of something like ability to manipulate symbols which human beings have and which animals probably do not.

Predetermined patterns of behaviour may, as Professor Bartlett suggests, very well be important, but the work of the animal behaviourists suggests that patterns which were once thought to be instinctive are learned at a very early age, in the case of birds, even in the egg. It is not altogether clear how one draws a line between instinctive and learnt behaviour.

I am very much in sympathy with Mr Battersby about optimal behaviour. The behaviour I have described is optimal only if the subject knows that reinforcement is random and that the rate of reinforcement will not change. This is one of the reasons why I proposed to use urns in experiments, because subjects could then see that the experimenter was not up to any "monkey business". In experiments with a T-maze or "bandit", the subject cannot know that the situation is stable.

Miss CANE subsequently replied in writing as follows:

I feel that the main problem in regard to the development of learning models is to assess what a model-maker is trying to do, and this requires some specification of what he might do ideally. There seem to be two possible aims: the fitting of curves which may suitably summarize the data, so that comparisons between different conditions may be made in terms of changes in a few parameters; or a description in terms of parameters that may ultimately be related to some neuro-physiological mechanism. Dr Jonckheere seems to prefer the first; but in this case it would surely be simpler to fit the proportion of correct responses to some function of the number of trials, say, to a sigmoid curve or a suitable polynomial. A specific model would be preferable only if, with the same number of parameters, it fitted the data more closely. Moreover, if a model such as the linear model is to be fitted to group data, it would be preferable to allow for variation within the population by making some assumption about the distribution of the parameters. (For the simplest linear model, which involves θ and p_1 only, one might reasonably suppose θ to lie in a Beta distribution, independently of p_1.)

Dr Stone prefers, as I do, the second aim, though, as he says, it presents great difficulties. He has not mentioned another advantage of an exact model—that one may be able to reject it. It seems to me that the whole class of linear models fail in that they predict much faster initial learning than actually occurs, and probably should be rejected for that reason. To Dr Stone's pertinent question as to what should be done next I agree that it is difficult to give an answer; only exploratory behaviour is possible. The particular path I am trying to explore is that leading to models that are crude versions of Bayes' decisions, an example of which was given in the final paragraph. I do not, of course, suppose that rats are familiar with characteristic functions and Bayes' Theorem, but I feel that there should be some sort of Correspondence Principle which would relate decisions made in exact mathematical terms to ones made intuitively, whether by human beings or by animals. In looking for a model of the same type as a Bayes' decision I have not assumed a measure of probability lying between 0 and 1, so I think that Dr Maxwell will find that his remark is not relevant.

I agree with Dr Audley that of course all models must be formulated in terms of stimuli and responses and I am afraid that what I said in the paper was not very clear. The difficulty with learning processes is that one cannot assume an invariant, or statistically invariant, relation between stimulus and response and so some sort of theory based on intervening variables must be used. Such a theory can be tested quantitatively and this is obviously necessary at some stage; it can also be checked qualitatively—this is surely valuable if it helps one to reject impossible theories. I am not sure of the relevance of

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some of Dr Audley's "qualitative" criticisms. Thirsty rats would, I suppose, be less likely to alternate than hungry ones if they relied on their previous experience, since water is generally found in the same place; but this could easily be checked experimentally by varying the position of their usual water supply. As to loss or gain of weight—perhaps statisticians gaining weight more often use minimax decisions.

I am grateful to Dr Good for pointing out that the logarithm of probability might be represented by a delay. I myself thought of the "weight of evidence" as being represented by a change in molecular structure, in a way recently suggested by Gaito (*Psychological Review*, **68** (1961), 288–292).

Finally, I should like to return to the question of what probability of response means in relation to a single individual. Dr Audley suggests that mathematical learning theories should be regarded as attempts to derive rigorously the properties of a system satisfying certain assumptions. I must protest that, no matter how rigorous the derivation, the results are useless if the meaning of the assumptions is not clear.

As a result of the ballot taken during the meeting, the candidates named below were elected Fellows of the Society:

BEDFORD, John Rodney Charles, B.Sc.
BRACKEN, Charles Edward, B.Sc., M.Sc.
BRAITHWAITE, George Roger, B.Sc., M.Sc.
BRAUNSBERG, Hannelore, B.Sc., Ph.D.
BRENNAN, Keiran, B.Sc., M.Sc., M.B., B.Ch., B.A.O., D.P.H.
CARRUTHERS, Peter McCormick, B.A.
CHAKRABARTI, Saurindrakumar, B.Sc., M.Sc.
CHERNOFF, Herman, B.S., Sc.M., Ph.D.
DOWNES, Michael Barrie, B.A.
GRESLEY-GREY, Harold Gilman
HARBORNE, Edgar Sherriff, M.A.
HARDING, Vernon
HOBBS, Clement Francis, B.Sc.
HOBSON, Terence Francis Joseph, B.Sc.
IYER, Jayashree, M.A.
LOW, Charles Frederick James
MCCUNE, Duncan Chalmers, A.B., M.S.
MARKS, Brian Lawton, B.Sc.
MILES, Roger Edmund, B.A.
NARAPALASINGAM, Suppiramaniam, B.Sc.
O'HERLIHY, Callaghan St. John, B.A.
PAGE, William Frank
POPE, Robert
PRINS, Hendrik Johan
QUAYLE, Donald Victor, B.Sc.
RAO, C. R., M.A., Ph.D.
WALFORD, Joan
WEGNER, Peter, B.Sc., M.A.

Corporate Representatives

HALLIER, Elizabeth, B.Comm., *representing* the Ministry of Economic Development, Statistics Branch, Entebbe, Uganda.
WEBB, Terry Peter, *representing* Burroughs Machines Ltd.

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The Prospect for Householders' Local Rates

By A. R. Ilersic

Bedford College, London

[Read before the Royal Statistical Society on Wednesday, February 21st, 1962, Mr B. P. Emmett, Honorary Secretary, in the Chair]

Summary

The Rating and Valuation Act 1961 provides for a new valuation of all rateable property; the new lists are expected to be available by the end of 1962 and will take effect as from April 1st, 1963. This article considers the possible impact of the new lists on the main classes of rateable property, and in particular on householders.

1. Introduction

The local rate is the oldest tax in England and in 1960–61 it yielded £675 m., equal to nearly 12 per cent. of the Budget tax revenues in the same fiscal year. While it is customary to find its origin in the Elizabethan Poor Law Act of 1601, Professor Cannan's researches reveal that a form of the local rate was employed in the parishes in mediaeval times. It was, and still is, intended to be based upon a man's "substance" which in the pre-industrial era was measured primarily in terms of his ownership of land and buildings. It is also, generally speaking, probably the most unpopular of modern taxes. This is not new; since the Royal Commission on Local Taxation in 1896 there have been numerous enquiries, official and unofficial, to consider alternative sources of revenue for the finance of local authority services. The local rate was severely criticized before the second world war, not least as a result of the researches of Professor and Mrs Hicks, as being regressive, inelastic, and militating against good housing, as well as generally unsuited to meet the growing cost of local authority services.

Although the local rate has not attracted the same amount of criticism in post-war Britain, the rating system at present is nevertheless in a most unsatisfactory state. During the Second Reading of the Rating and Valuation Bill in November 1960 an M.P. commented that "the wretched owners of advertising sites seem to be in the unique position in rating law as the only people who pay an entirely undiminished rate on rental values". The truth of this contention is evident in the light of the complete exemption since 1929 of agricultural land together with the 50 per cent. derating currently enjoyed by industrial and freight transport hereditaments, as well as the 20 per cent. abatement of the increased rateable value of commercial premises which arose from the 1956 revaluation; and finally the fact that householders' rates are assessed on valuations which are based upon 1939 rentals. The current confusion over valuation, defined by an ex-Minister as a "tesselated mosaic of abnormalities", has its origins in the political and economic circumstances of the inter-war period. The time has now come, however, or so Mr Henry Brooke, until recently Minister of Housing and Local Government, believes, "to get back towards everybody paying rates on full current values in place of the unsatisfactory chaos of different bases". This, then, is the primary objective of the Rating and Valuation Act 1961.

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2. The Distribution of Rateable Value

The local rate is assessed upon occupiers of property by reference to the rateable value of the property. Of the 16·1 m. separately rated hereditaments in England and Wales on April 1st, 1960, nearly 13·8 m. were domestic, 0·6 m. were shops, and 0·1 m. were classified as industrial. The total rateable value of all these properties was £702 m. The rate poundage itself—the average for England and Wales was 19*s.* 10*d.* in 1960–61—is determined by relating the total expenditure to be met from rates to the aggregate rateable value within the boundaries of the local authority. Given that expenditure, the

Table 1

Distribution of aggregate rateable value in England and Wales, 1955–61

Class of hereditament	1955† £m.	%	1956–57† £m.	%	1957–58‡ £m.	%	1958–59‡ £m.	%	1959–60‡ £m.	%	1960–61‡ £m.	%	1961–62‖ £m.	%
Domestic	219	59·8	308	49·4	315	48·4	321	52·6	327	47·6	334	47·6	341	47·4
Industrial and freight transport	16	4·4	41	6·5	41	6·3	42	6·9	80	11·6	80	11·4	83	11·5
Shops	39	10·7	89	14·3	89	13·6	73	11·9	75	10·7	76	10·8	77	10·7
Commercial	78	21·0	170	27·3	87	13·3	70	11·4	73	10·6	77	10·9	81	11·2
Miscellaneous					86	13·2	76	12·4	103	15·0	104	14·8	107	14·8
Crown	15	4·1	15	2·5	34	5·2	29	4·7	30	4·4	31	4·4	31	4·4
Totals§	367	100·0	623	100·0	652	100·0	611	100·0	688	100·0	702	100·0	720	100·0

† Cmnd. 9718. Old and new values at December 1st, 1955, the new values came into effect April 1st, 1956.
‡ Cmnd. 1258. Values at April 1st, except 1957–58 which are as at March 31st, 1957.
§ Totals may not agree exactly due to rounding.
‖ Added after paper was read.

lower the rateable value within the boundaries of any particular rating authority, the higher then will be the rate poundages. Before 1939 there was considerable variation in the interpretation of the law relating to valuation, which led to inequity in the distribution of the rate charge both between different classes of hereditament within any single valuation area, and between comparable properties in different valuation areas. It was therefore no surprise when the function of valuation for rating purposes was transferred from the local authorities to the Board of Inland Revenue by the Local Government Act of 1948. Since then only one valuation, that of 1956, has been carried out. This has been authoritatively described in a recent study of rating sponsored by the Institute of Municipal Treasurers and Accountants (1958) as eliminating most of the worst inequities and variations between different rating areas which existed before that date.

The results of the 1956 revaluation are shown in the first two columns of Table 1. The percentage figures given alongside indicate the proportion of every £100 of rate revenue contributed by each class of hereditament irrespective of the rate poundage. They also indicate the relative change—consequent upon the revaluation—in the proportionate share of the aggregate rate revenue collected in England and Wales

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from each of the main classes of hereditament. Nearly three-quarters of a million appeals were lodged against the new valuations, but only a small proportion of these came before the Courts.

The increased valuations of shop and commercial property following the 1956 revaluation, and reflected in the larger share of rateable value accounted for by those classes, provoked extensive protests from those affected. In the 1957 Rating and Valuation Act the Government conceded a 20 per cent. abatement on the new rateable value of shops, offices and other non-industrial and non-domestic buildings, limited to the period until April 1963, i.e. the next revaluation. The still sharper relative increase in industrial values did not attract any relief because at the time industry still enjoyed the 75 per cent. derating accorded it in 1929. Thus the main beneficiaries of the net changes in 1956 and 1957 were householders. Although there was an absolute increase in aggregate domestic valuations from £219 m. to £308 m., their contribution to the total rate revenue in England and Wales fell from 59·8 to 49·4 per cent. The reduction in rate poundage which accompanied the revaluation, from 22*s.* 10*d.* in 1955–56 to 15*s.* 8*d.* in 1956–57, meant that many householders, more especially in the London area, paid less rates in 1956–57 than in the preceding year, despite the increase from £401 m. to £492 m. in local authority rate-financed expenditure in the same period.

The second part of Table 1 summarizes the distribution of rateable value during each of the five financial years 1957–58 to 1961–62. The main feature of these years is the virtual doubling of the total industrial and freight transport rateable value, due largely to the reduction from 75 to 50 per cent. in the 1929 derating allowance. The increase in Crown property values, although small in absolute terms, was significant in sharply increasing its relative share of total rateable value. This was because the revaluation of Crown property was in 1957, not in the previous year on the occasion of the 1956 revaluation. Reviewing the figures over the period 1955 to 1960, the outstanding change is the sharp increase in both the relative and absolute contribution of industrial property to local rate revenues, compared with the favoured treatment of domestic property whose relative contribution has dropped from almost 60 per cent. to 47·6 per cent.

Table 2 illustrates the facts of the first table in money terms by showing the cash contribution to aggregate rate revenues made by each main class of hereditament. For the sake of completeness all the relevant information relating to the rates in 1955 to 1961 is given, including in the bottom horizontal row of Table 2 the average rate poundage levied in each year. Owing to vacancies, losses, etc., the effective rate collected per £ of rateable value is actually a few pence lower than this figure in each year. Although the relative contribution to rate revenues of domestic property has dropped from 59·8 to 47·4 per cent., the contribution in money terms has risen by over one-third, from £240 m. to £345 m. The same table brings out the most significant feature of local authority finance during these years, i.e. the rapid growth of expenditure met from the rate. The increase of £327 m. between 1955–56 and 1961–62 represents an increase of over 80 per cent. on the 1955–56 total of £401 m.

The increase in real terms, however, was not quite so marked. By deflating the money totals of rate revenues given in Table 2 by the index of prices of consumer goods and services used in the National Income Blue Book, a new series is derived giving an increase from £401 m. to £605 m. in constant money terms, representing an increase over the quinquennium of 50 per cent. This compares with the 69 per cent. rise in money outlays. This upward trend in local government expenditure, in both

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real and money terms, will continue, just as will the growth in the total rateable value as a result of new construction. Since the prospective revaluation will not be effective until April 1st, 1963, some consideration of the changes expected in both rate revenues and rateable values is needed before making any attempt to assess the probable impact of the revaluation.

Between April 1957 and April 1960 total rateable value in England and Wales rose from £602 m. to £702 m. Of this increase, £36 m. was due to industrial re-rating, i.e. the change from 75 to 50 per cent., so that new rateable value totalled £64 m.,

TABLE 2

Estimated distribution of rate revenue by class of hereditament. Annually 1955–61.† £ m.

Class of hereditament	1955–56	1956–57	1957–58	1958–59	1959–60	1960–61	1961–62
Domestic	240	243	256	290	308	321	345
Industrial and freight transport	18	32	32	38	75	77	84
Shops	43	70	72	66	69	73	78
Commercial	84	134	71	64	69	74	82
Miscellaneous			70	68	98	100	107
Crown	16	13	27	26	29	30	32
Total rate revenue‡	401	492	528	552	648	675	728
Total rate revenue at constant 1955–56 prices§	401	469	490	499	586	605	627
Average rate in £ levied‡	22*s.* 10*d.*	15*s.* 8*d.*	18*s.* 0*d.*	18*s.* 4*d.*	19*s.* 0*d.*	19*s.* 10*d.*	20*s.* 9*d.*

† Based on data in Table 1.
‡ *Rates and Rateable Values*, Table G.
§ Adjustment to 1955–56 prices made by deflating money totals by index of prices of consumer goods and services. National Income Blue Book, 1961, Table 15. Final figure estimated.

representing an annual growth of about £21 m. If this rate of increase is maintained until April 1963, then on the basis of current values, total rateable value will amount to about £765 m. It is hardly practicable to base this projection on a longer period than 1957–60 owing to the changes in valuation in 1956, but since the tempo of new building during the past three years seems to have been rather less than in some years before 1956, and since current restrictions on building, both direct and indirect, will doubtless be maintained, the continuation of this short-period trend seems quite likely to yield a fairly reliable figure for the total rateable value in England and Wales in April 1963.

For the same reasons, and despite the grant changes in 1958, it is probably safer to project the rate-financed expenditure for the next three years on the basis of the trend of rate revenues since 1956–57. Since then, rate revenue has risen from £492 m.

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to £675 m. in 1960–61, an annual increase of £46 m., representing a rate of growth of just over 8 per cent. per annum. It is highly unlikely that this rate of growth in rate-financed expenditure will decline in the next few years. It may well increase, more especially if the government reduces its grant aid to local authorities. For example, when the grant structure was revised in 1958, although the central government's contribution to local authority total expenditure was higher absolutely than in previous years, relatively it was not so great. However, if it is assumed that, at least until the revaluation, no change in the proportion of local authority rate- and grant-financed expenditure met by grants will be made, then outlays to be financed from rate revenues in 1963–64 should total about £813 m. Thus, whereas in 1960–61 revenue of £675 m. had to be raised from £702 m. of rateable value yielding an effective rate in the £ of 19*s.* 3*d.*, in 1963–64 on the foregoing assumptions £813 m. of revenue will be required from a total rateable value of £765 m. so that the effective rate poundage would rise to 21*s.* 3*d.*, i.e. an increase of just over 10 per cent. in the individual ratepayer's annual rate bill before any revaluation takes place.

3. The 1963 Revaluation

Apart from the fact that valuations are to be based on current rentals, nothing definite can be said about the probable contents of the 1963 valuation lists. The Minister of Housing and Local Government disclaimed any foreknowledge of the precise impact of this change upon the different classes of ratepayer when he opened the Second Reading debate in November 1960. It is, however, possible to make certain tentative estimates on the basis of the Minister's remarks as to what may be expected. Clearly, since valuations depend on rentals, the distribution of the total rateable value between the main classes of hereditament in April 1963 will be determined by the extent of the increase in rentals since the last valuation. In the case of domestic dwellings it is generally agreed that uncontrolled rentals are now about three times as high as in 1939. Admittedly, the increase varies from one part of the country to another. In Central London and other conurbation centres rent increases are often greater than this; in the smaller provincial towns and rural areas they may not be quite as great. The Minister has also stated that properties still controlled by the provisions of the 1957 Rent Act will be valued on the basis of their probable rental in a free market. Assuming this estimate of an average rise of 200 per cent. is approximately correct, the effect will be to treble current valuations of domestic property.

In respect of industrial hereditaments there is scope for greater variation. The loss of the current 50 per cent. de-rating after April 1963 will mean an automatic doubling of current rateable values. There has undoubtedly been some increase in rentals for industrial property in the larger towns. Mr Philip Bean, a chartered surveyor and rating valuer, in his paper to the 1961 conference of the Rating and Valuation Association stated that there was evidence of a 50 per cent. increase since 1956 in rentals for such property. If this is taken into account, as well as the increase arising from the de-rating, then industrial rateable values could also be trebled in 1963. The same problem exists in connection with shop and commercial properties, which will lose the current 20 per cent. relief, thereby automatically raising their valuations by 25 per cent. According to Mr Bean, however, there is ample evidence throughout the country of rental increases for this class of property since 1956, when it was last valued, of 66⅔ per cent. On the basis of his figures, 1963 valuations for this class of

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hereditament would be 2·1 times greater than the 1956 figures. The miscellaneous class contains licensed premises, public utilities and educational and cultural institutions, as well as places for entertainment and recreation. Their basis of valuation is rather different from the earlier classes, and a more modest increase of 50 per cent. in valuation is generally expected. Crown property pays a sum in lieu of rates and may be expected to follow the upward trend. An increase of 50 per cent. seems a reasonable expectation.

The scale of the rise in rateable value of each of these main classes of hereditament is unknown and opinions vary as to the outcome. In contrast with Mr Bean's predictions, another authority, Mr Chisholm, County Treasurer of Cheshire, has forecast that domestic valuations will be more than trebled in the aggregate (Chisholm, 1961); given his assumption that rental *increases* in London are equal to 300 per cent., while in the County Boroughs and country districts the corresponding increases will be 250 and 200 per cent. On the other hand, he expects only a doubling of industrial hereditaments due to the loss of de-rating, and a mere increase of about 50 per cent. for shops and commercial properties, part of which is accounted for by the loss of their 1957 relief. Compared with these figures, those of the Federation of British Industries—which have a very strong interest in the trend of the local rate—are less pessimistic about the outcome for householders since, unlike Mr Chisholm, they expect industrial and commercial valuations to increase sharply by 175 and 200 per cent. respectively, although this is partly offset by their estimate of a 300 per cent. increase overall in domestic valuations. These differing expectations produce some quite striking variation in the proportions of the total rateable value—and in effect rate contribution—to be contributed by domestic property as a class after April 1963. Compared with the figure of 47·6 per cent. in April 1960, Mr Chisholm's estimate for April 1963 is 66·6 per cent. (in his opinion to be abated by the Minister—see below—to 59·3 per cent.), while Mr Bean estimates the corresponding percentage at 55·8 per cent. and the Federation of British Industries at only 53·2.

The distribution of rateable value in England and Wales after April 1963 is thus dependent upon the adjustment made to each main class of rateable values depending upon the changes in rental values since the last revaluation. Table 3 sets out the results in terms of the relative contributions from each class to every £100 of rate revenue to be raised in 1963–64 on the basis of varying assumptions regarding the increases in rateable values for each class. The first two columns show the actual, i.e. April 1960, valuations of each class and their percentage distribution. The next four main columns give the factors for converting the current valuations of each class of property into 1963 values and in each case the relative contribution to each £100 of rate revenue, as well as the estimated total rateable value arising from using each set of conversion factors.

Given that the assumptions on which the conversion factors are based are reasonable, then the scope for variation in the results is considerable. For example, domestic valuations which currently comprise 47·6 per cent. of the total (i.e. domestic ratepayers pay £47 12*s.* 0*d.* in every £100 of rate revenue) may rise to 56·9 per cent., or even as high as 69·2 per cent. This last figure is almost certainly too extreme for, as can be seen from the conversion factors employed, it presupposes that domestic rentals have risen on average 250 per cent. since 1939, while industrial, shop and commercial property rentals have remained unchanged since 1956. Hence the conversion factor for the latter class merely allows for the loss of the current 50 per cent. de-rating of industry as well as the 20 per cent. abatement granted in 1957 for shops and commercial

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property. A similar 25 per cent. increase is applied to the remaining two classes which are assumed to keep approximately in pace with the others.

The obvious fact which emerges from Table 3 is that the increase in the share of any one class, e.g. domestic property, must be matched by a corresponding decrease in the relative share of one, or more, of the others. It is quite clear that the domestic ratepayer will bear the main impact of the revaluation, while shops and commercial properties in particular will contribute proportionately less of total rate revenues

TABLE 3

Estimated re-distribution of aggregate rateable value in England and Wales between main classes of hereditament after April 1963 on various assumptions

Distribution as at April 1st, 1960			*Assumptions about rental increases for 1963 lists*							
			A		B		C		D	
Class of hereditament	*Total rateable value* £m.	*Percentage distribution*	*Conversion factors*	*% distribution*	*Conversion factors*	*% distribution*	*Conversion factors*	*% distribution*	*Conversion factors*	*% distribution*
Domestic	334	47·6	3·0	56·9	3·5	60·5	3·0	64·2	3·5	69·2
Industrial and freight transport	80	11·4	3·0	13·6	3·0	12·4	2·0	10·3	2·0	9·5
Shops	76	10·8	2·1	9·0	2·1	8·2	1·25	6·1	1·25	5·6
Commercial	77	10·9	2·1	9·1	2·1	8·3	1·25	6·2	1·25	5·7
Miscellaneous	104	14·8	1·5	8·9	1·5	8·1	1·5	10·2	1·25	7·7
Crown	31	4·4	1·5	2·6	1·5	2·4	1·5	2·9	1·25	2·3
Totals† £m. %	702	100·0	1762	100·0	1929	100·0	1555	100·0	1689	100·0
Percentage increase over 1960 in share contributed by Domestic class			19%		27%		35%		45%	

† Totals may not agree due to rounding.

than at present. This conclusion undoubtedly corresponds to the Minister's views for in the Second Reading debate he referred to the prospect that "householders would carry more" while "industry would bear much the same burden, and commercial property (including shops) less, which, I would say in passing, is in itself not unfair because the commercial property was hard hit at the last valuation" (Hansard, 1960). On the other hand, the Minister believes that "householders can least stand this severe upward jolt in their share of the rate burden" and he has therefore taken powers in Section 2 of the 1961 Rating and Valuation Act to enable him to abate the increases in domestic valuations where he considers them to be excessive. The consequences of any such relief to the domestic ratepayer will be to shift a larger part of the rate burden on to the other classes. For example, suppose that the 1963 lists gave the results set out under Assumption B in Table 3, then a flat rate national rebate of 25 per cent. on

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the increase experienced by domestic property as a class would produce a result somewhat similar to that in the column headed Assumption A.

Which of the four alternatives will approximate most closely to the final lists presented by the Inland Revenue to the Minister can only be guessed. Substantial changes in the relative contributions of each main class of ratepayer are to be expected when the largest class of valuations is almost twenty-four years out of date, and those for two other main groups will be only seven years old by April 1963. On the

TABLE 4

Proportionate change in relative shares of total rateable value contributed by each main class of hereditament. 1956–57 *actual figures,* 1963 *estimated*

	Class of hereditament					
	Domestic	*Industrial and freight transport*	*Shops*	*Commercial*	*Miscellaneous*	*Crown*
	%	%	%	%		%
1956 revaluation†	−17·4	+47·7	+33·1	+30·0		−37·7
1957 figures related to 1955	−24·1	+43·2	+27·1	+26·0		+26·8
1963 increases over 1960 values:—						
Assumption A	+20	+19	−17	−17	−40	−41
B	+27	+ 9	−24	−24	−45	−46
C	+35	−10	−43	−43	−31	−34
D	+45	−17	−48	−48	−48	−48

† Figures from Cmnd. 9718; all others estimated.

other hand, the realities of political life cannot be ignored. Some slight guidance to the inter-class transfer which may be acceptable to the Minister may be derived from his statement in the House of Commons that he would not wish to see the share of domestic ratepayers increase by as much as 30 per cent. This suggests that the results derived from applying Assumption B, i.e. from 47·6 to 60·5, representing an increase of 27 per cent., could be fairly close to what the Minister considers to be possible for domestic property to bear. It also seems highly probable that Assumptions C and D give rather unrealistic results in the light of the Minister's statement unless, of course, he is prepared to abate the increase in domestic valuations on a national basis. This would appear unlikely since the impression gathered from his speech during the Second Reading of the Rating and Valuation Bill in November 1960 was that only selective abatement, as and when justified by hardship, was to be given.

An indication of the measure of the Minister's problem is provided by comparing the class changes under the various assumptions with the class changes which followed the 1956 revaluation. They are set out in Table 4. The reason for inserting the 1957 figures in this table is that Crown properties were not revalued in 1956, but in the year after, so that by itself the 1956 figure gives a false impression of the re-distribution following that revaluation. Which particular set of assumptions for converting current valuations on to a 1963 rental basis give the best approximation to the final outcome is debatable. It is, however, immediately apparent that the position of the domestic householder will be very different from that following the previous revaluation of 1956. Whereas in 1956 the share of domestic ratepayers fell by 17 per cent., in

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1963 it may rise by as little as a fifth or as much as one-half. It is thus not surprising that discretionary powers of abatement have been acquired by the Minister, and it would perhaps be uncharitable to refer to the fact that 1963 will probably be an election year. There is admittedly a precedent for such abatement. A similar course was followed for shop and commercial property in 1957, after the 1956 increases. The Minister has, however, promised the representatives of industry that the abatement of domestic rates—which will shift a larger share of the rate burden on to industry, among other classes—will be temporary only, i.e. until 1968. Thus the smaller the abatement, the easier in terms of public outcry will it be to remove it in 1968. On the assumption that the outcome of the revaluation will approximate to the distribution of rateable value given in column B of Table 3, to what extent would relief for the domestic ratepayer be needed?

4. The Effect on the Householder

The total rate revenue raised in 1960–61 was £675 m., of which domestic property as a class contributed £321 m. (Table 1). In 1963–64 domestic property would, given the earlier projections and in the absence of any revaluation, be paying *as a class* £387 m , representing an increase of £66 m., i.e. 21 per cent. over 1960–61. In the absence of revaluation the individual householder, despite this rising expenditure, but because of the larger number of hereditaments charged in 1963–64 compared with 1960–61, would find his individual rate-charge higher by only 10 per cent., i.e. the increase in the effective rate poundage from 19*s.* 3*d.* to 21*s.* 3*d.* as estimated on p. 214. What, however, is likely to be the combined effect of these various increases in local authority rateable value, rate poundages and the revaluation on the individual domestic householder? It has been estimated earlier that total rateable value will amount to £765 m. by April 1963, so using the conversion factors in Assumption B of Table 3, and on the assumption that the additional rateable value created between April 1960 and April 1963 is divided *pro rata* between the classes, then the total rateable value under Assumption B—regarded as the most realistic—will be £2,102 m. in April 1963. Of this total, 60·5 per cent., equal to £1,272 m., is accounted for by domestic property. Given the foregoing figure of total rateable value and the estimated sum to be raised in rate revenue in 1963–64, the rate poundage in that year will be 7*s.* 9*d.*, i.e. £813 m. divided by £2,102 m. Thus, if after revaluation the aggregate rateable value of domestic property is equal to £1,272 m. and the rate poundage in 1963–64 is 7*s.* 9*d.*, the contribution to total rate revenue from domestic property as a class will be £491 m. This is an increase of £170 m. over the 1960–61 contribution of £321 m. and represents a rise in domestic ratepayers' total contributions of 53 per cent. between 1960–61 and 1963–64. The actual impact upon this class of ratepayers as individual householders will not be so marked since, as stated above, even without the revaluation, the aggregate domestic rateable value would by 1963–64 have totalled £364 m. This figure, with a rate poundage of 21*s.* 3*d.*, would produce the rate contribution of £387 m. given above. The post-revaluation domestic contribution is estimated at £491 m., only 27 per cent. higher than the expected 1963–64 charge without revaluation.

The Board of Inland Revenue (1961) has published in its 103rd Annual Report a breakdown of domestic rateable value at April 1960, reproduced in the first three columns of Table 5. From those figures it is possible to make an estimate of the actual rate payable on a dwelling in each class. Thus, the division of the total rateable value in each class by the corresponding number of dwellings yields the mean rateable

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value of dwellings in that class. In 1960–61 the effective rate poundage in England and Wales was 19*s.* 3*d.*, and in column 4 are given the estimated rate payments per dwelling in each class to the nearest £ in that year. The Ministry of Housing and Local Government annual publication *Rates and Rateable Values* distinguishes between the estimated average rate in the £ *levied* and the amount *collected*; for present purposes the latter figure is used. In the next column, (5), the figure represents the probable rate payment for 1963–64 that would be made without the revaluation by an average

TABLE 5

Estimated domestic rate payment by class of domestic dwelling

Houses and flats with rateable value (1)		*April 1st, 1960*		*Estimated rate payment based on*		
Exceeding	*Not exceeding*	*Number* (2)	*Value* (3)	*Current valuations £ per dwelling*		*April 1963 valuation £ per dwelling*
£	£	m.	£m.	(4) 1960–61	(5) 1963–64	(6) 1963–64
—	10	2·1	16·9	8	9	11
10	13	1·1	12·8	12	13	17
13	18	2·0	32·2	16	18	22
18	25	3·4	75·1	22	24	30
25	30	1·8	48·8	27	30	37
30	40	1·6	55·4	35	39	49
40	50	0·8	36·4	45	50	63
50	60	0·4	19·5	55	61	77
60	—	0·3	30·8	103	113	144
All classes		13·4	328·0†	19*s.* 3*d.*‡	21*s.* 3*d.*‡	7*s.* 9*d.*‡

† The difference between this total and that given in Table 1 for 1960, i.e. £334 m., is accounted for by the omission of agricultural dwellings.

‡ Amount collected per £ of rateable value.

householder in each class of property, on the assumption that domestic rateable value and rate-financed expenditure have increased between 1960–61 and 1963–64 to the extent estimated earlier. Those estimates suggested that domestic rateable value without any revaluation would increase by 9 per cent. from £334 m. to £364 m., while its contribution in rate revenue would increase by 21 per cent. from £321 m. to £387 m. The combined effect of these two factors working in opposite directions would raise the individual householder's actual payment by just over 10 per cent.

In the final column, (6), are given the probable payments after the revaluation, but taking into account the increased number of rateable domestic hereditaments (which accounted for the 9 per cent. increase in domestic rateable value between 1960–61 and 1963–64), as well as the higher expenditure taken into account in estimating the figures in the previous column. The overall effect is to raise the individual householder's 1963–64 rate payment about 27 per cent. above the payment he would have made in that year but for the valuation. Clearly, the accuracy of this prediction depends on the correctness of Assumption B in Table 3. If, for example, either Assumption C or D were to be nearer the Minister's final valuation list, then the increases in the householders' rate payment would be larger, i.e. 35 and 45 per cent. respectively.

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5. Regional Differences

While the national average serves to provide an indication of the probable change in the rate liability for householders as a class, like all averages it conceals wide variations about the mean. Two other considerations must be taken into account in evaluating the probable effects of the 1961 Act upon domestic ratepayers. First, there is the fact that the impact of the revaluation will be greater upon the modern dwelling which nowadays attracts a higher rental than the less attractive older house. There

Table 6

Effect of different assumptions about (i) *rental increases and* (ii) *differing distributions of rateable value, between classes of hereditaments on the post-revaluation contribution of domestic hereditaments to total rateable value*

(a) *County Boroughs*

Class of hereditament	*Birmingham* %		*Brighton* %		*Liverpool* %		*Middlesbrough* %		*Plymouth* %		*Southampton* %	
Domestic	41		53		42		48		49		46	
Industrial	16		3		10		12		8		12	
Shops and commercial	23		30		11		22		23		16	
Miscellaneous and Crown	20		14		37		18		20		26	
Total	100		100		100		100		100		100	
Assumptions:	B	D	B	D	B	D	B	D	B	D	B	D
Percentage share of Domestic rateable value in total rateable value	54	63	67	75	57	64	60	70	63	71	61	70
Relative increase in proportionate share	32	54	26	43	36	50	25	45	28	45	32	52

(b) *Counties*

	Devon %		*Durham* %		*East Sussex* %		*North Riding* %		*Northumberland* %		*West Sussex* %	
Domestic	56		46		67		44		45		62	
Industrial	4		22		3		19		13		5	
Shops and commercial	22		22		16		23		30		18	
Miscellaneous and Crown	18		10		14		14		12		15	
	100		100		100		100		100		100	
Assumptions:	B	D	B	D	B	D	B	D	B	D	B	D
Percentage share of Domestic rateable value in total rateable value	70	77	59	66	79	85	55	65	57	66	71	81
Relative increase in proportionate share	25	37	28	43	18	27	25	48	26	47	20	30

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has been a marked shift in the public's preferences for types of accommodation. In particular, bungalows, more especially those in the favoured parts of the country such as the South-East, and flats and maisonettes in central urban areas are likely to sustain a greater relative increase in valuation than the conventional house, especially the larger and older types. It is, of course, quite impossible to estimate the probable differences from the norm in these cases, but there seems little doubt in professional circles that some householders will get a shock. Even the Minister has gone so far as to declare during the Second Reading that "it is perfectly clear that increases in valuations will be substantial".

Another complication in determining the prospective increase in valuations is the varying proportion of domestic property in the total rateable value in different rating areas. If the conversion factor for adjusting current values to 1963 values in each class of hereditament were the same, i.e. if the same proportionate rental increase had been experienced by all classes, then the revaluation would merely increase the aggregate rateable value but leave its distribution between the main classes of hereditament unchanged. Given, however, the different conversion factors required to bring each main class of hereditament on to 1963 rental values, it is obvious that (i) the higher the conversion factor for domestic properties *vis-à-vis* the factors for other classes, and (ii) the lower the proportion of domestic property in a valuation area, the heavier will be the impact of the revaluation on that class. The composition of total rateable value in the individual counties and county boroughs varies widely. For example, if the ratio of domestic property alone to the total rateable value in each county is taken, there is some dispersion about the national figure of 47·6 per cent. In some residential counties such as Sussex and Surrey, domestic property accounts for over 60 per cent. of total rateable value; in others such as Durham, the North Riding of Yorkshire, and Wiltshire, the ratio of domestic property is only 45 per cent. Among county boroughs, Birmingham and Liverpool derive just over 40 per cent. of the rate income from domestic property; in Brighton the figure is 53·3 per cent.

The foregoing conclusions can be demonstrated by means of Table 6. For each of six counties and six county boroughs the breakdown of rateable value is given. While the Domestic and Industry figures are directly comparable for each area, the two other classes may be affected slightly by slightly inconsistent grouping, but it does not affect the main picture. Using the conversion factors under Assumptions B and D in Table 3, i.e. those which were considered most likely to produce the result most acceptable to the Minister, and the most extreme in terms of the additional burden on householders as a class, the proportion of rateable value contributed within each area to the total rateable value after the revaluation is given. For example, domestic property in Birmingham, which now represents 41 per cent. of all rateable value, will after April 1963 contribute 54 per cent. or 63 per cent., depending on which assumption, B or D, is nearer to the facts. The final row of figures in each half of the table merely expresses the change under Assumptions B and D as a ratio of the current figure of domestic rateable value. The conclusions are obvious. It should not be assumed, however, that these figures purport to give any indication of the prospective re-distribution of rateable value in these areas after April 1963. The assumptions B and D apart, the table is based on two further dubious assumptions, i.e. that rents within each class of hereditament have risen equally throughout the country, and that the ratio of rental increases between the classes is also constant for England and Wales as a whole. The sole object of Table 6 is to verify the assertions made above that, in areas with a low proportion of domestic property, rate increases after April 1963 could

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be—the Minister's powers of abatement apart—higher than in those areas where domestic property accounts for a high proportion of all rateable value.

6. The "Burden" of the Rate

Whatever the increases in domestic rates, there can be no doubting the chorus of criticism and protest which will ensue. How far that reaction can be attributed to anything more than a natural dislike of all taxes and the local rate in particular is

Table 7

Income distribution of two samples of house purchasers

Co-operative Permanent Building Society†			*Alliance Building Society*‡		
Annual income	*All borrowers* Number	*Per cent.*	*Weekly income*	*All borrowers* Number	*Per cent.*
Under £500	630	6·6	Under £12 10*s.*	1,380	32
£500–£749	4,525	47·4	Under £15 8*s.*	1,264	30
£750–£999	2,703	28·3	Under £19 5*s.*	939	22
£1,000–£1,249	939	9·8	Under £29 0*s.*	532	13
£1,250–£1,499	349	3·7	Over £29 0*s.*	138	3
£1,500 and over	399	4·2			
	9,545	100·0		4,253	100·0

† Data based on applications for loans granted between September 30th, 1959–March 31st, 1960.
‡ Data based on applications for loans granted between January–November 1960.

difficult to assess, but the available evidence does not suggest that many protests will be justifiably based on grounds of hardship. Using the data given in the 1961 Blue Book on National Income and Expenditure, if the domestic ratepayers' share of total rate payments is related to consumer expenditure at current prices, then the proportion of the latter accounted for by rates in 1938 was 3·37 per cent.; it was only 2·02 per cent. in 1956 and in 1960 it was 2·19 per cent. This relatively slight impact of the local rate on average incomes is confirmed by the Household Expenditure Enquiry of 1953–54 and the recently published Family Expenditure Survey relating to 1957–59. The proportion of average weekly expenditure accounted for by the collective item "Housing" was 8·7 per cent. in the earlier survey and 9·2 per cent. in the later enquiry. Since this covers rent, rates, repairs and maintenance, it can hardly be asserted that this tax constitutes a particularly severe burden on the average household.

Although the two household expenditure enquiries collected information relating to owner-occupiers, the latter are not given as a separate group. According to the White Paper on the *Enquiry into Rents* (Cmnd. 1246), the proportion of all non-council accommodation units found to be owner-occupied had risen between 1948 and 1958 from 33 to 51 per cent. A sharp increase in rate charges might impose an onerous and unexpected burden on this group. No information is available about the expenditure habits of owner-occupiers as a group, but two Building Societies, the Co-operative Permanent and the Alliance, have published information about the incomes of a sample of house purchasers borrowing from them, which is partially reproduced in Table 7. The data from the two Building Societies are not quite comparable in so far as they classify incomes differently, but the picture which emerges from the two sets of data is much the same. In many of these households it is probable

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that there are other earners, although the Building Societies when granting a mortgage take into account only the income of the head of the household. In assessing the capacity of these households to bear the additional rate burden which may arise in 1963–64, which on average has been estimated in Table 5 to be between £6 and £14 for houses valued currently at £18 to £40 p.a. respectively (these comprise in the main the type of dwelling acquired by owner-occupiers), due account must be taken of the probable increase in incomes which will take place between 1961 and 1963.

7. Conclusions

Some of the concern which has been expressed in the House of Commons over the impact on domestic ratepayers of the revaluation in April 1963 seems to be based more on the known public dislike of the local rate than on the actual increase in the rate charge which the average household may expect. There will undoubtedly be areas where the increase will exceed the average, just as there will be individual cases of hardship. The Minister has power to abate the new valuations where he feels that it is necessary, although it seems probable that for administrative reasons any such relief will have to be given on an area basis and will thus accrue to large sections of householders irrespective of their individual needs. The significance of the 1961 Rating and Valuation Act is that it offers the possibility of putting the rating system on to a sound and equitable footing with all classes of hereditament valued on a current rental basis. To the extent that any sectional relief is granted by the Minister, the attainment of this goal is delayed.

There is, however, a more important aspect to consider. Given the larger resources of rateable value available to them after April 1963, local authorities will have the means to raise a very much larger proportion of their total current expenditure from rates. They may well be compelled to use them, if the Government decides that the new resources reduce the dependence of the local authorities on central grants. Such a policy would appear to be in line with the trend of thinking in Conservative circles in respect of the financing of certain welfare services. Furthermore, in his speech during the Second Reading of the Bill, Mr Henry Brooke indicated that he would probably be having discussions with the representatives of local authorities to "see whether there is here an occasion for redressing the balance between grants and rates". The Government's interest in this matter is evident from the growth in current grants to local authorities in the United Kingdom (not only England and Wales) which are set out hereunder, with the corresponding rate revenues:

Table 8

	1950	1952	1954	1956	1957	1958	1959	1960
Current grants £m.	298	372	422	521	594	632	679	741
Rate revenue	337	392	460	556	615	649	714	764

Source: National Income Blue Book 1961.

The object of this comparison is not to draw attention to the apparent equivalence of the trend in the two sets of figures, but rather to emphasize the growth in the Exchequer grants which now constitute over 11 per cent. of the central government's current expenditure as compared with only 8 per cent. in the period 1950–54. It is generally acknowledged that the increase in grants is due simply to the failure of rate

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revenues to expand commensurately with local authorities' needs. The change in the grant formula in 1958 from the "percentage" type of specific grant, e.g. 25 per cent. of the cost of the fire service, to a single "general" grant which the local authorities may utilize at their discretion has not meant any reduction in the total grant aid, although the different basis for its calculation means that its distribution between local authorities has changed. This change in the form of grant is the logical precursor to a reduction in the proportion of local authority expenditure covered by grants, if and when the rating resources should be increased. If after the revaluation this policy is implemented, then clearly rates generally will increase more markedly than hitherto with successive increases in the volume of local authority spending. It may well be that the Government feels that this consideration may encourage a greater sense of economy among local authorities than many observers feel has been displayed hitherto.

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Discussion on Mr Ilersic's Paper

Dr B. Benjamin: As your Honorary Secretary, it is my normal custom to speak only when spoken to and, if possible, not to speak at all. This gives me a particular advantage, in that I am able to sit out here in front and nod my head very wisely as paper after paper is read before you, as if I understand them all but am loftily forbearing to criticize. The truth is, of course, that they are usually far too difficult for me to attempt to comment upon them.

However, tonight, they have thought it safe to let me speak. If this is a compliment to the simplicity of the paper, I am very proud to make it, because the more important papers are often not the more abstruse but the more elementary.

It has been the tradition of this Society to pass comment—scientific and informed comment—upon contemporary social and economic problems. The *Journal* of the Society is, as a result, a dynamic account not only of methodological developments within our own profession but also of the evolution of British society. The present paper falls into the latter category, and I am very glad to welcome it. It has a homely atmosphere, in more senses than one. It is about homes; it is about a problem which touches most of us in the home; and it is written in a reasonable, fireside style, enabling us all to contrive, in a nice, leisurely sort of way, to put the problem in proper perspective.

What Mr Ilersic is telling us is that the time has come to base local rates on a tidier and more realistic basis of current values for all types of property; this means that, broadly,

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householders and industry will pay more, and shops and offices (and the Crown) will pay less; but it will not be painful because, for householders, the average expenditure on rates is very small in relation to expenditure on other living essentials—and inessentials. In short, Mr Ilersic says that there is nothing to make a fuss about and, if we do make a fuss, it is because we hate the local rate.

The paper raises one or two questions. In formulating his assumptions for forecasting, the author refers to the rise of 50 per cent., in real terms, in the expenditure met by local rate between 1955–56 and 1960–61. Table 8 makes it clear that this is not due to any reduction in central government subventions, which, over the same period, have risen in roughly the same proportions. Without some information as to the elements of new construction or of other expanding expenditure, it is difficult to follow the assumption that "it is highly unlikely that this rate of growth . . . will decline". According to Local Government Financial Statistics for England and Wales for 1955–56 and for 1959–60, the greatest expansion in the four years, 52 per cent., occurred in expenditure on education and the smallest in housing where the increase appears to have been only of the order of 10 per cent. For other items, such as health, highways and police, the increase was about 40 per cent. To the extent that, at local level, the main strain of the post-war educational bulge will not be reached until 1964, the educational expansion must continue, but there might be some easing in the expansion of the other items. One would like to see a study of the prospects in greater depth.

Equally, one would want to examine the decision to extrapolate the relatively slowed but still rapid rate of new building in the period 1957–60 in order to arrive at the projected rise in total rateable value.

All this is important since, obviously, the rate payable in the pound, revaluation apart, is the ratio of two quantities which, given different rates of expansion in each, might produce quite different results.

Then, again, one wonders why, having produced plausible reasons for forecasting the conversion factors under "A" in Table 3, Mr Ilersic then decides that the set of factors under "B" is more realistic. This decision is based on the fact that the resulting increase of the domestic share of rate expenditure is as high as the Minister is alleged to think can be imposed without general, as distinct from selective, abatement. If this is true, it means that the whole process of revaluation is less factual than frankly political. This seems to invite strong words from a Society such as ours.

All of us who are suspicious of averages will be interested in the important analysis of regional differences. The author has provided a valuable guide to the probable scale of local differences in rate increases. It looks as though the case for selective abatement for areas with a low proportion of domestic property in the total rateable value is a strong one.

At the last, I am left a little disturbed. It does not entirely comfort me to know that all the fuss is over an element which forms 2·19 per cent. of consumer expenditure. The oldest trick in the game of taxation is to pretend that it cannot hurt because it is so much less than we spend on other things. Such comparisons are clearly odious. Furthermore, there is the problem of what happens to the extra load on industry: will this be passed on to the consumer?

Finally, Mr Ilersic leaves us with a threat: that, in order to encourage local thrift, government grants may be reduced and rate revenues further increased. I hope he is wrong, but I fear that he may be right.

I have much pleasure in proposing a vote of thanks to Mr Ilersic for his interesting and practical paper.

Mr P. J. O. Self: I am not proposing to follow the interesting calculations that Mr Ilersic has made of the effects of revaluation when it comes about, but I should like to take up one or two of the points that he makes in his conclusions. Here I think I would agree with the point made by Dr Benjamin, that rates may be a very small proportion of national income or personal expenditure, but that is not the end of the matter. In absolute terms, as

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Mr Ilersic has pointed out, rate revenue has well over doubled since 1950. It has gone up in real terms, much more modestly of course, as well as in monetary terms.

This raises some points which he touches on at the end of his paper, and this was the part of the paper where I was not altogether happy. Mr Ilersic points out that housing, as a whole, will have to bear an increased burden, and then he singles out owner-occupiers as if they were either the most deserving or the most hard-hit group of householders. I was not quite sure what his criterion was and it was not made clear. After all, from the point of view of the householder, the significance of rates is that they are part of his total housing costs. Any question, therefore, of housing hardship ought to be related to the total outlay of individuals, or I should say families, on housing.

Now who is in the worst position today of the various groups? I should have thought undoubtedly those people who are renting properties on the free market. I say this on the grounds that there is one large block of people who are still rent-controlled, another large block who live in subsidized local authority dwellings, and another large block who are owner-occupiers and who, at least, get certain taxation advantages out of that fact. The fourth group do not have any of those advantages and moreover obviously suffer from the narrowness of the freely rented housing market; I assert with confidence that these are the people who at present have the largest housing burden, and who will consequently suffer most, or have most cause to complain of injustice, if they get a stiff increase in rates which is related to rents which are themselves inflated in this sector by the existence of those subsidized housing sectors which in fact take up most of the housing field. That is my first point.

My second point, in a sense, follows on, because at the end Mr Ilersic says that this increase of the rateable resources may encourage the Government to reduce its grants-in-aid. It may or may not. There is no knowing what will encourage governments to do things; this is plumbing a very difficult point of political psychology. But I would suggest that there is absolutely no logical reason whatever why it should have the effect stated, and I think that this is a point which this Society might well clarify for the Government, if it is in any doubt on the matter. After all, the revaluation will not in any way reduce the total incidence of rates nor will it make rates, as a tax, any easier to pay. What it will do is to redistribute the burden of rates and, on Mr Ilersic's own figures, it will redistribute this burden in a clearly regressive way, inasmuch as householders will pay proportionately more. Thus, the effect of the revaluation will be to make rates more regressive than before; and consequently it would be harder and not easier to substitute more rate-income for government grants.

That is my conclusion, which is rather the opposite one to the point made in the paper.

I should like, very briefly, to supplement this point by saying that some confusion arises from the fact that this new revaluation will establish a uniform basis for valuations, at least in theory, and this is regarded as a tidy and logical and sensible thing to do. It is not only in this field of public administration that people get the idea that something which is tidy and uniform is also efficient and will make the whole system work better. This is a complete *non sequitur*.

Indeed, there seems to be confused thinking on this whole revaluation question. It is assumed that there is some logical good in getting uniform valuations for all types of property. I am not convinced of this and would go so far as to suggest that, if the aim is to make rates a less regressive, more elastic and less unpopular form of taxation (and that, I think, would be wholly to the good), the burden of rates should have been distributed rather differently.

I have not time to go into suggestions now, but supposing industry were to pay 100 per cent. on full rental values, and commerce were to pay a full 100 per cent.; and supposing agriculture were actually asked to pay a modest 25 or 50 per cent.; and suppose that you placed housing at something like 75 per cent. of its current value, and then looked into the possibility of supplementing rates by some such device as differential site valuation—not yet a completely dead duck—or by different valuations of property in congested areas

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(there is a link here with office congestion in London): if you were to do these things, you might make rates a more satisfactory and more elastic tax.

I do not myself follow the two particular conclusions which Mr Ilersic draws at the end of his paper. I do not think the owner-occupiers will be the worst hit group, on present intentions, nor that the effect of present government intentions can be to make the rating system more elastic.

I should like to say how helpful it is to have a paper of this sort which we can think about at the present time before the revaluation is, so to speak, on top of us.

The vote of thanks was put to the meeting and carried unanimously.

Mr R. H. A. CHISHOLM: I should like to congratulate Mr Ilersic on this most interesting paper.

I may be the only person here who has a vested interest in this subject, being the County Treasurer of Cheshire and Honorary Secretary of the County Treasurers' Society.

We are deeply indebted to Mr Ilersic for making projections from so many moving points. As he has indicated, I was personally involved in this subject and was rash enough to stick my neck out as a pioneer by preparing a less expansive document. I realize now the wisdom of the adage "Fools rush in where angels fear to tread". In self-defence, perhaps I ought to indicate that my exercise was in the form of special pleading. It was associated with what Mr Ilersic has written in Section 7 of his paper, hinting that the government Adam might be tempted to take its bite of the revaluation apple. Those of us in Local Government are particularly worried about this, because the Government might, as on previous occasions, take the opportunity to reduce the level of Exchequer assistance. So, immediately after the Bill became law, I took this action, as attack was the best form of defence.

Apart from that, the indications were (and perhaps Mr Ilersic thinks they still are) that a very heavy burden will be placed on the householder.

In my view, for the Government to exacerbate the situation by withdrawing government assistance at that particular moment would be political suicide.

I tended, therefore, in my paper to paint an unduly sombre picture of the effect on the householder, by being very conservative in my assessment of probable increases in the value of industrial and commercial properties. In fact, I allowed for no increase in the assessment of industrial hereditaments, other than the 100 per cent. increase arising from the removal of derating.

I am a member of an official Working Party and the knowledge now available to me is of a confidential nature so that I cannot say all that I should like to. My fear, and that of many in more exalted places, about the effect of revaluation on the householder might well be confounded. I dare not say more than that.

I am glad that Mr Ilersic has emphasized the fact that an average assessment for the whole country can be misleading, because it can conceal the effects of a serious impact on householders in particular areas. For example, though there is now a distinct possibility that the burden on the householder will not be as severe as many of us first thought, I think that it will be very difficult for the Minister to avoid making a derating order in respect of domestic property in the central metropolitan area, i.e. the area of the London County Council, where residential property consists of only about 30 per cent. of the whole in terms of rateable value.

Those of us directly concerned with Local Government welcome the provisions of this Act as a whole, because it has taken thirty years, as Mr Ilersic has indicated, to achieve a situation where all properties will contribute to local rates on a common basis—that of current rental value. In these circumstances, one sincerely hopes that the Minister will not find it necessary to exercise his power of defating house property in particular areas to any great extent, otherwise we shall start this untidy business all over again.

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On the other hand, I think that Local Authorities would be most unwise to use this wider basis of rateable resources to embark upon extravagant schemes.

There is a distinct danger here. I think Mr Ilersic will confirm that all those engaged in this forecasting agree that the total rateable value will increase threefold. In other words, if the present rateable value as a whole is taken at the nominal figure of £100, it will be £300 in future. The effect of that will be that the average rate in the pound for the whole country, which now is about 21s., will be reduced, under this revaluation, from the "psychologically" high figure of 21s. to about 7s. 21s. in the pound sounds horrible: 7s. in the pound does not present the same reaction.

In other words, if, when revaluation becomes effective, the local rates in any year go up 6d. in the pound, that will be equivalent to a rate increase of 1s. 6d. under the current system.

Mr J. H. Woods: I should like to follow Mr Self in disagreeing to some extent with the final conclusions of Mr Ilersic's interesting and stimulating paper.

I do not know that I follow Mr Self all the way. I agree with him that the level basis of valuation for rating will not necessarily give rise to greater resources but I do not know whether his method would, either. Ultimately, the rates will be borne by the people who consume the goods and, if they do not pay rates in one way, I suspect they will have to pay them in another. But, certainly, expenditure is going to be borne, and we just do not know the best way of bearing it. There is no evidence, in my opinion—this is where I agree with Mr Self—that having no derating at all is going to give rise to any greater reservoir of rateable resources than the present one. It may substantially reduce them or it may not.

Arising out of that point, I should like to make the point that this is basically a political decision. We are in danger of looking on the local rate as a tax whereas, as Mr Ilersic's paper shows, it is nothing of the sort. Half of local authority revenue is financed from the local rate; half is financed from the central government, and that is found, by some mysterious process which we cannot follow, from all the government taxes. I would suggest that the local rate is simply a form of purchase tax. It happens to be a purchase tax on a particular type of property and has a longer history than the other forms of purchase tax. It is simply a part of the total tax process and any decision on that is, I think, basically bound to be political.

My third point—and I apologize for raising it—is the rather odd effect of the government grant system for Local Authorities.

You have the Rate-Deficiency Grant, the idea of which is that, to the extent that the rateable value of any area is less per head than the average for the country, the Exchequer steps in and pays rates of an amount sufficient to bring the rateable value per head for the area up to the national average. In other words, if I might be excused for saying something obviously wrong, no one can fall below the average. This would have the rather odd effect that receivers of the rate-deficiency grant under the new scheme, and we do not know who they will be, if they are the subject of an Order by the Minister to derate housing, will have the loss borne by the Exchequer. The effect of this will not fall upon the other classes of ratepayers, commercial and industrial. If, however, the Authorities are non-receivers, then the burden of the increase will fall on other classes of ratepayers in the area.

What the effect of that will be, I do not know. I can only say that, at the present time, very roughly three-quarters of Local Authorities are receivers of the grant. What the ultimate effect of this decision will be, in 1963, is something upon which, again, I should not like to comment.

Mr W. F. Page: The inelasticity of rates is primarily caused by inflation. If there is no inflation, the rateable value still has the same effectiveness, but if there is continual inflation the rateable value falls behind, and so the rate poundage has to go up. Inelasticity results from the inflation, and while the inflation continues the inelasticity is there. Revaluation does not in any way modify the inelasticity; it merely brings it more

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up to date. Then it falls behind and has to be brought up to date again—there is another quinquennial valuation (but the revaluations tend to be made less frequently than every five years). So revaluation is not a cure for inelasticity.

I think the statistics show generally that uncontrolled tenants are not paying very high rents. It is only the few who are doing so, say four times gross value, but the majority are paying only about the same as Local Authority tenants, say two to about two-and-a-half times gross value.

One other point. Unlike Mr Chisholm, I am not on a Working Party, but I hear rumours, and I do not commit anybody. The rumours I hear are that the multiplier is going to be about two-and-a-half times for domestic properties, which is rather below the figure Mr Ilersic was using.

Mr Chisholm referred to the temptation to Authorities to put up the rates, when the rates are only at 7s., in order to start financing more adventurous schemes. Last time, the County District Councils said that the people who succumbed most to the temptation were the County Councils.

Mr C. T. SAUNDERS: Two very small points: first, the Schedule A valuation will doubtless be increased in line with rating valuations—which will add to the tax burden on house-owners; second, the "miscellaneous" category of hereditaments includes non-profit-making bodies, such as our own Society, which has hitherto been exempt from rates. As Treasurer of the Society, I cannot refrain from reminding you that under the new Rating and Valuation Act we shall probably be obliged to pay rates (although not for some time full rates).

Mr P. J. O. SELF: I do not quite agree with two of the points made by Mr Page.

As regards inelasticity—and this also applies to what Mr Chisholm said—we surely have to distinguish between inelasticity which is due to psychological factors and inelasticity which has some basis in the social structure. I think this is an important distinction. No doubt there is a certain inelasticity during periods of inflation, from psychological resistance to higher poundages; but this is not a real burden. Its cure lies in what we may broadly call administrative or political education—to make people see they are not really contributing more.

Similarly, when Mr Chisholm spoke of his fear of a sudden increased elasticity which might appear when valuations go up and poundages go down, it is the same thing in reverse; and I think, myself, it is a transitional thing because people will soon get wise to the fact that a sixpenny increase in rates is as much as a one-and-sixpenny one was before. I agree this is a problem in political psychology but it is one which does not cause real hardship to anyone.

The sort of inelasticity which I was talking about arises from real burdens, in other words a situation where some groups (and we have spoken mainly of householders or groups of householders) are really, through the total operation of the rating scheme, paying more than they can afford.

I am interested to hear that my remark about the free-rented sector was not borne out by the statistics, but I am still not convinced: one suspects that, owing to the process of decontrol, the average free-market or free-rented property may be qualitatively rather poor. In other words, it is probably not as good a property on average now as the average owner-occupied property; in fact, I should say it is a good deal worse, because the luxury flats and so on are balanced by a lot of properties which are pretty poor. So that point did not impress me.

I am more impressed by a more normal economic analysis, which would suggest, for instance, that in London rents are artificially high because of the concessions made to other groups of housing property. And this is a link with rates, because it must follow that any rating valuations based upon free rents will lay a further burden upon that particular group of people.

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Mr ILERSIC replied briefly at the meeting and subsequently in writing as follows:

Dr Benjamin is quite justified in his comments on my efforts to extrapolate the future level of rate-financed expenditure, and new building as a source of rateable value. I agree that there may be changes in the near future to falsify my figures. For example, it is possible that the "bulge" in the school and university populations in the mid-1960's could compel the Government to revise the basis of the education grant to local authorities, if not the entire structure of education finance, during the next decade. On the other hand, few assumptions are so well grounded and supported by history as the assumption that government expenditure will rise and continue to rise indefinitely. (See, for example, Peacock and Wiseman, *The Growth of Public Expenditure in the U.K.*, O.U.P., 1962.) I do not accept his point that I am unjustified in asserting that the rate burden is not excessive when compared with other taxes, or with other forms of consumer outlay. I think this is a highly relevant consideration, not least because for political reasons the householder *qua* ratepayer has been shielded from the full effects of the post-war increase in local government outlays. It does no harm to remind him that it is because of this subsidy that he can at present afford to increase his consumption of other goods and services, many of them more heavily taxed, and yielding a lower return, than local authority services.

In so far as Mr Self is criticizing inferences and conclusions drawn from "average" or global figures, I accept his criticism. There are many exceptions to the rule or "average". Some householders and tenants undoubtedly pay a disproportionately large part of their net incomes in rent and rates, especially in London and other major conurbations. As the housing situation eases—and I draw a different picture from the official figures given in the White Paper *Rent Act* 1957, *Report of Inquiry* (Cmnd. 1246) than does Mr Self—this particular problem is probably of short duration. I included the data relating to owner-occupiers not so much because I believe that they constitute "hard cases", as because I believe that the Government will so regard them. It is apparent from the figures given in Table 7 that many such persons are buying their houses on modest incomes and the addition of a substantial increase in rates would severely strain their budgets. Unlike the tenant of rented accommodation, they cannot simply change their accommodation with changes in their financial position. The report of the Fitzgerald Committee and the Government's reaction to their findings in 1939 suggest that governments are very conscious of the ratepayers' reactions to sharp increases in the rate charge. The proposed abandonment of the Schedule A tax for owner-occupiers in the recent Budget address reinforces this opinion. In my opinion, the only group of domestic ratepayers deserving of preferential treatment in the matter of rates is the aged and retired. They have in many cases lost the protection of the Rent Acts and their pensions and savings have been eroded by inflation as local authority demands have risen. The obvious solution is to halt the inflation. A more realistic course would be for the National Assistance Board to take rate changes into account, as they do with rents, when fixing the level of assistance.

I am indebted to Mr Chisholm for his observations and pioneer work in this subject. I fully share his fears concerning the future trend of rate poundages in consequence of rising expenditure. The last quinquennium has shown that the gain in reduced rate poundages is rapidly wiped out by the upsurge in total expenditure. In this context I can only repeat my observations on Dr Benjamin's remarks.

Since this paper was read, the Minister of Housing and Local Government has presented a White Paper (Cmnd. 1663) to Parliament which reveals that, contrary to all expectations, householders as a class in England and Wales will contribute about the same proportion of rate revenues as in the past quinquennium. According to the White Paper, entitled *Revaluation for Rates* 1963, the increase in domestic values since 1934 is fourfold, and when allowance is made for the changes in the 1956 valuation when they were revised on to a 1939 basis (an increase of about 40 per cent.), the effective increase since 1939 for domestic property, after allowance for increased statutable deductions

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representing repairs allowance, is about 166 per cent. This contrasts with an increase, after loss of 50 per cent. derating, of 144 per cent. in industrial values, and of about 130 per cent. in commercial property values after allowing for the termination of the 20 per cent. relief. The effective "conversion factors" upon which the White Paper results seem to have been based (they are not given, but can be estimated approximately from the information disclosed) have the effect of greatly increasing the relative contribution of industrial properties to total rateable value, i.e. by 47·5 per cent. In brief, the revaluation, instead of increasing the burden on the domestic ratepayer, has left the latter in the same relative position; has considerably worsened that of industry and eased that of commercial property owners by about 10 per cent.

Commenting on the results disclosed in the White Paper, *The Times* (March 6th) remarked that nobody would be more surprised by the outcome of the valuation than Mr Henry Brooke, the Minister in charge of the Bill which became the Rating and Valuation Act 1961. Given this unexpected relief, the domestic ratepayer has been less concerned with the overall effect of the revaluation than with the regional variation which, as indicated in the above paper, is likely to be considerable. But, whereas in my paper I anticipated that in predominantly industrial areas the domestic ratepayer would fare worse than in the residential areas, the marked increase in industrial valuations has reversed this forecast. Instead, the most substantial increases, up to 25 per cent., in the domestic ratepayers' contribution will occur in the predominantly residential areas, e.g. Eastbourne, Bournemouth, Blackpool. In contrast, in highly industrialized areas such as West Ham and Halifax, the share of domestic ratepayers as a class falls by about one quarter. Some part of this result is due to the increase in the Rate Deficiency Grant of some £23 m., but no information about the distribution of the grant, or of the probable effects of the revaluation on individual properties, is available.

The White Paper estimates, however, have led to some forty M.P.s advocating a new enquiry into the structure of local government finance and, more especially, the role of the rate as a revenue-raising instrument. The frankly political concession in the 1962 Budget address, whereby only owner-occupiers will be relieved of the Schedule A income tax, suggests that the last has not yet been heard of the 1963 revaluation.

As a result of the ballot taken during the meeting, the candidates named below were elected Fellows of the Society:

ADELSON, Ronald Maurice, B.A.
AZHAR, Sahib Khan Aleem, M.A.(Econ.)
BADHAM, Harold, B.Sc.
BAILLIE, David Harold, B.Sc.
CONSTABLE, Patrick David Lindsay, M.A., Dip. Stats.
DUNSTAN, Ernest Gordon, B.Sc., B.Ed., Ph.D.
FORD, John Raymond
HARSANT, John David Uden, M.A.
HART, Peter Edward, B.Sc.(Econ.)
HUMPHREY, Geoffrey Theodore, B.A.
INSUA, Alvaro de
LAMPKIN, William, B.Sc.
NEUSS, Ronald Frederick, D.F.C.
PARIKH, Ashok, M.Com.
PETRIE, Ann, B.A., Dip. Stats.
RADHAKRISHNA, Sistla, M.A.
RAGHAVAN, Krishnaswamy, M.A.
SMITH, Ronald Leverington, B.Sc., Dip. Ed.
WILLIAMS, Raymond John

Corporate Representative

HARRISON, Kenneth Cecil, M.B.E., *representing* City of Westminster Public Libraries.

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The Structure of English Milk Distribution

By E. Strauss
Milk Marketing Board

1. Introduction and Summary

In a study of the structure of the milk industry in England and Wales (Strauss, 1960), an attempt was made to define the sectoral composition of the dairy industry in terms of milk throughput and to supply quantitative data for a single month. This made it possible to describe in some detail the shares in milk supplies of different sectors and to analyse liquid milk sales to final consumers, but without providing quantitative estimates of the flow of milk through different distributive channels. The present paper endeavours to fill this gap, as far as this can be done by the analysis of operations during a single month, and to discuss on this basis the nature of wholesaling operations in the milk distributive trade.†

It includes calculations of the relative quantities of milk passing through different distributive channels and uses a "flow coefficient" representing the average number of movements as a measure of the complexity of distribution.

The wholesale trade in milk is analysed in terms of "primary" and "secondary" wholesaling: "primary" wholesaling consists of the assembly and bulk despatch of milk for subsequent processing; "secondary" wholesaling involves processing and packaging for resale to other distributors. In June 1959, each type of wholesaling accounted for a roughly similar part of the excess of the flow coefficient over the practicable minimum.

Specific applications of these concepts are attempted on a geographical basis. The flow coefficient of Greater London exceeds that of the country as a whole because of the more than average need for primary wholesaling. Amongst four other large towns selected for analysis, Birmingham has a specially low flow coefficient owing to little primary and secondary wholesaling (ignoring shop sales), while Liverpool has a relatively high flow coefficient due to a large proportion of secondary wholesaling.

Country depots are numerically evenly divided between small, medium and large establishments, with considerable technical concentration despite geographical dispersion. About 30 per cent. are specialized primary (and/or secondary) wholesalers, with the proportion of intake used for liquid disposals rising at first with the size of the depot but declining sharply for large and very large units.

Secondary wholesaling is concentrated in medium-sized establishments (from 1,500 to 19,999 gallons daily sales) and much less important for smaller and larger units. This may correspond to a geographical concentration of secondary wholesaling in the smaller and medium-sized towns rather than in semi-rural areas and large conurbations.

A rough estimate of the size distribution of consumer sales allocates 37 per cent. to small distributors (below 1,000 gallons per day), 23½ per cent. to medium establishments (1,000–9,999 gallons per day) and 39½ per cent. to large establishments (10,000 gallons per day and over).

† The calculations were carried out in the Milk Marketing Board's Marketing Economics Department by Mr S. M. Tennant and Mr M. H. Dixon.

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A study of the vertical integration within the industry shows a close relationship between size (in terms of intake from producers or of ultimate utilization) and the adequacy of farm supplies, with over half even of the largest organizations short of their full requirements of farm milk in a month of seasonally high production such as June.

2. The Flow of Milk to the Liquid Market

The flow of milk to the consumer and the size distribution of distributive firms is a subject of intrinsic interest, particularly in relation to the problem of wholesaling, but does not lend itself to simple short-cuts and involves the analysis of well over a thousand individual claim forms. The month of June 1959 has therefore been chosen, because a broad framework is already in existence for this month in the form of a sectoral milk balance showing global movements of milk from farms through depots, town processors, and non-processing dairymen to consumers (and manufacturers).

In calculating the flow of milk through specific distributive channels, certain assumptions had to be made, because it is as a rule impossible to distinguish the actual utilization of milk received at dairy plants from different sources. Thus it was assumed that farm supplies received by "*direct*" *processors* have been used in the first place for retail sales (including those of retail dairies belonging to the same firm); if any "surplus milk" was thrown up by processors, this was allocated to ex-farm supplies immediately after the satisfaction of retail requirements. The following stages in the order of priority of allocation were wholesale sales, wastage on liquid sales and manufacture, using in every case farm supplies before receipts from the "direct" market (whether from other processors or from non-processors) and direct market receipts before depot supplies. For *depots*, similar principles were employed, but the much greater proportion of farm supplies generally simplified the procedure. The net effect of slightly artificial assumptions is probably insignificant, provided they are consistently applied.

As far as possible, the *physical* movements of milk on its way to the market were reconstructed. To a certain extent, this again involved certain arbitrary distinctions. The analysis was made in terms of "stages" between farm and consumer, i.e. the number of times the same milk had to be handled by intermediaries—the greater this number, the greater the variety of ways through which milk reaches the consumer.

The restriction of this study to one month, however essential for practical purposes, limits the validity of the numerical values presented in the tables. June happens to be a month of high production. Hence the quantity of farm supplies available to processors is high and their need of depot supplies correspondingly small. In a month of low supplies, such as September, the proportion of depot sales to processors would have been higher and that of processors' farm supplies considerably lower.

3. The Flow Coefficient

Total liquid sales during June 1959 amounted to 113·1 million gallons. The estimated physical movements required for getting this gallonage to the consumer involved just under 300 million gallons or 2·63 moves per gallon. This ratio between total quantity moved and ultimate sales will be called the "flow coefficient" and used as a simple measure of the complexity of the flow of milk to the liquid market. In modern conditions, the volume of producer-retailing is so small that the practicable minimum flow coefficient in the most favourable case is 2, i.e. the supply of farm milk to the processor for direct sale to the consumer. Put in this way, the efficiency

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of marketing may be tested by the extent to which the excess of the actual flow coefficient over 2 is necessary. This technique has, therefore, definite practical uses in its specific field. Perhaps of more general interest, however, is the study of the part played by wholesaling activities in the movement of milk.

Table 1 shows the physical flow of milk from farms to consumers.

TABLE 1

The physical flow of milk to consumers—June 1959 (*in percentage of total liquid sales*)

Farm to consumer via:	%	*Number of stages*	*Cumulative flow coefficient*
Producer retailer	3·8	1	1·0
(i) Depot	0·7	2	
Processor	41·6	2	
(ii) Non-processor	1·1	2	
	43·4	2	1·92
(i) Depot–Processor	18·3	3	
"Direct"–Processor	2·1	3	
(ii) Depot–Non-processor	6·3	3	
Processor–Non-processor	13·1	3	
	39·8	3	2·41
(i) Depot–Depot–Processor	0·5	4	
Depot–"Direct"–Processor	2·3	4	
"Direct"–Depot–Processor	0·2	4	
"Direct"–"Direct"–Processor	0·1	4	
(ii) Depot–Depot–Non-processor	0·6	4	
"Direct"–Depot–Non-processor	0·1	4	
Depot–Processor–Non-processor	6·9	4	
Depot–Non-processor–Non-processor	0·3	4	
"Direct"–Processor–Non-processor	0·7	4	
	11·7	4	2·60
(i) Depot–"Direct"–"Direct"–Processor	0·1	5	
(ii) Depot–Depot–Processor–Non-processor	0·2	5	
Depot–"Direct"–Processor–Non-processor	0·9	5	
"Direct"–Depot–Processor–Non-processor	0·1	5	
	1·3	5	2·63
Total	100·0	—	2·63
Absolute total (m.g.)	113·1	—	—

4. "DEPOTS" AND "WHOLESALERS"

In the technical language of the dairy industry there is a sharp distinction between "depots"—with few exceptions styled "main supply depots"—and "wholesalers", which is of considerable importance in connection with the operations of the price structure. Using broader economic terms, these categories both form part of the

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concept of wholesaling; nevertheless, there is a functional difference between their typical operations which justifies their description as "primary" and "secondary" wholesaling (Stacey and Wilson, 1958). The characteristic depot function is "primary" wholesaling in the sense of collecting or assembling milk for bulk supply to processors in consumption areas with insufficient ex-farm supplies for their requirements. Some depots also engage in "secondary" wholesaling which consists largely of the sale of packaged milk to non-processors, though there is also a small trade of "special" milks (e.g. farm-bottled or Channel Islands milk) to other processors.

By far the largest part of secondary wholesaling involves the wholesaler in the productive process of heat-treating and packaging milk and is, therefore, a borderline case from the point of view of economic classification. "Packaging is included as a distributive function as, in practically all European countries, the distributors as well as producers undertake packaging. However, in an economy where packaging is normally performed only by producers it is more logical to consider it a production function" (O.E.E.C., 1954, p. 13n.).

"Primary" wholesaling covers almost all transactions between depots and *processors*, with or without intermediate links, though there is a minor element of secondary wholesaling. If all the movements from depots to processors in Table 1 are combined they account for about 29·5 per cent. of total liquid sales. Supplies reaching processors from other "direct" markets without touching depots were about 2·9 per cent. of total liquid sales. Thus, processors receive 32·4 per cent. of all liquid sales after some kind of wholesaling transaction. Of this, primary wholesaling enters into 29·4 per cent. of all liquid sales and 3·0 per cent. is accounted for by secondary wholesaling.

"Secondary" wholesaling consists mainly of transactions between depots and/or processors and *non-processors*. Secondary wholesaling involving non-processors enters into 29·2 per cent. of liquid sales and secondary wholesaling involving processors accounts for a further 3·0 per cent., a total of 32·2 per cent. In at least 9·8 per cent. of all liquid sales both primary and secondary wholesaling occurred. Broadly speaking, each of these forms of wholesaling was equally responsible for the excess of the flow coefficient of 2·63 over the basic minimum of 1·92.

Though far from simple in practice, it is theoretically possible to test the need for primary wholesaling by definite technical standards. On the other hand, purely technical considerations will rarely permit definite conclusions about the economic efficiency of secondary wholesaling. Heat treatment and bottling are capital-intensive processes and there must be a point at which the high cost of small-scale processing outweighs any benefits from proximity on the distribution side. This point is almost certainly as high as 500 gallons per day and in certain circumstances may indeed be higher. Where conditions such as low population density call for small distributive units, there is obviously a case for the small non-processing dairyman. Distribution on a very large scale involves certain overheads which a small distributor does not incur. For this reason it may be that in favourable circumstances the specialization of function between processors and non-processors does not increase costs and perhaps even reduces them.

5. The Flow of Milk to Selected Large Towns

As an illustration of the importance of location for the flow of milk, detailed calculations of the physical flow of milk through establishments situated in Greater London, Birmingham, Liverpool, Manchester and Bristol have been carried out.

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(These figures relate to sales by dairymen and not to consumption within each area, though the differences may not be very important.) In Table 2 they are compared with the position for the rest of England and Wales as well as with that for the country as a whole excluding the Greater London Area.

The table highlights the effect of the special needs of London on the national pattern. The over-all flow coefficient for England and Wales is 2·63, that for Greater London 3·11 and for the whole country excluding Greater London 2·50, which is the

TABLE 2

The physical flow of milk to selected towns—June 1959
(*in percentage of total liquid sales*)

No. of stages	*Greater London*	*Birmingham*	*Liverpool*	*Manchester*	*Bristol*	*England and Wales less*	
						Greater London	*Selected 5 towns*
(i) *By number of stages*							
1 . . .	—	0·1	0·8	1·5	1·3	4·9	5·6
2 . . .	13·8	79·2	63·0	75·4	66·5	52·0	48·4
3 . . .	64·3	18·2	20·7	19·9	27·9	32·6	34·7
4 . . .	18·8	2·5	15·1	3·1	4·2	9·7	10·4
5 and over .	3·1	—	0·4	0·1	0·1	0·8	0·9
Flow coefficient	3·11	2·15	2·50	2·25	2·35	2·50	2·53
(ii) *By type of distributor*							
Producer retailers	—	0·1	0·8	1·5	1·3	4·9	5·6
Processors .	86·6	94·4	66·4	77·8	76·6	59·9	56·2
Non-processors	13·4	5·5	32·8	20·7	22·1	35·2	38·2
Absolute totals (m.g.)	25·4	4·6	2·8	3·9	1·7	87·7	74·7

Note: Producer retailers' sales of processed milk are included amongst "processors".

same as for Liverpool, but rather higher than for Birmingham, Manchester and Bristol. The exclusion of these towns would, however, raise the flow coefficient for the rest of the country only to 2·53.

The crucial difference between London and the rest of the country is in the need for depot supplies on a very large scale, even in a month of high milk production such as June. No less than 82·5 per cent. of London's milk had to pass through depots, generally in the form of primary wholesaling; secondary wholesaling plays a very small part in the London area, where only 13·4 per cent. of liquid milk sales pass through the hands of non-processors compared with 35·2 per cent. in the rest of the country and no less than 38·2 per cent. in England and Wales excluding the five towns in question. Retailing by large processors is thus most prevalent in the largest population centres, with Birmingham in the van, though the proportion of processors' direct retail sales is inflated in this case by the inclusion of sales of sterilized milk through shops which cannot be satisfactorily separated from direct retail sales.

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To a much smaller extent, this applies to the other four towns as well, all of which—and particularly London—show a much higher proportion of processors' retail sales than the rest of the country, where it is only 56·2 per cent.; although the area outside the selected five towns has a much higher proportion of sales by producer retailers (5·6 per cent.), its flow coefficient is thus higher than that of any of these towns except London.

6. Size Structure and Liquid Sales of Country Depots

The arbitrary element in the definition of main supply depots causes difficulties in their analysis, for apart from their primary function of providing bulk supplies for the liquid market and manufacturing the surplus, a number of depots engage in secondary wholesaling of processed (pasteurized and sterilized) milk and even in some retail trade.

The relative importance of these activities for depots of different size (i.e. intake) is shown in Table 3.

TABLE 3

Country depots—proportion of intake and liquid milk sales in depots of different size —June 1959

Average daily intake (g.)	*Number (in % of total)*	*Intake (in % of total depot intake)*	*Liquid sales in % of*			
			Intake			*All depot sales*
			Bulk	*Others*	*Total*	
Under 1,000	4·4	0·2	2·4	0·2	2·6	—
1,000– 2,499	7·9	0·9	29·4	0·1	29·5	0·5
2,500– 4,999	6·4	1·3	18·5	11·5	30·0	0·7
5,000– 9,999	15·8	7·1	41·9	18·7	60·6	8·4
10,000–19,999	33·0	26·8	51·8	19·5	71·3	37·2
20,000–49,999	29·5	52·7	38·7	5·4	44·1	45·2
50,000 & over	3·0	11·0	36·8	0·7	37·5	8·0
Total . .	100·0	100·0	41·8	9·6	51·4	100·0
Absolute totals .	203 (units)	103·0 (m.g.)	43·1 (m.g.)	9·9 (m.g.)	53·0 (m.g.)	—

The most important feature is the fairly pronounced technical concentration of this part of the industry. Numerically, the 203 depots fall into three almost equal groups. 34·5 per cent. of all depots are "small" with an average daily June intake of less than 10,000 gallons; 33 per cent. are "medium" (10,000–19,999 gallons) and 32·5 per cent. are "large" with an intake of 20,000 gallons or over, including 3·0 per cent. "very large" depots with an intake of 50,000 gallons or over.

Small depots are responsible for 9·5 per cent. of the combined depot intake, medium depots for 26·8 per cent. and large depots for 63·7 per cent. The share of liquid milk sales (i.e. mostly primary and secondary wholesaling activities of depots, for their direct consumer sales are very small) rises steadily with size up to the medium depots and declines sharply and progressively for the larger units. Sales of heat-treated and bottled milk are insignificant in the smaller and the larger establishments, but

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absorb a substantial proportion of intake in depots with an average daily intake of 5,000–20,000 gallons.

At the extremes there are 28 depots without any liquid sales and 66 engaged entirely in supplying the liquid market. All but 3 of the former are small depots (under 10,000 gallons per day), while no fewer than 46 of the latter are medium and large depots. Almost half the medium-sized depots of 10,000–19,999 gallons per day intake are specialized suppliers of liquid milk, a higher proportion than amongst small or large depots.

TABLE 4

Country depots—proportion of intake and liquid milk sales by organizations of different size—June 1959

Average daily intake (g.)	*Numbers (in % of total)*	*Establishments (in % of total)*	*Intake (in % of total)*	*Liquid sales (in % of total)*	*Liquid sales (in % of intake)*
Under 1,000	13·7	3·4	0·1	—	3·6
1,000– 2,499	15·7	4·0	0·4	0·1	11·7
2,500– 4,999	19·6	5·9	1·0	0·2	9·8
5,000– 9,999	11·8	3·4	1·3	1·0	38·2
10,000– 19,999	15·7	4·4	3·0	3·8	67·8
20,000– 49,999	7·9	4·0	4·5	3·6	41·4
50,000– 99,999	3·9	3·0	3·6	1·1	15·7
100,000–199,999	3·9	6·9	10·2	4·3	21·5
200,000 & over	7·8	65·0	75·9	85·9	58·2
Total	100·0	100·0	100·0	100·0	51·4
Absolute totals	51	203	103·0 (m.g.)	53·0 (m.g.)	—

The technical concentration of country depots is limited by the facts of geography and the pattern of milk production. It is, therefore, much less complete than the economic concentration of ownership. The 203 depots belonged to 51 organizations, counting the cooperative movement as one. Their intake and sales structure are shown in Table 4.

7. SECONDARY WHOLESALING AND RETAILING

While primary wholesaling is almost entirely in the hands of country depots, secondary wholesaling in the sense of the supply of processed and packaged milk (including "special" milk such as T.T. farm-bottled or sterilized milk) to distributors may be carried out by depots or by processing dairymen whose trade very frequently also includes direct sales to consumers. Table 5 distinguishes between size groups of all processors selling only by retail, of those selling partly by retail and partly by wholesale and of specialized wholesalers.

Only amongst units with sales below 500 gallons per day is the pure retailer predominant; in all other size groups mixed establishments form by far the most important group, both in numbers and trading volume, and their share rises with the size of throughput up to about 10,000 gallons per day and thereafter tends to decline slightly. Pure wholesalers are numerically unimportant and their share in throughput is even

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smaller than their share in sales; their main strength, such as it is, lies in the size groups from 2,500 to 9,999 gallons daily sales.

TABLE 5

Retailing and secondary wholesaling establishments—June 1959

Average daily sales of processed and special milk (g.)	*Numbers (in % of total)*				*Throughput (in % of total)*			
	Retailers	*Mixed*	*Whole-salers*	*Total*	*Retailers*	*Mixed*	*Whole-salers*	*Total*
Under 500 .	22·0	4·5	—	26·5	1·1	0·4	—	1·5
500– 999 .	6·8	8·5	—	15·3	1·2	1·4	—	2·6
Under 1,000 .	28·8	13·0	—	41·8	2·3	1·8	—	4·1
1,000– 1,499 .	2·6	6·1	0·2	8·9	0·7	1·8	0·1	2·6
1,500– 2,499 .	2·1	8·0	1·0	11·1	0·9	3·8	0·5	5·2
2,500– 4,999 .	2·2	12·9	1·0	16·1	1·8	11·0	0·9	13·7
5,000– 9,999 .	1·2	10·5	0·3	12·0	2·0	18·1	0·7	20·8
1,000– 9,999 .	8·1	37·5	2·5	48·1	5·4	34·7	2·2	42·3
10,000–19,999 .	1·2	5·3	0·1	6·6	4·0	17·5	0·3	21·8
20,000–49,999 .	0·8	2·1	—	2·9	5·0	14·0	—	19·0
10,000–49,999 .	2·0	7·4	0·1	9·5	9·0	31·5	0·3	40·8
50,000 and over	0·2	0·4	—	0·6	2·9	9·9	—	12·8
Total . .	39·1	58·3	2·6	100·0	19·6	77·9	2·5	100·0
Absolute totals	347	518	24	889	21·8	86·6	2·7	111·1

Note: Differences between this table and Table 11 in Strauss (1960) are due to rounding and, in the case of total numbers, to the use of final rather than provisional figures.

The complex relationships of the preceding table express a noticeable difference in size structure between processors' direct sales to consumers and their secondary wholesale sales (including branch transfers) given in Table 6.

The percentage of secondary wholesaling in terms of total sales of processed and special milk may be called the "wholesaling ratio"; it rises from 9·1 per cent. for the smallest establishments to a maximum of 45·5 per cent. for the size group 2,500–4,999 gallons per day, is maintained on a slightly lower level for the next size group of 5,000–9,999 gallons and then falls sharply to only 8·9 per cent. for the largest establishments.

As there is a close relationship between population density and size of processing unit, these figures suggest that secondary wholesaling is most strongly entrenched in the smaller and medium-sized towns. This conclusion is in line with the proportion of non-processors' sales in London and some other selected towns compared with the rest of the country (see Table 2).

No less than 59·3 per cent. of all direct consumer sales by processing establishments were made by large units with daily sales of 10,000 gallons or more and only 5 per cent. by small units below 1,000 gallons. As these establishments were responsible

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for almost exactly two-thirds of all consumer sales, this means that at least 39·5 per cent. of all consumer sales were made by large establishments. The available information makes it impossible to be more specific than this, for not enough is known about the size distribution of non-processing retailers and producer retailers to provide a complete analysis of consumer sales by size of distributive unit. The bulk of them are obviously small or very small, but it is impossible to combine the existing data, because they overlap to an unknown extent.

TABLE 6

Sales to consumers and secondary wholesale sales—June 1959
(in percentage of total)

Average daily sales of processed and special milk (g.)	*Direct consumer sales*	*Secondary wholesale sales*	*Wholesaling ratio*
Under 500	2·0	0·4	9·1
500– 999	3·0	1·8	22·4
Under 1,000	5·0	2·2	17·7
1,000– 1,499	2·8	2·3	28·5
1,500– 2,499	4·6	6·8	42·0
2,500– 4,999	11·1	19·0	45·5
5,000– 9,999	17·2	28·1	44·5
1,000– 9,999	35·7	56·2	43·5
10,000–19,999	21·4	22·6	34·0
20,000–49,999	20·6	15·5	26·9
10,000–49,999	42·0	38·1	30·7
50,000 and over	17·3	3·5	8·9
Total	100·0	100·0	32·8
Absolute totals (m.g.)†	74·6	36·5	—

† Including about 3 per cent. duplication—see Strauss (1960, p. 156).

If it is assumed that *all* non-processing distributors, including the overwhelming numerical majority of all producer retailers, are "small", i.e. distribute less than 1,000 gallons per day to final consumers, and ignoring shop sales of milk, the following rough-and-ready division may be obtained.

Small distributors (perhaps 8,000 sub-purchasers overlapping with about 13,700 producer retailers and 372 processing establishments) sold about 37 per cent. of liquid milk, *medium distributors* (428 processing establishments) sold about 23½ per cent. of liquid milk and *large distributors* (89 processing establishments) sold about 39½ per cent. of liquid milk.

8. VERTICAL INTEGRATION

To round off the structural analysis of the milk industry as a whole, calculations were made of the extent to which organizations of different size cover the whole range

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of handling milk from the farm to its final form, the processing and packaging of liquid milk and the manufacture of milk products. The figures necessarily exclude non-processing dairymen, but as their total receipts of milk from farms amounted to only 1·4 million gallons out of 161·6 million gallons sold by wholesale producers, this is not a very serious limitation. (Producer retailers' levied gallons have only been included where they formed part of the operations of processing establishments, as it would have been difficult to exclude them; the quantitative importance of this item is even smaller than that of non-processors' farm supplies.)

TABLE 7

Ex-farm supplies and final utilization (excluding primary wholesaling)—June 1959. By size groups of organization in percentage of numbers in each size group

Average daily ex-farm intake (g.)	*Ex-farm supplies in % of final utilization*							*Organizations in group*	
	Nil	*Under* 10	10–24·9	25–49·9	50–74·9	75–99·9	100 *and over*	%	*Number*
Under 500	8·7	1·1	3·0	6·8	13·9	24·4	42·1	100·0	266
500– 999	—	—	1·0	2·8	12·3	39·6	44·3	100·0	106
Under 1,000	6·2	0·8	2·4	5·6	13·4	28·8	42·8	100·0	372
1,000– 1,499	—	—	1·6	6·2	17·2	35·9	39·1	100·0	64
1,500– 2,499	—	—	—	7·5	5·7	52·8	34·0	100·0	53
2,500– 4,999	—	—	—	3·8	5·7	62·2	28·3	100·0	53
5,000– 9,999	—	—	—	—	16·1	51·6	32·3	100·0	31
1,000– 9,999	—	—	0·5	5·0	10·9	49·8	33·8	100·0	201
10,000–19,999	—	—	—	—	6·6	46·7	46·7	100·0	15
20,000–49,999	—	—	—	—	—	44·4	55·6	100·0	9
10,000–49,999	—	—	—	—	4·2	45·8	50·0	100·0	24
50,000 & over	—	—	—	—	—	55·6	44·4	100·0	9
Total	3·8	0·5	1·7	5·1	12·0	36·8	40·1	100·0	—
Numbers in group	23	3	10	31	73	223	243	—	606

The shape of the table indicates its main feature—the close relation between the size of organization (measured in terms of dairy ex-farm intake) and the adequacy of its ex-farm intake for its retailing, secondary wholesaling and manufacturing activities. 15 per cent. of small organizations (below 1,000 gallons per day farm intake) cover less than half their ultimate requirements from this source, with another 13·4 per cent. covering less than 75 per cent.; for medium-sized organizations these proportions are only 5·5 per cent. and 10·9 per cent., while large organizations invariably have 50 per cent. or more ex-farm supplies, and organizations with 50,000 gallons per day and over have at least 75 per cent. or more. A similar pattern persists

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if volume of final utilization rather than numbers is examined, but there are important differences in detail.

Corresponding calculations based on size groups of final utilization rather than on those of intake ex-farms produced very similar results and are, therefore, not worth quoting. This similarity indicates that, generally speaking, size of ex-farm intake is an adequate standard for measuring the relative size of organizations in the dairy industry. Whichever way the figures are looked at, they indicate that the smaller organizations are substantially less self-sufficient in their supplies of milk from farms than the larger firms; vertical integration is thus largely a function of size.

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The Megalithic Unit of Length

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Summary

A large number of megalithic circle diameters is now available. The analysis shows that the unit suggested for the radius (2·72 ft) can be accepted without hesitation. It also shows that there is a marked increase in the accuracy with increasing circle diameter. The distances between stones in rows and alignments are also examined. Here again the same unit appears but it seems that in a significant number of cases the half unit was also used.

1. Introduction

In an earlier paper (Thom, 1955) it was shown that in a statistically significant number of cases the diameters of stone circles were multiples of 5·44 ft. This might be called the megalithic fathom. The analysis was based on surveys of 52 circles and in the appendix supporting evidence from other sources was given. Since then many more surveys have been made. These include sites where there are two or sometimes three circles close together and from the surveys it is possible to obtain the distances between the circle centres. Thus, including these distances, there are now available 156 measurements. It is not proposed to include distances to outliers from the circle centres even although these distances are in a number of cases multiples of the fathom. The reason for excluding these data is that numerous ambiguous cases arise; for example, where an outlier is near two circles it may belong to either; also there are many stones which may or may not be true outliers. The diameter of a circle is unambiguous if at times uncertain in value and the same applies to the distance between circle centres.

Table 1

Circles in England and Wales

Site	*Lat.* ° ′	*Long.* ° ′	*D. (ft)*	*L. (ft)*	*Remarks*
D1/3 Nine Ladies	53 11	1 38	35·5		
D1/7 Barbrook	53 17	1 35	47·7		Type B
D1/8 Owler Bar	53 17	1 34	86·6		Type A
D1/9 Moscar Moor	53 23	1 41	54·2		Type A
D2/1 Mitchell's Fold	52 35	3 11	93·3		Type A
D2/2 Black Marsh	52 36	3 0	76·0		Type A
L1/1 Castle Rigg	54 36	3 6	107·8		Type A
					Resurveyed
L1/4 Burnmoor	54 25	3 17	70·0 ± 2 (omit)	122·5	
			49·7	150·7	Resurveyed
			54·6	339·3	
			52·0	419·0	
			104·5 ± 2 (omit)		Type A
L1/10 Seascale	54 24	3 29	88·9		Type D

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TABLE 1 (*continued*)

Circles in England and Wales

Site	*Lat.* ° ′	*Long.* ° ′	*D.* (*ft*)	*L.* (*ft*)	*Remarks*
L1/13 Lacre (S)	54 13	3 18	49·7		
L5/1 Birkrigg Common	54 9	3 5	87·0 ± 2 (omit)		
			27·7		
S1/1 The Hurlers	50 31	4 27	107·6	204·0	
			136·8	215·9	
			113·7	419·1	
S1/2 Nine Stones	50 34	4 29	49·6		
S1/3 Duloo	50 24	4 29	38·6		Type A
S1/5 Treswigger	50 33	4 39	108·3		
S1/6 Leaze	50 34	4 38	81·5		
S1/7 Rough Tor	50 35	4 37	150·7		Type D
S1/8 Dinnever Hill	50 35	4 38	139·7		Type A
S1/10 Nine Maidens (Cambourne)	50 11	5 15	53·6		? Type A
S1/11 Nine Maidens (Ding Dong)	50 10	5 36	71·6		
S1/13 Boscawen-un	50 5	5 37	82·6		Type B
S1/14 Merry Maidens	50 4	5 35	77·8		
S1/16 Botallack	50 8	5 39	71·6		Type A
S2/1 Grey Wethers	50 38	3 56			
South Circle			108·5	128·3	
North Circle			104·5		
S5/2 The Sanctuary	51 25	1 50	129·7		
			64·8		
			46·8		
			34·3		
W6/1 Kerry Pole	52 28	3 14	86·9		Oval shaped
W8/3 Four Stones	52 14	3 6	17·2		
W9/2 Gors-fawr	51 56	4 43	73·2		
W9/4 Castell-Garw	51 54	4 42	43·7		
W11/2 Trecastle	51 58	3 42			
South-west Circle			43·7 }		Concentric
			24·3 }		
North-east Circle			76·3	144·2	
W11/3 Maen Mawr	51 52	3 40	59·8		
			? × 62·5		Egg shaped
W11/4 Usk River	51 55	3 43			
West Circle			65·0	365·8	
East Circle			68·2		
W13/1 Gray Hill	51 38	2 49	32·6		

Tables 1 and 2 give a list of these new sites with the lengths found at each, tabulated as *D* (diameter) and *L* (distance between centres). A few of the earlier surveys used in the 1955 paper had been made in unsatisfactory conditions of weather and lighting. This applies especially to the circles on Burnmoor (L1/4) where on a subsequent new survey extra stones were found by bayonet prodding. New values are also given in Tables 1 and 2 for the Loupin Stanes (G7/4), The Seven Brethren (G7/2) and Castle Rigg (L1/1) all of which were resurveyed. Otherwise the values in the earlier paper are to be taken with those now given with the proviso that all diameters are now omitted where the uncertainty was considered to be greater than ± 1·5 ft.

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TABLE 2

Circles in Scotland

Site	*Lat.* ° ′	*Long.* ° ′	*D. (ft)*	*L. (ft)*	*Remarks*
B1/5 Upper Auchnagorth .	57 36	2 16	45·0		
B1/6 Easter Aquorthies, Manar . . .	57 17	2 27	64·0		
B1/8 Sheldon of Bourtie .	57 19	2 18	108·4		
			53·5		Concentric
B1/9 South Ythsie . .	57 22	2 11	28·0		Type B
B1/10 Foutain Hill, Tarves .	57 23	2 12	16·9		
B1/16 Westerton . .	57 16	2 29	49·0		
B1/18 Holywell or Ardlair .	57 20	2 45	37·6		
B2/1 Tyre Bagger, Dyce .	57 13	2 14	59·3		
B2/2 Sun Honey . .	57 8	2 28	83·2		
B2/4 Esslie Major . .	57 1	2 28	76·2		
			59·2		Concentric
			20·6		
B2/5 Esslie Minor . .	57 1	2 27	43·6		
B2/6 Garrol Wood . .	57 1	2 27	58·9		Type B
B2/7 Cullerlie . . .	57 8	2 21	33·4		
B3/1 Aquorthies, Kingausie	57 3	2 10	75·1		
			49·7		Concentric
B3/3 Raedykes . . .	57 0	2 16	57·1 ±	315·5	
B3/4 Raedykes (N) . .	57 0	2 16	32·5		
B4/1 Carnousie House .	57 33	2 32	84·0	163·1	
			27 ±		
B4/2 Burreldales . .	57 35	2 32	21·3		
B4/4 Milltown . . .	57 32	2 45	92·0		
B5/1 Urquhart . . .	57 40	3 11	110		
B7/1 Clava . . .	57 28	4 4			
Middle Tumulus .			103·9	189·2	
South-west Tumulus .			103·6	232·8	
North-east Tumulus .			103·5	413·7	Egg shaped
B7/2 Miltown of Clava .	57 28	4 5	59·1		
			22·0		Concentric
B7/5 Daviot . . .	57 26	4 7	48·7		
B7/6 Castle Dalcross . .	57 31	4 2	39·2		
B7/10 Easter Delfour, Alvie	57 9	3 54	57·2		
			23·6		Concentric
B7/12 Aviemore . .	57 12	3 50	76·0		
			43·0		Concentric
B7/14 Belladrum . .	57 26	4 28	10·8		
B7/15 Mains of Gask .	57 24	4 12	119·9		
			82·9		Concentric
B7/16 West Farr . .	57 22	4 12	113·2		Type A
			66·8		Concentric
B7/17 Farr (P.O.) . .	57 22	4 11	32·0		
G7/2 Seven Brethren . .	55 8	3 14	65·5		Type A Resurveyed
G7/3 Wamphray . .	55 15	3 21	38·0		
G7/4 Loupin Stanes . .	55 16	3 10	37·7		Type A
			44·0 ±	65·5	Resurveyed
G9/10 Borrowston Rig .	55 46	2 42	136·0		Egg shaped
M8/1 Loch Creran . .	56 31	5 21	12·6		
P1/3 Killin . . .	56 28	4 19	32·2		Type B

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TABLE 2 (*continued*)
Circles in Scotland

Site	*Lat.* ° ′	*Long.* ° ′	*D.* (*ft*)	*L.* (*ft*)	*Remarks*
P1/4 Weem Carse . .	56 37	3 57	15·4		
P1/13 Monzie . . .	56 24	3 49	16·4		
P1/14 Tullybeagles Lodge .	56 31	3 36	23·0	54·0	
			31·4 ±		
P1/16 Meikle Findowie .	56 32	3 41	27		? Type B
P2/1 Leys of Marlee . .	56 35	3 22	48·4		
P2/3 Blindwells . .	56 28	3 25	28		
P2/4 Courthill or Glenballoch . .	56 37	3 20	22·8		
P2/8 Shianbank . .	56 26	3 22	27·5	70·5	
			27·5		
P2/9 Guildtown . .	56 28	3 23	27·0		
P2/11 Scone . . .	56 25	3 24	21·6		
P2/14 Spittal of Glenshee .	56 49	3 27	12·7		

It has become apparent that the most reliable values are often to be obtained from the least known and least impressive sites. One reason is that these sites are often constructed from small stones so that the circle is more accurately delineated; but it also appears that many well-known impressive sites have been tampered with by well-meaning people who have re-erected the fallen stones. Unfortunately, often no record of the re-erection is to be found. It is known definitely that H1/1, S1/1, S2/1 and P1/3 have had stones re-erected and several other sites are in this respect suspect. A little-known circle of stones on a lonely moor is most likely to have escaped attentions of this kind and we can assume that where a stone, even a small stone, in such a circle has its major axis vertical it is in, or at least near, its original position.

2. ANALYSIS

Broadbent (1955) has given a method of assessing the probability of the reality of an *a priori* value of a quantum. In a later paper (Broadbent, 1956) he dealt with the case where the assumed value comes from the measurements themselves. It seems to the author that a sufficiently rigid proof lies in the fact that the measurements made *since* the publication of his earlier paper in themselves show a high significance value.

It may be of interest to look at the presentation of the results in Fig. 1. The circle diameters were arranged in order of magnitude and numbered so that they could be plotted consecutively. It is seen how the diameters progress in steps, tending to cluster round the integral fathoms.

About 55 per cent. of all circles have the diameter an even number of fathoms. In setting out a circle it is the radius rather than the diameter which has to be measured out on the ground, so that where the diameter is an odd number of fathoms, that is, in about 45 per cent. of all circles, the constructors used a half fathom. Thus for our present purpose it seems better to take as the unit a length of half a fathom (2·72 ft) and for convenience call it the megalithic yard.

With Broadbent's notation (Broadbent, 1955) put

$$y = 2r\delta + \epsilon$$

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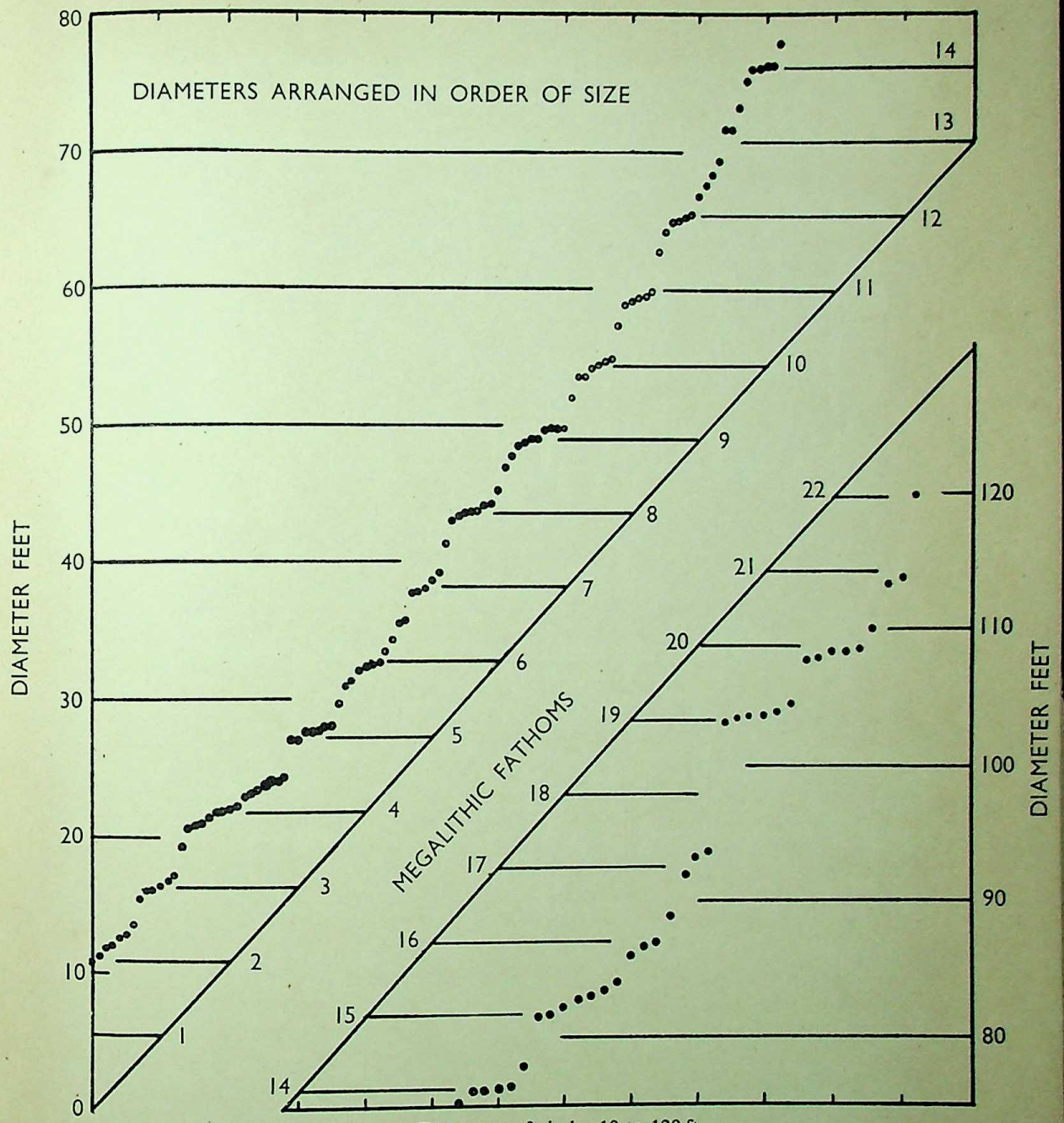

FIG. 1. Diameters of circles 10 to 120 ft.

where y is the radius or other length,
2δ is the quantum to be examined, e.g. the megalithic yard,
r is a positive integer,
and ϵ is the deviation of the measurement from the expected length.

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Taking the quantum (2δ) to be 2·72 ft the value of ϵ was found for each radius. Fig. 2 shows the histogram for $|\epsilon|$ obtained by dividing the half range 1·36 into twelve equal parts. Fig. 3 shows the 18 measured distances between circles (L) treated in the same way. The overwhelming significance of the megalithic yard for the radius (i.e. the megalithic fathom for the diameter) becomes apparent, but it is interesting to see whether the large or the small circles contribute most to the agreement.

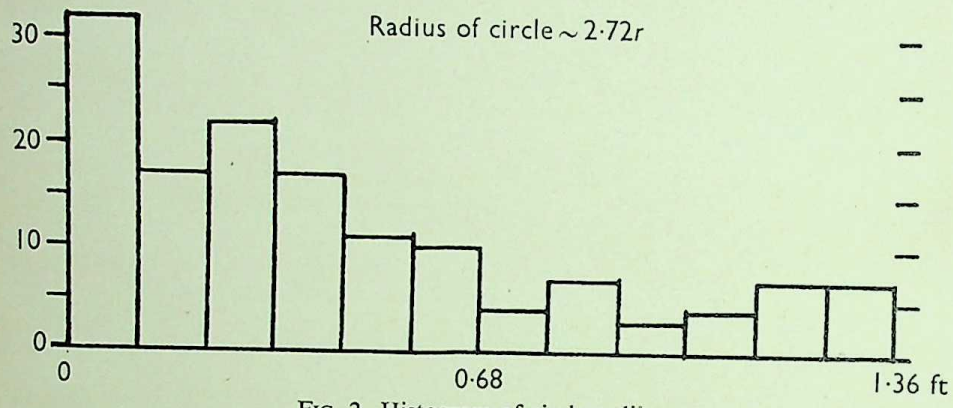

FIG. 2. Histogram of circle radii.

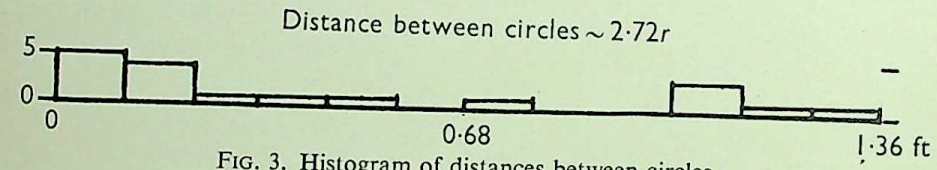

FIG. 3. Histogram of distances between circles.

No deterioration of the agreement with increasing size of circle is apparent in Fig. 1. To examine this matter in greater detail the total of 141 circles has been divided into three groups, small, medium and large, containing respectively 50, 50 and 41 circles. With Broadbent we have for the "lumped variance"

$$s^2 = \frac{1}{n}\Sigma\epsilon^2.$$

The results are given in Table 3.

TABLE 3

The effect of size of circle on the variance

Radius (feet)	*Number in group*	$\Sigma\epsilon^2$	s^2	s^2/δ^2	P
5–19	50	19·8	0·396	0·214	0·0005
19–38	50	17·3	0·345	0·187	0·0002
38–185	41	9·9	0·241	0·130	0·0001
all	141	46·9	0·333	0·180	<0·00001

P is estimated by extrapolation from Broadbent (1955, Table 3) as being the probability level.

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If the circles were set out by, say, pacing the radius, the error would increase with size. Instead we see that the variance (s^2) decreases with increasing radius. This probably means that the large important circles were set out with much greater care; however, another explanation should be considered. Suppose that, for some reason some circles were set out with the diameter ($m+\frac{1}{2}$) fathoms (m an integer), i.e. with a radius ($m+\frac{1}{2}$) yards. Fig. 2 shows that in fact some 23 per cent. lie nearer the half yard than the yard. If a larger percentage of these half-yard circles lies in the lower group, then this would raise the variance in this group as calculated in Table 3. The actual percentages are, 28 per cent. in the lower, 22 per cent. in the middle, and 17 per cent. in the upper 41. Here we are in a dilemma; we do not know whether these percentages are due to the larger circles being more carefully set out or whether the apparent better setting out of the larger circles is due to these percentages representing a greater tendency towards the use of half yards for the smaller circles.

In fact there remains unsolved the problem of determining statistically the probability level at which we can accept the hump at the right of Fig. 2 as being real. If it were real and we could remove it, the variance from the integral megalithic yard would be lowered by some 60 per cent., leaving a standard deviation on the radii of the larger circles of less than 4 in. or about 1 in 350. This is almost comparable with ordinary chain survey accuracy and indicates that future surveys of circles ought to be made with a steel rather than a cotton tape. It is most impressive to superimpose the tracings of circles from opposite ends of the country when the diameters are obviously meant to be identical, e.g. G9/10 from Berwickshire and S1/1 from Cornwall. Both of these are egg-shaped circles, but the stones in the circular parts fit together on to what appears to be identically the same circle.

3. Distances Between Stones

The difficulty of knowing how to treat the subdivisions of the fathom or of the yard led the author to look to other megalithic measurements, if possible of shorter lengths. Without very much hope of finding anything useful the distances between stones (centre to centre) in rows were tried. All the alignments of which surveys were available were used and the distances between the centres of the stones measured. The definition of an alignment given in Thom (1961) was used to decide whether or not two stones standing close should be included. The alignments at Mid Clyth (N1/1 Thom, 1961) were all omitted as the distances there seem to depend on special considerations.

The measured distances are shown in Fig. 4 arranged in ascending order. It will be seen that up to about 12 ft there is definite indication that the megalithic yard was in use. Above this distance the indication fades out almost entirely.

The first hundred measurements lie between 2·3 and 15·6 ft. Comparing each with the megalithic yard we obtain for the deviations the histogram in Fig. 5. Since the histogram is nearly as high at the right end as at the left it appears that the half yard was almost as much used as the yard. The histogram for the half yard (range $\pm 0{\cdot}68$ ft) is shown in Fig. 6. Applying the "lumped variance" test (quantum 1·36 ft) we find

$$n = 100, \quad \Sigma\epsilon^2 = 12{\cdot}25, \quad \text{Variance } s^2 = 0{\cdot}122, \quad s^2/\delta^2 = 0{\cdot}264,$$

giving a probability level of 0·01. Again we are faced with the problem of the reality of the lump at the right-hand end of the figure.

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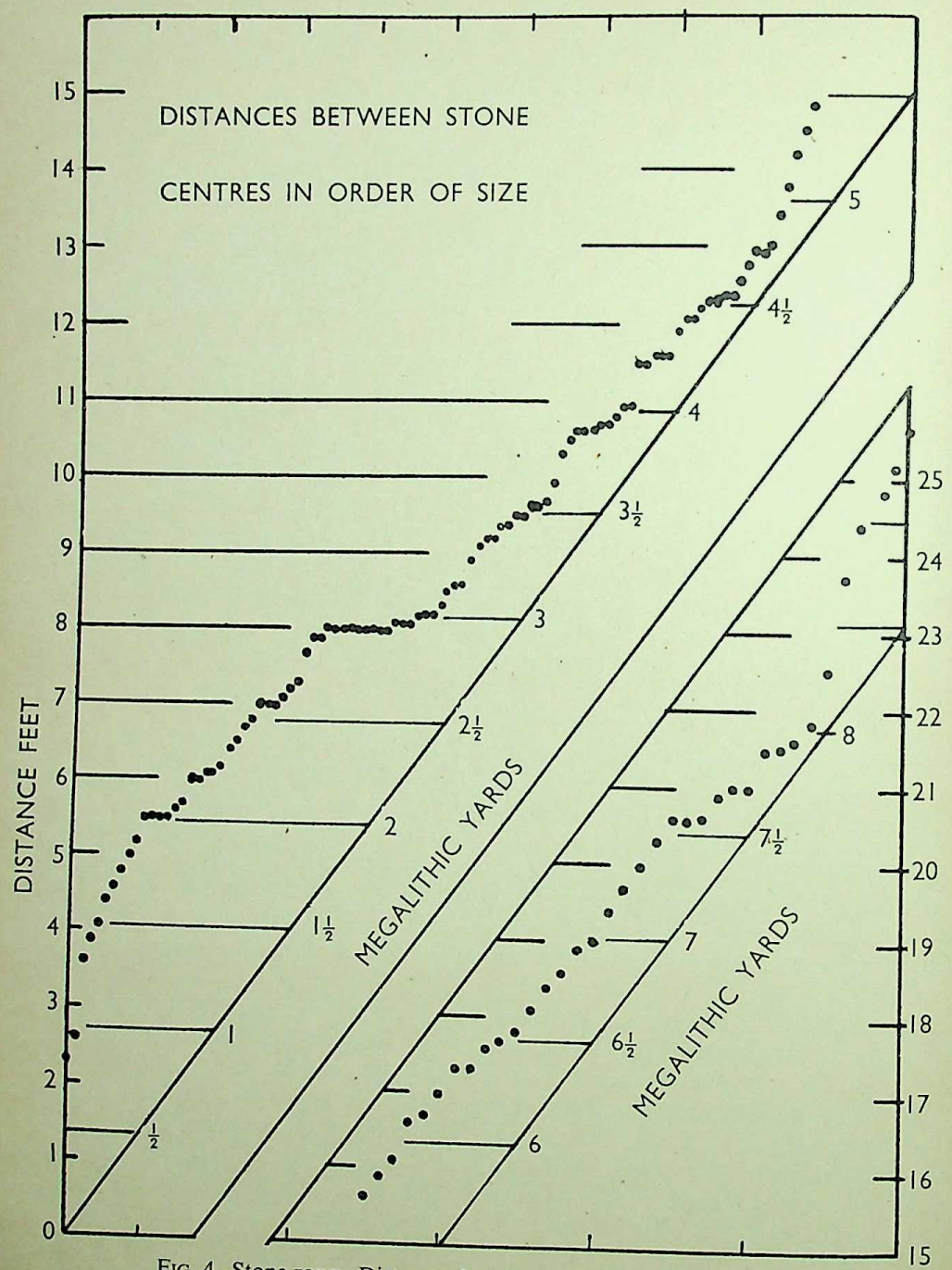

FIG. 4. Stone rows. Distances between stone centres 2 to 26 ft.

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Care must be taken in analysing subdivisions of the assumed unit. Thus, suppose we are dealing with a distribution giving a very close grouping round each multiple of the quantum or unit; if we apply Broadbent's method to the half or quarter unit, or for that matter to any subdivision, we shall still obtain a high significance which in fact would be quite misleading.

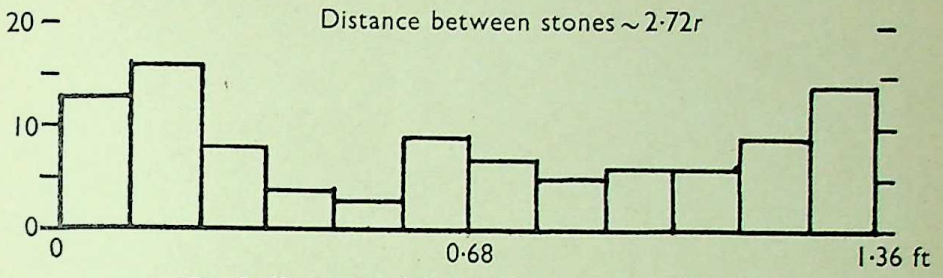

FIG. 5. Histogram of distances between stones in rows.

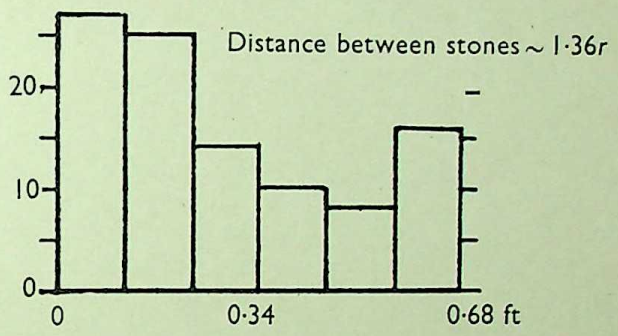

FIG. 6. Histogram of distances between stones in rows.

4. CONCLUSIONS

In conclusion we can say that from an examination of the radii of megalithic circles it is certain that the unit of 2·72 ft was used with the possibility that the yard was occasionally halved. From an examination of the distances between stones we see a high probability (0·01 level) for the use of the half yard (1·36 ft); the use of the quarter yard cannot be ruled out but no probability level can be attached.

It seems likely that the larger circles were set out with greater care and accuracy than the smaller.

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Estimating Efficient Production Functions under Increasing Returns to Scale

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Summary

This paper applies the method developed in Farrell (1957) of estimating efficient production functions to the case of increasing returns to scale. Two methods of dealing with the non-convexity implicit in increasing returns are discussed, and both are used in analysing observations on British farms.

1. Introduction

An earlier paper by one of us (Farrell, 1957) developed, *inter alia*, a method for estimating efficient production functions from observations of the inputs and outputs of a number of individual production units. The method consisted essentially in plotting these observations as points in a space of a suitable number of dimensions, forming the convex closure of this set of points and taking the appropriate part of the surface of this convex closure as the estimate of the efficient production function.

In the case where n inputs are used to produce a single output under conditions of constant returns to scale, if output is represented by X and the inputs by $x_1, x_2, \ldots, x_n$, then each firm can be represented as a point in n-dimensional space with coordinates $(x_1/X, x_2/X, \ldots, x_n/X)$. This set of points is then augmented by the points at infinity on each of the n axes, and the convex closure of the augmented set is formed. The surface of this convex closure, omitting the facet at infinity, is our estimate of the efficient production function.

The earlier paper was primarily concerned with this simple version of the method. Not only was it used for the basic exposition but a large part of the paper was devoted to applying it to observations on American agricultural production. The paper did, however, discuss at a formal level the relaxation of the assumptions of single product and constant returns to scale, and in Section 2.3 showed how the method could be generalized to the case of m outputs $X_1, X_2, \ldots, X_m$ by treating each observation as a point in $n+m$-dimensional space with coordinates $(X_1, X_2, \ldots, X_m, x_1, x_2, \ldots, x_n)$. A broadly similar procedure which would accommodate the possibility of decreasing returns to scale was indicated in Section 2.4.

Underlying the use of the method in all these cases is the assumption that the efficient production function to be estimated is convex. In no case does this assumption hold necessarily, but it is one frequently made in economics, and will probably hold in most actual situations. Certainly there is nothing in the nature of multiple outputs or decreasing returns to scale that is inconsistent with the convexity assumption.

This happy state of affairs ceases to obtain as soon as we consider a situation with increasing returns to scale, because this essentially involves a non-convex production function; indeed they are in practice probably the most important example of non-convex production functions. The estimation of efficient production functions under

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conditions of increasing returns to scale is thus both important and difficult; the present paper discusses two solutions to this problem and, by way of illustration, applies them to a set of empirical observations.

2. The Grouping Method

The first solution is the rather obvious one outlined in Farrell (1957). It consists simply in grouping the observations according to output and then estimating the efficient production function for each output group separately, and we shall call it the "grouping" method. It will obviously work only if there are enough observations to permit homogeneous output groups with an adequate number of observations in each. It is therefore particularly satisfactory to have found a set of observations (Jones, 1955) where not only are increasing returns to scale likely to be present, but also the observations are quite numerous. Jones's basic data were the individual returns of the Farm Management Survey for 1952–53. He selected 2,363 farms drawn from all parts of England and Wales, excluding only specialized pig and poultry farms and a few farms in which "trading" seemed to be financially important.

For the present exercise, we have not gone back to the individual returns but have worked from a summary table (Table 1) presented by Jones in his article. Here he cross-classifies the 2,363 farms into 25 groups according to labour input and 18 groups according to land input, making a total of 450 cells, of which about half are empty. For each cell he states (a) the number of farms in the cell, (b) their average purchases of fodder and (c) their average gross output. In addition, Mr Jones kindly provided us with an unpublished table showing the corresponding value of capital for each cell. We derived our observations from these tables in the following manner. Since the average labour input and average land input for each cell were not specified we discarded all the open-ended cells—the first and last rows and columns of the table—and for the remaining cells took the mid-points of the intervals as estimates of the appropriate average inputs. This left us with observations on five variables—output, and inputs of labour, land, fodder and capital—for each of 208 non-empty cells. We treated each cell—or group of farms—as a point and the 208 sets of coordinates are set out in Table 1, grouped according to output.

It is obvious that working from these summary tables has led us to use several simplifying procedures. First, the use of mid-points as estimates of cell means is obviously only an approximation; but, since the groupings for labour and land are quite fine, it should be a satisfactory approximation. Secondly, we have perforce grouped the original data, so that each observation represents, in general, not a single farm but a group of farms with similar land and labour inputs. Our method of estimating efficient production functions involves two sorts of variation: first, errors of observation—the type of random error commonly dealt with in statistical work—and secondly, variations in efficiency, which can lead to deviations of particular observations from the efficient production function in one direction only. (See Farrell, 1957, especially Sections 3.3 and 3.6.) Only by making assumptions as to the distributions of these two types of random variable can one reach any conclusion as to the effect on the estimated production function of using grouped observations. Here we may observe that our grouping procedure will not necessarily lead to a worse estimate, and that the particular grouping adopted preserves a large degree of independent variation of land and labour inputs. In sum, although working from Jones's published data has forced upon us a number of approximations, it does not seem likely that they will have a seriously adverse effect on our analysis.

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n	X	x_1	x_2	x_3	x_4
201	387	70	·625	163	540
202	718	25	·625	293	452
203	745	15	·875	389	649
204	768	15	·625	503	656
205	784	90	1·125	175	1536
206	813	37·5	·875	358	724
207	892	15	1·375	211	601
208	948	25	1·125	358	579
209	962	25	·875	324	576
210	976	70	·875	60	925
211	989	25	3·375	257	718
301	1055	225	2·875	338	3934
302	1080	37·5	1·125	354	842
303	1195	15	1·125	660	705
304	1263	37·5	1·375	484	1002
305	1301	70	1·125	305	1101
306	1301	115	1·375	286	1613
307	1346	25	1·375	526	898
308	1347	70	1·375	333	1253
309	1403	25	1·625	390	798
310	1409	15	1·875	681	785
311	1455	115	1·625	306	1733
312	1480	52·5	1·375	441	1215
313	1522	52·5	1·125	517	1004
314	1547	37·5	1·875	483	1101
315	1559	70	1·625	390	1559
316	1577	90	1·375	296	1623
317	1600	90	1·625	410	1403
318	1610	25	3·125	164	1138
319	1613	52·5	1·625	552	1351
320	1620	15	1·625	919	1005
321	1649	37·5	1·625	526	1172
322	1651	145	1·875	442	2368
323	1715	52·5	1·875	550	1412
324	1726	37·5	2·125	643	1256
325	1734	25	1·875	265	865
326	1851	37·5	2·375	338	1063
327	1855	52·5	2·125	588	1658
328	1857	90	1·875	606	1844
329	1861	225	2·125	277	2127
330	1910	70	1·875	521	1584
331	1938	25	2·125	672	1070
332	1982	70	2·125	526	1602
333	1996	145	1·625	661	2674
401	2055	115	2·125	486	2156
402	2180	115	1·875	400	2017
403	2197	115	1·125	260	2879
404	2197	70	2·375	578	1786
405	2233	25	2·375	516	1160
406	2251	525	1·375	12	3490
407	2251	90	2·375	519	2081
408	2262	52·5	2·375	596	1593
409	2274	225	2·375	249	2698
410	2285	145	2·625	394	2807
411	2286	90	2·125	597	2041
412	2300	145	2·375	549	2580
413	2398	115	2·625	422	2194
414	2408	37·5	2·625	404	1356
415	2421	115	2·375	472	2379
416	2480	37·5	7·5	30	1257
417	2490	180	2·625	362	3330
418	2506	52·5	5·5	0	802
419	2546	70	2·625	798	2042
420	2556	145	2·125	613	2762
421	2559	90	3·125	638	2041
422	2564	37·5	3·125	536	1029
423	2578	90	2·625	582	2176
424	2633	145	3·125	347	2611
425	2651	52·5	2·625	882	1888
426	2671	225	1·875	210	3224
427	2674	70	2·875	725	1839
428	2747	52·5	2·875	945	1767
429	2881	90	2·875	764	2101
430	2896	180	2·375	545	3356
431	2935	337·5	2·625	265	4305
432	2967	115	3·125	586	2431
433	2972	115	3·375	613	2466
434	2978	70	3·125	686	2002
435	2981	115	2·875	577	2423
501	3001	145	2·875	462	2886
502	3013	180	2·125	720	3014
503	3016	70	3·375	692	1948
504	3104	52·5	3·125	1001	1840
505	3160	337·5	3·375	368	5171
506	3178	90	3·375	614	2329
507	3244	275	2·375	262	4348
508	3251	180	3·125	312	2833
509	3253	225	2·625	542	4511
510	3283	180	2·875	703	3568
511	3304	225	3·125	591	4298
512	3347	225	3·375	266	4660
513	3424	145	3·375	585	2960
514	3428	70	4·75	285	1510
515	3528	70	3·75	1148	2195
516	3580	90	3·75	752	2413
517	3584	25	2·625	1692	2044
518	3599	337·5	3·75	126	5473
519	3648	52·5	3·75	1088	1900
520	3751	37·5	2·875	1188	1821
521	3751	337·5	3·125	356	5804
522	3757	525	4·25	612	5860
523	3775	115	3·75	996	2828
524	3809	180	3·75	495	3693
525	3845	225	4·25	451	4691

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n	X	x_1	x_2	x_3	x_4
526	3847	750	3·75	689	3358
527	3856	52·5	4·25	621	1502
528	3943	145	3·75	805	3111
601	4022	180	3·375	580	3640
602	4028	275	2·875	55	5732
603	4050	37·5	3·75	2232	1982
604	4117	525	3·75	889	5918
605	4184	70	4·25	861	2103
606	4226	412·5	2·375	170	5693
607	4250	90	4·25	1206	2600
608	4258	412·5	4·25	136	5026
609	4271	25	3·75	849	1248
610	4340	225	3·75	525	5014
611	4426	90	6·5	1049	3022
612	4503	145	4·25	872	3248
613	4512	180	4·25	797	4164
614	4568	115	4·25	1320	2854
615	4590	115	4·75	978	2960
616	4649	275	4·75	805	4180
617	4678	145	4·75	958	3490
618	4736	180	1·875	775	4547
619	4769	52·5	3·375	1991	2207
620	4924	145	5·5	839	3742
621	4942	225	4·75	913	4737
701	5127	70	5·5	509	2072
702	5185	180	4·75	772	4153
703	5251	90	4·75	1552	3059
704	5260	412·5	3·75	304	5636
705	5302	275	3·75	731	5686
706	5351	275	4·25	448	5122
707	5370	115	7·5	196	1807
708	5489	115	5·5	1257	3694
709	5598	90	5·5	1496	2866
710	5722	70	6·5	173	2666
711	5856	115	8·5	94	1334
712	5885	145	6·5	1360	3842
713	6174	225	5·5	1013	5001
714	6213	337·5	5·5	533	5768
715	6234	115	6·5	796	2515
716	6303	180	5·5	1461	4586
717	6379	412·5	5·5	50	5181
718	6593	337·5	4·75	1074	8230
719	6664	412·5	6·5	476	6556
720	6700	750	4·25	80	8196
721	6738	525	6·5	228	5527
722	6796	412·5	4·75	908	6868
723	6856	90	7·5	296	1958
724	6868	337·5	6·5	758	5749
725	6932	275	6·5	1058	5621
726	6958	275	5·5	684	6147
801	7030	180	7·5	1455	4426
802	7043	180	6·5	1864	5224
803	7226	525	5·5	1779	7162
804	7384	90	11·0	258	2674
805	7524	225	6·5	1420	5025
806	7548	750	5·5	635	9170
807	7782	145	7·5	1736	4433
808	7857	90	8·5	1280	3417
809	7978	225	7·5	1706	5641
810	8104	337·5	4·25	1544	9258
811	8188	525	7·5	594	7901
812	8376	337·5	7·5	897	6730
813	8782	145	11·0	322	2064
814	8910	412·5	7·5	889	7791
815	9024	275	8·5	1491	5711
816	9041	275	7·5	1618	6550
817	9126	180	8·5	1677	5264
818	9208	525	8·5	789	9417
819	9212	225	8·5	1940	6060
820	9280	145	8·5	1777	3997
821	9321	180	14·5	349	4492
822	9417	180	11·0	2376	5908
823	9528	337·5	8·5	1510	6964
901	10083	412·5	8·5	1270	8102
902	10248	275	11·0	1304	5555
903	10588	750	8·5	944	8347
904	10721	337·5	11·0	1431	7577
905	10747	1075	5·5	116	12966
906	11367	115	11·0	3575	4263
907	11368	750	11·0	1246	11673
908	11836	225	14·5	2181	5486
909	11844	145	14·5	1570	4090
910	12287	225	11·0	1809	6945
911	12805	412·5	14·5	2016	7840
912	12844	525	11·0	1437	10106
913	13318	412·5	11·0	1826	8828
914	14818	337·5	14·5	3115	8370
1001	15226	180	21·0	1900	5546
1002	15634	525	14·5	1760	9697
1003	15951	275	14·5	2861	7481
1004	17070	750	14·5	1495	12769
1005	17432	225	21·0	817	6152
1006	19441	412·5	21·0	2016	9349
1007	19985	275	21·0	31	2860
1008	19994	750	21·0	1847	13261
1101	20496	525	21·0	2347	9889
1102	21154	337·5	21·0	2544	8828
1103	24354	525	35·0	4030	19068
1104	28022	1075	14·5	3756	15672
1105	28569	1075	35·0	2299	16534
1106	29851	1075	21·0	1712	20406
1107	33048	750	35·0	6383	18375
1108	33845	412·5	35·0	6026	17140
1109	40112	337·5	35·0	11734	15105

For notes on Table 1, see overleaf

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Notes to Table 1

The following seem to be the most noteworthy points regarding the definitions of the variables in Table 1. But see also discussion in Jones (1955).

X. Gross output in £'s. It represents the proceeds of all sales of livestock, crops and milk at the appropriate wholesale prices and corrected for changes in stocks.

x_1. Land input in "adjusted acres". This means that land scheduled as "rough grazing" has been translated into the equivalent acreage of "good land", sometimes on the basis of the relative valuation of the two types of land and at other times using a conversion ratio of 6 acres rough grazing to 1 acre good land.

x_2. Labour input in "man year equivalents". These are obtained by dividing the total wages in cash and kind, plus the estimated value of unpaid family labour, by the current minimum annual wage.

x_3. Fodder input: actual expenditure on purchased feeding stuffs measured in £'s.

x_4. Capital in £'s. This is "tenant's capital"—the sum of the values of livestock, machinery and equipment, as stated on the returns (average of opening and closing inventories).

Efficient surfaces were estimated for each of the 10 output groups and it would have been a simple matter to produce tables specifying these surfaces by their vertices and, indeed, giving their tangential coordinates as well. But although such a specification is exact and complete, it does not lend itself readily to interpretation by

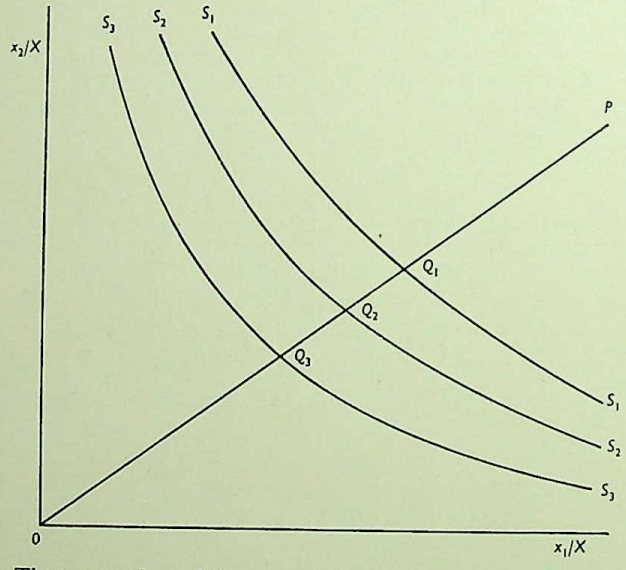

FIG. 1. The generation of the average cost curves described in Section 2.

inspection, and it seems desirable to seek a form of presentation with more intuitive appeal. There are in fact a large number of possible graphical methods of presentation. For instance, a surface in three dimensions can be represented by a contour map in two dimensions. But, to investigate economies of scale, one must compare the production functions that correspond to different scales of operation, so that even with only three inputs, one is concerned with a four-dimensional production function, and this requires whole sets of two-dimensional contour maps for its graphical presentation.

For investigating economies of scale, the following graphical representation has considerable advantages. Consider, for the sake of exposition, the case of two factors of production and constant returns to scale. Then any efficient production function can be represented as in Fig. 1 by a curve like $S_1 S_1$, and the observed performance of

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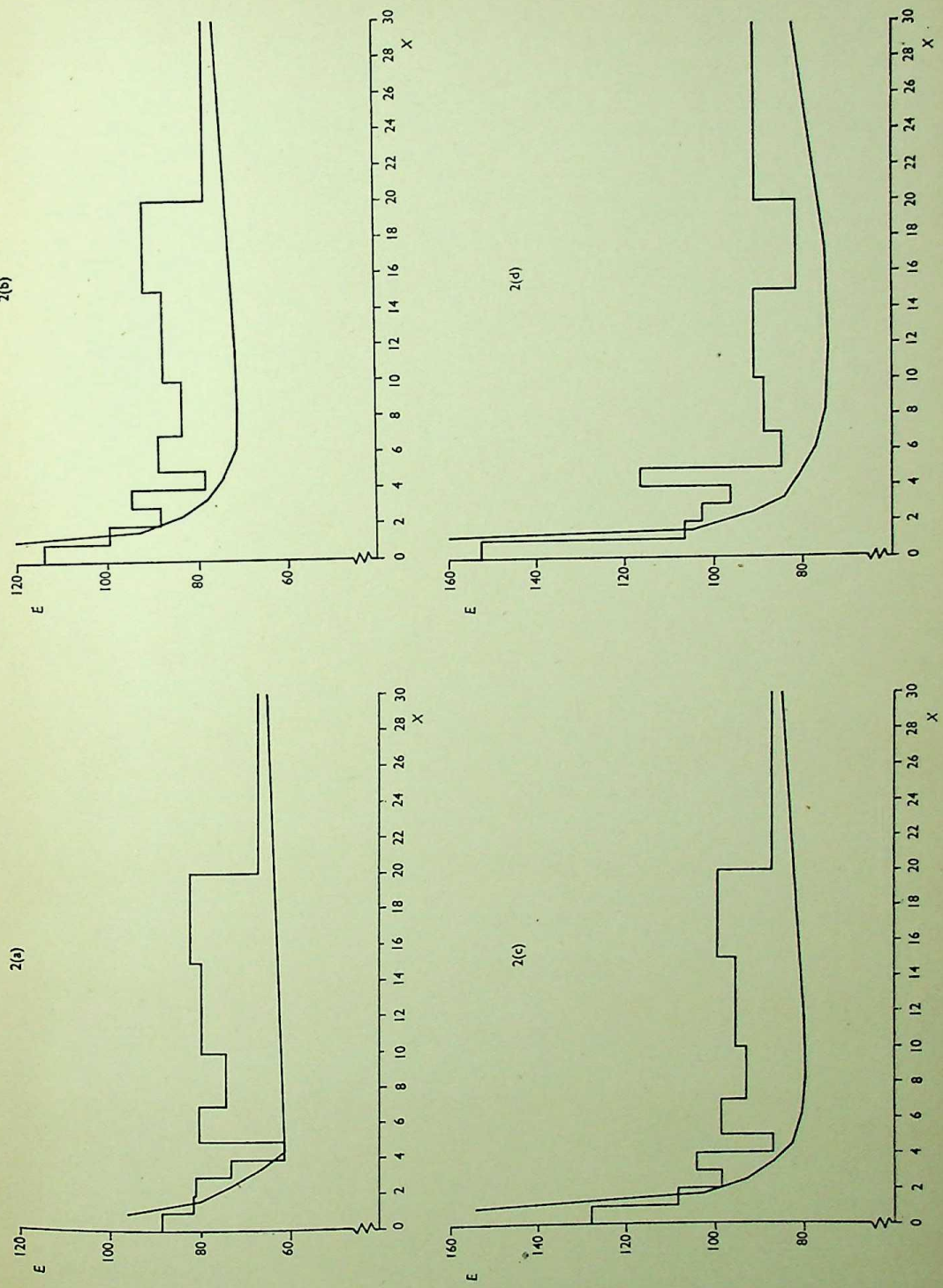

FIG. 2. Representative average-cost curves generated by: (a) P_3; (b) P_6; (c) P_8; (d) P_{11}.
(The scale of X is numbered in £'000.)

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any production unit can be represented by a point—say P. Now suppose that OP intersects S_1S_1 in Q_1. In Farrell (1957) the ratio OQ_1/OP was defined as the "technical efficiency" of P with respect to S_1S_1. The properties of this ratio as a measure of efficiency were there extensively investigated but here we need only one property—that, given the prices of the inputs, the ratio OQ_1/OP is equal to the ratio of the costs of production at Q_1 to those at P. Now suppose that there are a series of efficient production functions $S_1S_1, S_2S_2, S_3S_3, \ldots$; then, given the prices of the factors of production, the series of ratios $E_1 = OQ_1/OP$, $E_2 = OQ_2/OP$, $E_3 = OQ_3/OP, \ldots$ will be proportional to the costs of production at $Q_1, Q_2, Q_3, \ldots$.

This argument holds for any arbitrary point P and generalizes readily to more than two factors of production. Suppose that there are n inputs and that $S_1, S_2, S_3, \ldots$ are a series of efficient production functions in n dimensions; take any arbitrary point P and let OP intersect these surfaces in $Q_1, Q_2, Q_3, \ldots$; then if $E_1, E_2, E_3, \ldots$ are the technical efficiencies of P with respect to this series of efficient surfaces, the series of efficiencies will be proportional to the costs of production at the points $Q_1, Q_2, Q_3, \ldots$; and the latter are simply those points on the efficient surfaces with the same input proportions as P. If we now return to our problem of economies of scale and interpret the efficient surfaces $S_1, S_2, S_3, \ldots$ as the efficient production functions at different scales of output, we may plot the series of efficiencies $E_1, E_2, E_3, \ldots$ against output, and so produce what are, subject to a factor of proportionality, a series of points on an average-cost curve. This average-cost curve will differ from that of the economics text book, since the latter presupposes that factor proportions are varied so as to give the minimum average cost for each level of output, while we assume constant factor proportions, as determined by the arbitrary point P. Our curve, however, is potentially more informative, for we can, by choosing a variety of P's, obtain a number of different average-cost curves.

Fig. 2 shows a number of such average-cost curves, each corresponding to a different P. The curves derived by the method of grouping appear as step-functions.

3. The Overall Method

But before commenting on these results we should consider the second method of analysis—that which led to the continuous curves in Fig. 2, and which we shall call the "overall" method. First, let us ask what we mean by saying that a production function is convex.

Consider a production function relating the quantity X of a single output to the inputs $x_1, x_2, \ldots, x_n$ of n inputs. This production function may be thought of as stating the largest amount of X that can be obtained from any given set of inputs and so represented by the equation

$$X = f(x_1, x_2, \ldots, x_n).$$

Alternatively, we may think of it as determining the set of all combinations of inputs and outputs that are technically possible. In this case we shall represent it by the inequality

$$X \leqslant f(x_1, x_2, \ldots, x_n).$$

This set of points is sometimes referred to as the "attainable point set" and when we talk of the production function being convex we really mean that the attainable point set is convex.

Thus the production function may be represented as a surface bounding a set of points in $n+1$ dimensions; and it is usual to take these dimensions as corresponding,

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one to output, and one to each of the inputs, so that the coordinates of any point are $(X, x_1, x_2, \ldots, x_n)$. If we represent a production function in this way, the statement that increasing returns to scale obtain is equivalent to the statement that this attainable point set is non-convex in a particular way. But the representation is not unique, for there are an infinite number of ways in which any given production function can be represented in $n+1$-dimensional space, and an infinite number of ways of representing the attainable point set, some of which may be convex while others are not. In other words, it may be possible by a suitable transformation of the variables to find a representation of the production function in which increasing returns to scale do not imply a non-convex attainable point set. A simple example of such a transformation is

$$y_1 = X,$$
$$y_i = x_{i-1}/X \quad i = 2, 3, \ldots, n+1.$$

This leads us to represent any combination of output and inputs in $n+1$-dimensional space with coordinates $(X, x_1/X, x_2/X, \ldots, x_n/X)$.

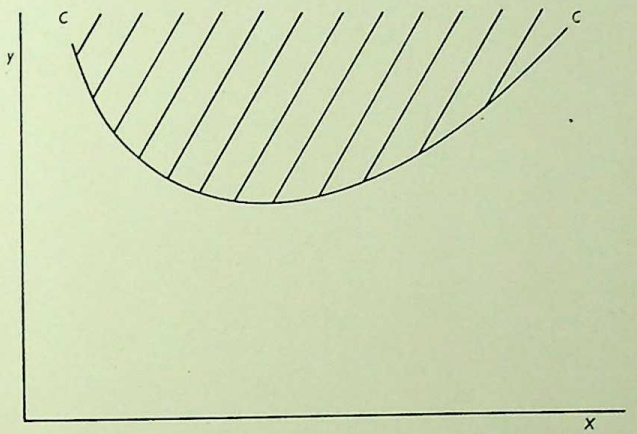

FIG. 3. Average-cost curve discussed in Section 3.

What are the economic implications of assuming that the attainable point set in this representation of the production function is convex? Consider first the case where $n = 1$, that is, where a single variable output is being produced by means of a single variable input. If we interpret Fig. 3 as representing this case, with $y = x/X$, then the production function is represented by the curve CC and the attainable point set by the shaded area. The curve CC shows the minimal values possible for x/X at any given value of X. In other words, it is a curve showing the average cost of producing X in terms of the sole variable input x, at each level of output, just as in the economics text books. As we have drawn it in Fig. 3, it is of the familiar U-shaped type and it is clear that a curve of this shape is consistent with the convexity assumption. The characteristic of the U-shaped average-cost curve is that average costs fall monotonically with output up to a certain point and thereafter rise monotonically; in other words, there is first a range of increasing returns to scale and then a range of decreasing returns.

Thus this transformation permits us to represent as convex a production function showing first increasing and then decreasing returns. In fact we get a convex representation of the production function under much more general conditions. For instance, we could have increasing returns followed by a range of constant returns to scale;

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The efficiencies of the observed points with respect to four efficient surfaces

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n	Efficiency of point n with respect to: G_3	G_4	O_3	O_4
201	54·1	55·2	100·0	100·0
202	99·4	100·0	100·0	100·0
203	93·0	93·0	100·0	100·0
204	100·0	100·0	100·0	100·0
205	61·7	61·7	86·4	86·4
206	81·3	81·5	81·0	81·0
207	100·0	100·0	100·0	100·0
208	88·7	98·2	88·2	98·6
209	100·0	100·0	100·0	100·0
210	100·0	100·0	100·0	100·0
211	85·2	92·2	80·0	89·2
301	45·0	45·0	31·6	31·6
302	82·5	82·5	87·2	87·2
303	100·0	100·0	100·0	100·0
304	81·4	81·4	82·6	82·7
305	96·9	99·1	89·5	93·2
306	84·1	84·1	72·8	74·5
307	93·5	93·5	93·4	93·6
308	86·3	87·3	80·9	83·1
309	88·5	90·5	93·3	96·5
310	97·9	100·0	100·0	100·0
311	84·0	84·0	67·8	72·5
312	91·0	91·0	87·7	88·4
313	100·0	100·0	97·9	100·0
314	80·4	80·4	80·3	80·4
315	86·3	86·3	78·8	79·9
316	100·0	100·0	84·6	88·1
317	85·0	87·6	73·8	82·1
318	100·0	100·0	100·0	100·0
319	84·6	84·6	83·0	83·2
320	100·0	100·0	100·0	100·0
321	94·2	94·2	92·3	92·4
322	75·6	75·6	59·3	64·6
323	81·9	81·9	80·1	80·2
324	79·8	79·8	77·9	77·9
325	100·0	100·0	100·0	100·0
326	84·0	86·9	81·6	84·9
327	80·9	80·9	78·2	78·2
328	81·7	81·7	71·9	75·3
329	100·0	100·0	58·1	73·1
330	89·7	89·7	81·3	83·5
331	95·4	97·7	92·3	92·5
332	85·5	85·5	77·6	80·3
333	93·9	93·9	68·1	74·9
401	68·0	80·8	68·0	75·5
402	78·1	95·1	77·1	88·5
403	100·0	100·0	100·0	100·0
404	80·5	86·5	77·8	78·9
405	100·0	100·0	100·0	100·0
406	100·0	100·0	100·0	100·0
407	76·5	83·9	74·4	77·9
408	89·2	92·5	84·9	85·1

n	Efficiency of point n with respect to: G_3	G_4	O_3	O_4
409	73·9	84·8	65·8	76·2
410	67·4	74·5	65·2	69·1
411	83·3	91·9	79·3	82·2
412	64·6	78·6	63·2	72·1
413	75·3	85·1	71·1	77·8
414	100·0	100·0	93·0	93·0
415	76·4	86·5	73·2	79·7
416	100·0	100·0	100·0	100·0
417	69·6	76·6	66·4	70·4
418	100·0	100·0	100·0	100·0
419	86·7	89·7	78·7	79·1
420	76·2	90·0	71·5	79·5
421	72·7	80·9	68·3	72·4
422	92·5	100·0	82·4	100·0
423	81·7	88·7	77·1	80·1
424	76·4	84·9	71·6	75·3
425	96·7	96·7	84·9	84·9
426	100·0	100·0	88·6	94·7
427	85·1	91·7	77·7	79·5
428	93·2	95·4	81·1	81·4
429	85·7	94·6	76·8	80·4
430	73·4	87·6	69·4	78·1
431	80·5	81·8	68·6	75·8
432	79·4	89·3	73·0	78·1
433	75·9	84·9	69·7	74·1
434	89·0	93·9	80·6	81·6
435	83·3	94·0	76·4	82·3
501	93·4	93·4	75·0	80·9
502	100·0	100·0	72·2	85·3
503	86·3	86·3	77·2	79·1
504	83·7	83·9	82·5	83·3
505	73·1	74·1	57·0	64·4
506	91·3	91·3	78·3	80·3
507	100·0	100·0	80·9	87·2
508	100·0	100·0	79·5	87·7
509	91·7	91·7	68·1	72·5
510	87·8	87·8	67·7	74·6
511	83·5	83·5	62·8	68·6
512	96·6	96·6	75·6	76·7
513	92·3	92·3	74·1	79·4
514	100·0	100·0	90·0	94·4
515	80·1	80·6	74·5	75·5
516	91·3	91·3	77·4	79·2
517	100·0	100·0	100·0	100·0
518	100·0	100·0	71·0	76·2
519	86·3	86·7	80·4	81·1
520	100·0	100·0	100·0	100·0
521	90·1	90·1	70·7	75·0
522	64·4	70·6	48·4	59·1
523	85·7	85·7	71·6	74·6
524	95·7	95·7	73·9	79·4
525	87·8	87·8	67·2	70·6

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TABLE 2—*continued*

The efficiencies of the observed points with respect to four efficient surfaces

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n	*Efficiency of point n with respect to:* G_3	G_4	O_3	O_4	*n*	*Efficiency of point n with respect to:* G_3	G_4	O_3	O_4
526	74·2	90·5	51·0	76·9	803	72·2	88·3	57·6	72·4
527	100·0	100·0	88·3	92·3	804	100·0	100·0	99·4	100·0
528	92·4	92·4	73·0	77·7	805	90·9	95·0	77·6	81·3
601	83·3	87·8	78·3	84·3	806	100·0	100·0	72·2	79·9
602	100·0	100·0	100·0	100·0	807	92·5	92·5	79·8	79·8
603	87·0	87·0	80·0	86·1	808	100·0	100·0	84·8	84·8
604	51·3	62·8	51·9	62·0	809	85·7	87·3	74·0	74·7
605	84·6	85·5	82·3	83·8	810	100·0	100·0	85·6	85·6
606	100·0	100·0	100·0	100·0	811	99·6	99·6	75·3	79·2
607	72·7	75·2	73·3	75·8	812	96·9	97·2	79·3	80·8
608	69·5	100·0	69·3	82·5	813	100·0	100·0	85·8	98·3
609	100·0	100·0	100·0	100·0	814	100·0	100·0	80·2	81·9
610	82·5	83·2	77·0	79·0	815	89·2	93·6	75·3	79·0
611	72·7	72·7	61·6	62·1	816	93·6	96·2	79·9	80·4
612	79·0	82·1	74·8	79·3	817	96·4	96·4	83·5	83·5
613	77·5	79·0	72·0	75·1	818	96·7	96·7	74·8	76·3
614	73·0	78·3	73·0	78·2	819	89·6	90·0	78·2	78·2
615	77·4	78·9	73·5	76·1	820	100·0	100·0	87·9	87·9
616	66·7	77·7	61·9	72·6	821	93·9	93·9	70·3	71·1
617	75·9	77·8	71·2	74·3	822	79·7	79·7	69·1	69·1
618	100·0	100·0	100·0	100·0	823	88·7	93·9	74·3	77·8
619	100·0	100·0	99·1	100·0					
620	83·9	83·9	71·0	73·1	901	94·3	94·3	76·8	79·2
					902	95·5	96·1	75·4	77·9
621	72·7	76·1	66·0	71·2	903	86·3	99·7	75·7	85·9
					904	91·5	91·5	73·3	73·4
701	99·8	99·8	93·0	95·9	905	100·0	100·0	100·0	100·0
702	92·2	92·9	76·6	79·9					
703	100·0	100·0	81·3	81·5	906	100·0	100·0	84·7	87·5
704	98·8	99·1	84·8	93·7	907	79·6	79·6	66·7	68·5
705	100·0	100·0	79·9	81·7	908	82·7	89·1	70·7	70·7
					909	100·0	100·0	78·8	78·8
706	97·6	97·6	86·3	89·5	910	100·0	100·0	90·8	90·8
707	79·4	86·7	81·4	94·4					
708	92·8	92·8	75·3	75·3	911	81·6	86·4	67·5	69·6
709	98·4	98·4	78·5	79·6	912	97·1	97·1	78·1	78·6
710	100·0	100·0	100·0	100·0	913	100·0	100·0	83·4	83·4
711	100·0	100·0	100·0	100·0	914	90·2	92·7	78·2	78·2
712	84·0	84·6	68·9	70·3					
713	92·1	94·0	74·6	77·8	1001	100·0	100·0	85·3	85·3
714	89·6	91·3	79·3	82·7	1002	95·9	95·9	78·1	79·1
715	95·9	98·3	85·5	89·2	1003	100·0	100·0	89·6	89·6
					1004	100·0	100·0	81·2	81·9
716	97·1	97·6	75·4	77·1	1005	100·0	100·0	94·3	94·3
717	100·0	100·0	100·0	100·0					
718	99·1	99·1	74·4	74·4	1006	89·1	89·1	82·1	82·1
719	83·5	84·5	74·5	78·3	1007	100·0	100·0	100·0	100·0
720	100·0	100·0	95·9	100·0	1008	86·5	86·5	72·5	72·6
721	84·4	90·2	75·3	88·4	1101	87·7	95·7	80·8	80·8
722	99·7	100·0	77·2	82·3	1102	100·0	100·0	91·7	91·7
723	100·0	100·0	99·1	100·0	1103	73·6	73·6	65·1	65·1
724	83·9	89·2	74·1	78·6	1104	100·0	100·0	100·0	100·0
725	87·4	90·6	71·3	75·9	1105	85·8	100·0	77·3	90·3
726	100·0	100·0	88·1	88·7	1106	100·0	100·0	100·0	100·0
					1107	82·6	82·6	82·1	82·8
801	82·5	83·5	71·0	72·8	1108	100·0	100·0	100·0	100·0
802	88·0	88·0	74·0	74·2	1109	100·0	100·0	100·0	100·0

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or increasing returns throughout. Indeed, the only situation we have to rule out is one in which a range of increasing returns follows a range of decreasing returns. We must emphasize, though, that we have only argued that this transformation enables us to represent convexly some production functions displaying various combinations of increasing returns to scale with other types of returns to scale: we have not suggested that all production functions of these types can be represented convexly.

How must these conclusions be modified in the case of many inputs? We can investigate this question, too, with the aid of Fig. 3, if we re-interpret it suitably. Suppose that there are n inputs and that the average-cost curve in Fig. 3 is like those of Fig. 2, having been generated by the specification of a particular set of input ratios, and with $y = E$. It is now necessary but not sufficient that all the average-cost curves corresponding to all possible sets of input ratios should be convex. It is also necessary that the efficient production function corresponding to any particular scale of output should be convex. But the efficient production function corresponding to a particular scale of output is simply an isoquant, and we had to assume that the isoquants were convex even in the cases of constant or decreasing returns to scale. In fact, it is necessary (though not sufficient) for the convexity of our $n+1$-dimensional efficient point set that (a) all isoquants are convex, and (b) all possible average-cost curves, as defined in this paragraph, are convex. These seem to be the only conditions that have a simple economic interpretation.

4. An Illustrative Application

By way of illustration we have estimated four efficient surfaces—G_3 and G_4 by the grouping method, and O_3 and O_4 by the overall method—and the results of these calculations are given in Table 2 and Fig. 2. The latter shows representative average-cost curves of the type already described and the former shows the efficiency of each point with respect to each of the four surfaces.

TABLE 3

A comparison of the surfaces O_3 and O_4

	O_3	O_4
No. of dimensions	4	5
No. of finite extreme points	32	38
No. of infinite extreme points	3	4
No. of facets	150	392

The surfaces G_3 and O_3 differ from G_4 and O_4 in that only three factors of production were considered, capital being omitted. We made this second set of calculations because Jones found the available measure of capital unsatisfactory. (It includes only "tenant's capital"—the value of livestock, machinery and equipment—and makes no allowance for "landlord's capital". This is unsatisfactory *a priori* and, in addition, Jones found a very low partial correlation between this capital measure and output, inputs of land and labour being held constant.) Our analysis, although by no means conclusive, tends to confirm Jones's conclusions. Table 3 gives a summary comparison of the surfaces O_3 and O_4. Introducing capital raises the number of efficient observations only from 32 to 38, but it increases the number of efficient facets from 150 to 392. However, the latter increase may reflect the greater complexity of surfaces in five than in four dimensions rather than the causal importance of capital.

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Another way of assessing the importance of an additional factor is to compare the frequency distributions of efficiencies of points with respect to the two efficient surfaces (see Farrell, 1957, Section 6.3). O_3 and O_4 both yield markedly bimodal distributions —an unsatisfactory shape—with little to choose between them. G_3 and G_4 both yield the more satisfactory J-shaped distributions, but again it is hard to see that the fourth factor has improved the distribution.

We can also compare average cost curves like those in Fig. 2. In fact we calculated such curves for ten different P's for each of the four efficient surfaces. Comparing corresponding curves for G_3 and G_4 and for O_3 and O_4 we found that the differences were usually slight and not of a type to which any obvious significance could be attached.

Thus, on none of these criteria did the introduction of capital produce a definite improvement and we must tentatively agree with Jones that the measure of capital available here is unsatisfactory and does not add to the analysis.

Can we say anything from these results about the relative merits of the grouping and overall methods of analysis? We have already noted that the overall method yields bimodal, and the grouping method J-shaped, frequency distributions of efficiencies. Intuitively it seems more plausible that a genuine distribution of efficiencies—free from errors of measurement and the effects of omitted variables—should be J-shaped than that it should be bimodal. This argument would lead us to prefer the grouping method, but it might well be that neither analysis has produced a pure distribution of efficiencies.

A glance at Fig. 2, where typical average-cost curves for G_3 are superimposed on the corresponding curves for O_3, enables us to form some rough general impressions as to the merits of the two methods. Neither in fact seems entirely satisfactory. The G_3 curves, as well as emphasizing how broad some of the output groups are, attribute large and rather implausible irregularities to the production function. (This may be why they produce more plausible distributions of efficiencies—it would be analogous to a regression analysis that produced a high correlation by using up a large number of degrees of freedom.) On the other hand, the O_3 curves, although smooth and plausible lean heavily on the highly efficient groups 6 and 11.

Finally, what can we say about the shape of the production function itself? Consider the O_3 average-cost curves. The most noticeable feature is that all the curves display sharply decreasing average costs up to group 6 or 7, and that thereafter variations are small. There are, of course, differences between the curves corresponding to different P's. These P's were chosen to represent the different input proportions used on different sizes of farm (P_i having input proportions typical of the ith group) and we would expect the point of minimum average cost to occur at higher outputs for the input ratios associated with larger output groups. This is so, but the tendency is not nearly so marked as we would have expected. Even on the average-cost curves corresponding to their own input ratios, the smaller output groups are operating under conditions of steeply decreasing average cost and the largest output group under conditions of (slightly) increasing average cost. Thus the dominant impression is not of the differences between these curves but of the similarity; all the curves show average costs decreasing sharply at first and then becoming, very roughly, constant.

This conclusion—that the major economies of scale are exhausted when a gross output of about £5,000 per annum has been reached—should not be taken too seriously, for we have excluded capital from our analysis. The only remaining input that is indivisible is labour and it may well be that economies in the use of labour are exhausted

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by the time such a size is reached. It might still be true that, if a valid capital measure could be included in the analysis, economies of scale would be found to persist to much higher output levels. But we must emphasize that the purpose of the paper was to indicate the technical possibilities of a particular method of analysis and not to draw policy conclusions.

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APPENDIX

Methods of Computation

The computations involved fall into two parts. First we calculate the efficiencies of all the points P_j with respect to this set of P's. This yields in particular a set of efficient points P_j, that is, those with efficiencies of 100 per cent. This set of efficient points, in fact, defines the efficient surface but one may also wish to calculate the actual facets of the efficient surface and this constitutes the second part of the computations.

(a) *Computing the efficiencies*

Let the points

$$P_j(y_1, y_2, \ldots, y_m) = P_j(x_1/X, x_2/X, \ldots, x_m/X) \quad j = 1, 2, \ldots, n$$

be the given observations, the y's representing input per unit output. Add to these points the infinite points on the axes

$$P_{n+i} \quad (0, \ldots, \infty, \ldots, 0) \quad i = 1, 2, \ldots, n$$

where infinity appears in the ith position and there are zeros elsewhere. The first computing problem is to find the point

$$Q_k = (1/\lambda) P_k$$

which lies in the surface of the convex hull of all the P's. This is equivalent to finding the maximum value of

$$\lambda = \lambda_1 + \lambda_2 + \ldots + \lambda_{n+m}$$

such that

$$P_k = \lambda_1 P_1 + \lambda_2 P_2 + \ldots + \lambda_{n+m} P_{n+m}$$

and

$$\lambda_j \geqslant 0 \quad j = 1, 2, \ldots, n+m.$$

The theory of convex polyhedra ensures that at this maximum not more than m of the λ_j will be strictly positive and the rest will be zero (except in degenerate cases, where such a representation can be found although the maximum is not unique). In other words, the point P_k is a linear combination of just m points, the points of its efficient

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facet. Since P_k can appear on the right-hand side in the above expressions, $\lambda \geqslant 1$. Also, if $\lambda = 1$, then P_k is a point on an efficient facet and, in practice, an extreme point of the set.

This is a linear programming problem with unit cost for each activity P_j; but the infinite points on the axes with unit cost may be replaced by unit points on the axes with zero cost, to form a unit matrix with which to start the iterations of the simplex or dual simplex methods of linear programming. For the simplex method the unit points form a "feasible basis". Each iteration introduces a new point P_j and discards one of those in the basis, keeping the facet so formed "astride" the vector P_k, until a facet is reached which is a facet of the surface—that is, an efficient facet. For the dual simplex method we must introduce a point which has the minimum value in one of the coordinates and remove the corresponding unit point. (This cannot be done if the minimum value of the coordinate is zero—a degenerate case.) This gives an elementary efficient facet and iteration by the dual simplex method then yields new efficient facets until one is found which is "astride" the vector P_k—that is, "feasible". The tableau at each iteration is "efficient" if the "costs" of each point (which we assume are initially stored negatively) are all positive, and is "feasible" if the X_j are all positive.†

Either of the above computations will determine the efficiency $(1/\lambda)$ of P_k. Next examine the final tableau for P_k to see if the efficiencies of any of the other P_j's are determined by it—that is, to see if any of the other P's have the same efficient facet, as would be shown by their having a column of positive coefficients in this final tableau. One can also use the final tableau for P_k as the starting tableau for iterations for, say, P_{k+1}, using the dual simplex rules.

With the aid of an electronic computer these methods calculate the efficiencies very quickly and are adequate for problems of the size likely to arise. It does not seem worth while in this case to attempt to reduce the number of iterations any further by more sophisticated searching procedures; but this would be both possible and desirable if the calculations were performed by hand.‡

The foregoing methods of computation have to be modified for use in connection with the "overall" method of allowing for increasing returns to scale. In this case the efficiency of a point P is computed with respect not to the origin but to a point N which is the orthogonal projection of P on the X-axis. Thus if P is $(X, x_1/X, x_2/X, \ldots, x_m/X)$ we move the origin to $(X, 0, 0, \ldots, 0)$. The efficiency of P is now calculated exactly as before, except that, for each new point, the calculations must be started again from the beginning, which naturally makes the computations much longer.

This modification can also be used to calculate the efficiency of a point in the case of l outputs and m inputs under constant returns of scale, discussed in Farrell (1957, Section 2.3). Here for each point $(X_1, \ldots, X_l, x_1, \ldots, x_m)$, we move the origin to $(X_1, \ldots, X_l, 0, \ldots, 0)$.§ As returns to scale are constant, the true origin must be part of each basis, with its weight, λ_0, negative if necessary.

† For details of the linear programming procedures the reader may refer to Vajda (1956) or Gass (1958). The Revised Simplex Procedure, in which only the current basis is inverted at each stage, is particularly suitable for the work involved here, as m is usually a good deal smaller than n.

‡ Although we have associated unit costs with the vector $(x_1/X, x_2/X, \ldots, x_m/X)$, the linear programming methods would work equally well if we were to associate costs X with the vector $(x_1, x_2, \ldots, x_m)$.

§ Alternatively we may move the origin to $(0, \ldots, 0, x_1, \ldots, x_m)$ and proceed by minimization instead of maximization.

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(b) *Computing the efficient surface*

By these methods one can determine the efficiencies of all the points P and in particular determine which of them lie in the efficient surface (have $\lambda = 1$). Given the set of efficient points, the following method is satisfactory for computing all the facets of the efficient surface using an electronic computer. It was more suitable in our case than the "double description method" because it makes much smaller demands on the limited storage space of our computer.

We shall assume that each facet is determined by just m points. (Degeneracy—that is, linear dependencies among the P's—is unlikely in empirical data and can in any case be destroyed by small perturbations of the data.) Each facet is now a simplex—the m-dimensional generalization of a triangle—and has m neighbouring facets, each having $m-1$ points in common with it. Moreover, each neighbour is "opposite" a different vertex of the facet—the vertex which is not in common.

Efficient facets correspond uniquely to "optimal" tableaux—that is, tableaux in which all the elements of the transformed cost row are positive. The m row-vectors in the basis of the tableau correspond to the m points of the facet. If the m points of a facet are $P_1, \ldots, P_m$, their coordinates, considered as column-vectors, form an $m \times m$ matrix. Let its inverse be $(P^1, P^2, \ldots, P^m)'$, where P^1 etc. are row vectors. This inverse appears in the tableau as the transformed initial unit matrix. The tangential coordinates of the facet may be identified as

$$\sum_i P^i = Y'$$

where Y' is the transformed cost row of the initial unit matrix.

If we were to force out a row-vector and allow another vector to take its place according to the second selection rule of the Dual Simplex method, the new tableau would correspond to a neighbouring efficient facet. Thus, given an efficient facet, we can compute the m neighbouring facets from the corresponding simplex tableau by considering in turn the removal of each of the m rows and observing which column would be introduced to replace it. We need not actually transform the whole tableau each time—only the label of the column vector chosen and the new Y' row are required in order to specify each neighbouring facet. The tableau for a facet may be set up by straightforward inversion of the above $m \times m$ matrix—the method actually used—or by one iteration from some known neighbouring tableau, if this is more convenient.

Using this technique we may build up the entire convex surface. As they are calculated, the vertices of each facet and its tangential coordinates can be printed out by the computer. It is not necessary to store the tangential coordinates in the computer, but it is necessary to keep in the computer a list of the vertices of each facet. Because of the limited storage space, this list must be kept in a very compact form. The following method which can be used with a binary computer also allows the logical checking of the list to be carried out very rapidly: as each facet is determined by m out of a total of $n+m$ points, including the infinite points, allocate a binary digit to each of the $n+m$ points and let it take the value "1" if the point appears in the facet and "0" if it does not. Thus each facet is represented by $n+m$ digits, containing m "1's" and n "0's".

Now, starting with a list of facets containing solely the initial efficient facet, work down the list testing each facet in turn to see whether it has m neighbours in the list. (If two facets are neighbours they have $m-1$ "1's" in common.) As each facet is

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computed, add it to the bottom of the list. (Note that as each neighbouring facet is "opposite" one vertex of the facet one can tell which neighbours to look for—that is, which rows of the basis to consider leaving out.) When the bottom of the list is reached the convex surface is complete.†

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† If only part of the convex surface is required, and this requirement can be expressed in a convenient rule, then it is possible simply to omit the non-required facets from the list when they appear. Note, too, that if the origin is external to the convex set, the dual simplex selection rule will break down when it comes to a facet "not visible from the origin". To deal with this problem, the selection rule must be modified at this stage to allow a positive pivot element and to admit the column which would change the signs of the cost coefficients from all positive to all negative. Generally, when computing efficient surfaces, we require only that part of the convex surface "visible from the origin", but if we are to allow for increasing returns to scale, we must have all parts of the surface visible from any point on the X-axis.

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The Estimation of T-year Survival Rate in Patients Treated for Cancer

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Summary

The use of parametric maximum likelihood and the actuarial method to estimate T-year survival have been investigated theoretically in the case where the survivorship function can be represented by a three-parameter model that has been shown by Boag to be a good fit to the data obtained from groups of patients treated for cancer. For a number of values of the parameters and of T, the variances of the two methods and the bias of the actuarial method have been estimated. Calculations have also been made on suitable random samples. It is shown that the actuarial method, in which T is divided into yearly sub-intervals, and the effective number at risk in a sub-interval is taken to be the number entering *minus* half the number of withdrawals during the sub-interval, is widely applicable, but has certain limitations for small values of T, and in very large samples. The effects of deaths from intercurrent disease, and other methods of estimating the effective number at risk are discussed.

1. Introduction

The T-year survival rate, although it takes no account of the quality of living, still provides the most objective standard by which the results of treatment for cancer may be assessed. Particular stress has been placed in the past on the five-year survival rate, since for cancer at a large number of sites, the percentage of patients developing recurrences more than five years after treatment is small. It must be noted, however, that about a quarter of the patients alive at five years after treatment for cancer of the breast may die with recurrence before a further five years have elapsed, while there are other sites where the survival rate at less than five years is a close measure of the rate calculated after a much longer follow-up. Furthermore, since improvements in treatment may be reflected in both early and late survival rates, the comparison of two forms of treatment can often be made usefully by a consideration of two- or three-year rates before the five-year figures become known. In practice, therefore, we are faced with the problem of estimating T-year survival rates for values of T greater than or equal to two, in a situation where patients are observed for varying lengths of time due to the extended period during which they may be entered for treatment.

The actuarial method with the patients grouped in yearly intervals has been most widely recommended for the purpose (Berkson and Gage, 1950; Stocks, 1950), while more recently Boag (1960a) has suggested the use of parametric maximum likelihood. Littell (1952) has made a very thorough comparison of these methods when the individuals studied are subjected to a constant force of mortality, and Armitage (1959) has also investigated the comparison of survival curves in the same situation. Cancer mortality cannot be represented by such a simple model involving only one parameter,

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the state of affairs found in practice being more closely represented by a three-parameter model proposed by Boag (1948). This model assumes that a proportion, c, of the patients treated are cured (i.e. the probability of their dying subsequently from cancer at the site in question is zero), while the log survival times of the remainder when measured from the date of first treatment are distributed normally with mean μ and standard deviation σ. A good approximation to this model and to some actual treatment series, is one involving only two parameters in which it is assumed that the force of mortality on the whole group of patients due to cancer is not constant but decreases exponentially with time after first treatment (Haybittle, 1959, 1960). It therefore seemed worth while to investigate theoretically the uses of the actuarial method and parametric maximum likelihood for estimating T-year survival rate in a model which closely represents the data obtained in practice from groups of treated cancer patients. The results of such an investigation are given in this paper.

2. Scope of the Investigation

The model chosen was that suggested by Boag (1948) since this represents the distribution of deaths at short survival times more closely than the two-parameter approximation referred to above. It was assumed therefore that patients enter for

Table 1

Values of parameters in models investigated

Model	μ Log_{10} *months*	σ Log_{10} *months*	c	*Type of entry*
A_1	1·3802	0·48	0·25	Continuous
A_2	1·3802	0·48	0·25	Limited to 4 years
B_1	1·0792	0·40	0·25	Continuous
B_2	1·0792	0·40	0·25	Limited to 4 years
C_1	1·0792	0·40	0·50	Continuous

treatment at a constant rate of n per year, that a proportion, c, are cured, and that the frequency distribution of the survival times of the uncured patients is given by the function $z = e^{-\frac{1}{2}x^2}/\sqrt{(2\pi)}$, where $x = (\log_{10} t - \mu)/\sigma$, and t is the survival time in months from first treatment, and μ and σ are the mean and standard deviation respectively in $\log_{10}$ months of the log survival times of the uncured group.

In view of the laborious computations involved in a three-parameter model, the investigation was confined to five particular sets of conditions representative of those found in practice. These are listed in Table 1. The parameters in A_1 and A_2 are such as might be found in patients treated for cancer of the breast: 40 per cent. and 30 per cent. survive 5 and 10 years respectively. Conditions B_1, B_2 and C_1 are typical of cancer at many other sites where late recurrences are rare, and from a quarter to a half the number of patients have very long survival times and might be considered cured. In A_1, B_1 and C_1 it was assumed that patients were continuously entered for treatment right up to the time that the estimate was made. In A_2 and B_2 the entry period was limited to 4 years. This period at least is usually required at any one centre to collect sufficient patients suffering from cancer at a particular site to make analysis worth while.

The investigation was confined to the problem of estimating T-year survival rate at a time $T+1$ years after the first patient has been treated. By allowing a year's

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entry of patients to be followed for the full period of T years, the precision of the estimate is considerably increased. For example, the standard error of the five-year rate determined by the actuarial method is reduced by nearly one-third when the estimate is carried out at 6 instead of 5 years, while waiting until 7 years effects a further reduction in the standard error of only 16 per cent. In practice therefore the gain in precision obtained by estimating T-year rate at $T+1$ years may outweigh any disadvantage caused by the extra delay before estimates are available. On the other hand, waiting until $T+2$ years will not be so worth while.

The variance and bias of the estimated T-year rate were calculated for the actuarial method with different numbers of sub-intervals, and the corresponding variances of the estimates made by the method of maximum likelihood were also computed. The methods of calculation are described in the following sections.

T-year survival rate was taken to mean the probability of not dying from cancer at the site considered before T years have elapsed from the date of first treatment, and in the first instance no account was taken in the model of possible deaths from other causes. An intercurrent death of this nature could be treated as a withdrawal at the time of death and would thus contribute its share of information to the estimate. When the entry for treatment is continuous as in A_1, B_1 and C_1, the intercurrent deaths will be only a few per cent. of the withdrawals in any one period, and their omission from the model does not invalidate the results. The effect of intercurrent deaths when there are no withdrawals due to limited follow-up is considered in a later section.

The methods of obtaining the variance and bias of the estimate of T-year survival rate are described below.

3. Parametric Maximum Likelihood Solution

In this case the estimate of the T-year rate, p_T, is given by

$$\hat{p}_T = \hat{c} + (1-\hat{c})\hat{q}_T$$

where
$$\hat{q}_T = \int_{x_T}^{\infty} z\,dx \quad \text{and} \quad x_T = (\log_{10} t - \hat{\mu})/\hat{\sigma},$$

t being the time in months, i.e. $t = 12T$.

When μ, σ and c are estimated simultaneously from the data by the method of maximum likelihood, an approximate expression can be derived for the variance of $\hat{p}_T$ in terms of the second differential coefficients of the log-likelihood. If, using Boag's notation (1948, 1949), these coefficients are denoted by $D, E, \ldots,$ and K respectively, then

$$\begin{aligned}\operatorname{var}(\hat{p}_T) = \frac{1}{\Delta}\{&(1-c)^2 z_T^2(EF-K^2) + (1-c)^2 x_T^2 z_T^2(DF-H^2)\\ &+(1-q_T)^2(DE-G^2) - 2x_T(1-c)^2 z_T^2(GF-HK)\\ &-2x_T(1-q_T)(1-c)z_T(DK-HG) + 2(1-c)(1-q_T)z_T(GK-HE)\}\end{aligned}$$

where
$$\Delta = \begin{vmatrix} D & G & H \\ G & E & K \\ H & K & F \end{vmatrix}, \quad z_T = \exp(-\tfrac{1}{2}x_T^2)/\sqrt{(2\pi)}$$

and $E, F, G, \ldots,$ etc. have their expected values at time T.

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Boag (1949) has given expressions from which the expected values of the second differential coefficients may be calculated for any particular set of the parameters μ, σ and c, in the case where N patients are all treated simultaneously and then followed for a time t subsequent to treatment. For example, the expected value of F under these conditions is

$$E(F) = \frac{N\psi}{(1-c)},$$

where

$$\psi = \{c + q_t/(1-q_t)\}^{-1}.$$

When patients enter for treatment at a constant rate of $n/12$ per month, the expected value of F at time t after the first patient begins treatment is given by

$$E(F) = \int_0^t \frac{n}{12} \frac{\psi}{(1-c)} dt$$

and the expected values of the other coefficients are given by similar integrals. If patients enter for treatment at a constant rate, but for a limited time, t_0, and the estimates are made at time t ($t > t_0$) then

$$E(F) = \int_{t-t_0}^t \frac{n}{12} \frac{\psi}{(1-c)} dt.$$

These integrations were made graphically for the conditions under investigation, and the expected values then inserted in the expression for the variance of $\hat{p}_T$ given above.

4. Actuarial Method Solution

The calculations for this method followed closely the treatment given by Littell (1952). Let the period $(0, T)$ be divided up into a intervals by division points at times $T_1, T_2, \ldots, T_j, \ldots, T_{a-1}$ and let N_j be the number of patients who have survived at least T_{j-1} years at the time of the survey. Then if d_j patients die from cancer with survival times between T_{j-1} and T_j years, and w_j patients are withdrawn in the same interval owing either to the limited observation period or deaths from intercurrent disease, the conditional probability of dying from cancer in the jth interval, given that the patient survives the first $j-1$ intervals, is estimated from

$$\hat{\tau}_j = d_j/(N_j - \tfrac{1}{2}w_j).$$

The assumption that the effective number of patients at risk in an interval is the number alive at the beginning of the interval, less one-half the number of withdrawals, leads to bias in the estimate, the extent of which will be shown later. Nevertheless this is the most commonly adopted method of dealing with the withdrawals. The estimated value of the T-year survival rate is then given by

$$\hat{p}_T = \prod_1^a (1-\hat{\tau}_j).$$

The expected values of $\hat{\tau}_j$ for the model may be calculated as follows. $T+1$ years have elapsed since the first patient was treated. Therefore all the patients entered for

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treatment in the first $T+1-T_j$ years have been observed for the whole of the jth interval. Their contributions to the expected values of N_j and d_j are therefore

$$n(T+1-T_j)\{c+(1-c)q_{j-1}\}$$

and

$$n(T+1-T_j)(1-c)(q_{j-1}-q_j) \tag{1}$$

respectively, where q_j is the value of q as defined in the previous section at time T_j, and n equals the number of patients entered per year.

In addition $n(T_j-T_{j-1})$ patients have been observed for more than T_{j-1} but less than T_j years, and therefore contribute a further $n(T_j-T_{j-1})\{c+(1-c)q_{j-1}\}$ to the expected value of N_j, which thus becomes

$$n(T+1-T_{j-1})\{c+(1-c)q_{j-1}\}. \tag{2}$$

If we consider patients treated in a small interval of time dt' and observed for a period of t' where $T_{j-1}<t'<T_j$, then the number of deaths amongst these patients between T_{j-1} and t' is $n(1-c)(q_{j-1}-q_{t'})\,dt'$. The contribution to the expected value of d_j of those patients observed for a period greater than T_{j-1} but less than T_j (i.e. the patients who will withdraw during the jth interval unless they die of cancer) is therefore

$$n(1-c)\int_{T_{j-1}}^{T_j}(q_{j-1}-q_{t'})\,dt' = n(1-c)\left\{(T_j-T_{j-1})q_{j-1}-\int_{T_{j-1}}^{T_j}q_{t'}\,dt'\right\}. \tag{3}$$

The expected value of w_j is the number open to the possibility of withdrawal in the jth interval, i.e. $n(T_j-T_{j-1})\{c+(1-c)q_{j-1}\}$, less those who die from cancer before withdrawal, i.e. those given by expression (3). Therefore

$$E(w_j) = n\left\{(T_j-T_{j-1})\,c+(1-c)\int_{T_{j-1}}^{T_j}q_{t'}\,dt'\right\}.$$

The final expression for the expected value of $\hat{\tau}_j$ is from (1), (2) and (3)

$$E(\hat{\tau}_j) = \frac{(1-c)\left\{(T+1-T_j)(q_{j-1}-q_j)+(T_j-T_{j-1})\,q_{j-1}-\int_{T_{j-1}}^{T_j}q_{t'}\,dt'\right\}}{(T+1-T_{j-1})\{c+(1-c)q_{j-1}\}-\frac{1}{2}\left\{(T_j-T_{j-1})\,c+(1-c)\int_{T_{j-1}}^{T_j}q_{t'}\,dt'\right\}}$$

which may be computed since

$$\int_{T_{j-1}}^{T_j}q_{t'}\,dt' = T_jq_j-T_{j-1}q_{j-1}+\tfrac{1}{12}(q'_{j-1}-q'_j)\exp\{\mu\log_e 10+\tfrac{1}{2}(\sigma\log_e 10)^2\}$$

where

$$q' = \int_{x'}^{\infty}e^{-\frac{1}{2}x^2}/\sqrt{(2\pi)}\,dx,$$

x' being the value of x_T at time $(T_j-\sigma\log_e 10)$. The division by 12 and the appearance of $\log_e 10$ in the expression occur because, following the practice adopted by Boag (1948, 1949), the units of μ and σ are $\log_{10}$ months, whereas T is the time in years.

If no further patients have been entered for treatment after T_0 years (as in cases A_2 and B_2 where $T_0=4$), then for sub-intervals within the period up to $T+1-T_0$ years, there are no withdrawals and

$$E(\hat{\tau}_j) = \frac{(1-c)(q_{j-1}-q_j)}{c+(1-c)q_{j-1}}.$$

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The estimate of τ_j is unbiased, i.e. the expected value approaches the true value for the model when the sample size becomes infinite. The period from zero time to $T+1-T_0$ years may now be treated as the first sub-interval, no advantage being obtained in this theoretical model by making any earlier sub-division.

The expected value of T-year survival rate is calculated from the equation

$$E\{\hat{p}_T\} = \prod_1^a \{1-E(\hat{\tau}_j)\}$$

and its variance from the approximate formula given by Greenwood (1926), which may be written

$$\text{var}\,(\hat{p}_T) = \{E(\hat{p}_T)\}^2 \sum_1^a \tau_j/\{(1-\tau_j)(N_j-\tfrac{1}{2}w_j)\}.$$

The expression within the summation sign corresponds to what Littell (1952) has called the binomial form of the variance of $(1-\hat{\tau}_j)$ divided by $(1-\tau_j)^2$. The bias of the estimate is $B = p_T - E(\hat{p}_T)$, where, as in the previous section, p_T is the true value of T-year survival rate for the model, i.e.

$$p_T = c+(1-c)q_T.$$

5. Results

5.1. *Bias of the Actuarial Method*

The calculated biases of the actuarial method when T is divided into one, two, three or yearly sub-intervals are listed in Table 2, and are independent of the total number of patients in a sample. There are a number of differences which may be noted between these results and those obtained by Littell (1952) for a constant force of mortality. In the latter case the bias is always positive and for a given number of sub-intervals increases as the T-year survival rate decreases. For models A_1, B_1 and C_1, it may be seen that B tends to be negative at early values of T, and increases *algebraically* as T increases, becoming positive for later values of T. As a result B decreases numerically as T increases until the point when B becomes positive. This point occurs later when the deaths from cancer take place over a more protracted period as in A_1 (where, even for the one-period analysis, it evidently occurs after 10 years), and also when c is greater (compare C_1 with B_1). The effect of increasing the number of sub-intervals is also to postpone the time at which B becomes positive (see particularly the results for C_1).

These results follow from the fact that in the model chosen, the force of mortality is increasing rapidly for small values of T. In these circumstances there will be more likelihood of withdrawal in the first half than in the second half of an interval and $N_j-\frac{1}{2}w_j$ will be an overestimate of the effective number at risk. Thus τ_j will be underestimated, and the contribution of $(1-\hat{\tau}_j)$ to $E\{\hat{p}_T\}$ will tend to make $E\{\hat{p}_T\} > p_T$ and the bias negative. At greater values of T, the force of mortality approaches a maximum and then decreases. Under these conditions, as for a constant force of mortality, $N_j-\frac{1}{2}w_j$ is an underestimate of the effective number at risk and the bias in each interval is positive. The greater the value of μ, as in A_1, the greater will this effect of the rising force of mortality at early years be seen. The effect of decreasing c is also to make the maximum value of τ occur later. The overall result is that for a given value of p_T in the range investigated the bias is, except when $p_T \sim 0{\cdot}5$ at $T = 10$, numerically less than that calculated for Littell's model.

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In the models having a limited entry period of 4 years (A_2 and B_2), the first period in an analysis of more than one period was always taken to be from 0 to $(T+1)-4$, and the remaining 3 years were divided to give equal further sub-divisions. For a one-period analysis the bias is always positive and much larger numerically than the corresponding value for continuous entry. This is to be expected since there are no withdrawals at early values of T, and more withdrawals therefore occur in the second half of the interval than in the first. As discussed previously, therefore, the effective number at

TABLE 2

Bias of different actuarial estimators

Model	*T (years)*	p_T	*Bias*			
			One period	*Two period*	*Three period*	*Yearly*
A_1	3	0·5177	−0·0371	−0·0113	−0·0060	−0·0060
	5	0·4027	−0·0353	−0·0126	−0·0064	−0·0028
	7	0·3464	−0·0260	−0·0106	−0·0059	−0·0014
	10	0·3045	−0·0104	−0·0074	−0·0048	−0·0007
A_2	5	0·4027	+0·0537	−0·0008	−0·0002	−0·0001
	7	0·3464	+0·0742	+0·0019	+0·0006	+0·0004
	10	0·3045	+0·0803	+0·0016	+0·0005	+0·0003
B_1	2	0·4194	−0·0466	−0·0147	−0·0071	−0·0147
	3	0·3374	−0·0413	−0·0151	−0·0077	−0·0077
	5	0·2802	−0·0165	−0·0121	−0·0073	−0·0034
	7	0·2630	+0·0059	−0·0081	−0·0069	−0·0022
	10	0·2547	+0·0295	−0·0020	−0·0061	−0·0015
B_2	5	0·2802	+0·0611	+0·0088	+0·0010	+0·0010
	7	0·2630	+0·0766	+0·0040	+0·0006	+0·0005
	10	0·2547	+0·0771	+0·0009	+0·0002	+0·0001
C_1	2	0·6129	−0·0123	−0·0057	−0·0030	−0·0057
	3	0·5582	+0·0029	−0·0031	−0·0025	−0·0025
	5	0·5201	+0·0369	+0·0041	−0·0004	−0·0012
	7	0·5087	+0·0622	+0·0125	+0·0022	−0·0007
	10	0·5031	+0·0872	+0·0206	+0·0063	−0·0006

risk is underestimated and the bias is positive. For more than one period the first period calculation is unbiased, and in subsequent periods the force of mortality is usually decreasing, so that the bias is positive. This is seen in Table 2, negative bias only occurring at $T = 5$ years in the model with the greater value of μ. It may also be seen that the bias is smaller than the corresponding value for continuous entry, and tends to decrease as T increases. This occurs because the unbiased estimate of τ_1 becomes more and more predominant in determining $E\{\hat{p}_T\}$ as T increases.

5.2. *Variance*

Table 3 lists the values of $N\,\mathrm{var}\,(\hat{p}_T)$ for the different actuarial estimators and also for the method of maximum likelihood, N being the total number of patients. It may be seen that the effect of increasing the number of sub-intervals is to increase the variance, but this effect is much less marked than that found by Littell. For example, at $p_T = 0{\cdot}4$ he found that the variance of the three-period estimator was twice that of

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the one-period and 1·22 times that of the two-period estimator. For the data presented here the corresponding ratios are 1·43 (A_1), 1·28 (B_1) and 1·05 (A_1), 1·04 (B_1) respectively. In fact in all cases, the loss of precision when the number of sub-intervals is increased from two upwards is so small as to be of little practical significance.

Comparison with the maximum likelihood variance shows that the one-period actuarial estimator would be more efficient if its bias could be ignored, while the other actuarial estimators never have a variance which is greater than the maximum

TABLE 3

N var $(\hat{p}_T)$ for different estimators
(N = total number of patients in sample)

Model	*T (years)*	*N var $(\hat{p}_T)$*				*Parametric maximum likelihood*
		Actuarial				
		One period	*Two period*	*Three period*	*Yearly*	
A_1	3	0·341	0·436	0·456	0·456	0·402
	5	0·333	0·451	0·475	0·493	0·436
	7	0·334	0·427	0·444	0·476	0·412
	10	0·277	0·395	0·419	0·440	0·401
A_2	5	0·278	0·294	0·335	0·344	0·302
	7	0·233	0·264	0·274	0·283	0·258
	10	0·199	0·232	0·237	0·238	0·227
B_1	2	0·319	0·392	0·407	0·392	0·352
	3	0·299	0·386	0·398	0·398	0·361
	5	0·258	0·348	0·356	0·357	0·324
	7	0·232	0·312	0·320	0·321	0·306
	10	0·208	0·269	0·280	0·280	0·267
B_2	5	0·196	0·238	0·250	0·254	0·243
	7	0·170	0·210	0·212	0·212	0·206
	10	0·161	0·194	0·195	0·195	0·192
C_1	2	0·316	0·367	0·377	0·367	0·338
	3	0·337	0·389	0·400	0·400	0·374
	5	0·341	0·379	0·380	0·385	0·357
	7	0·336	0·355	0·358	0·359	0·334
	10	0·329	0·329	0·334	0·328	0·316

likelihood value by more than 15 per cent., in marked contrast to Littell's results, where the "asymptotic efficiencies" of the three-period estimator are in the region of 0·5. This is not unexpected since in the present work the maximum likelihood method is being used to estimate three parameters instead of only one. Table 4 lists the maximum sample size for which the bias is less than 10 per cent. of the standard error of $\hat{p}_T$ for differential actuarial estimators. For sample sizes less than those shown in the table the effect of bias on the accuracy of the estimate will be small. The values can in fact be considered as figures of merit, the larger the value the more widely applicable will the estimator be in actual series of patients. The bias of the one-period analysis evidently makes it useless in practice; and when the entry is continuous the two-period analysis is also seldom applicable to reasonable-sized samples, i.e. > 50.

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Yearly grouping may be safely used on samples of most sizes met with in practice, except for early values of T, when further sub-divisions (e.g. into six-monthly periods) is evidently desirable.

For limited entry (A_2 and B_2), three periods, and sometimes only two, will be adequate, but this conclusion depends on the assumption that the estimate of survival rate for the first interval is unbiased, and this will not be the case if deaths from intercurrent disease are present.

TABLE 4

Maximum sample size for which the bias is $< 10\%$ of the standard error of $\hat{p}_T$ for different actuarial estimators

Model	*T (years)*	*One period*	*Two period*	*Three period*	*Yearly*
A_1	3	—	34	127	127
	5	—	28	116	640
	7	—	38	128	2,600
	10	25	72	181	8,000
A_2	5	—	4,600	190,000	500,000
	7	—	760	9,000	21,000
	10	—	940	11,000	28,000
B_1	2	—	18	82	18
	3	—	17	67	67
	5	—	24	67	300
	7	66	47	67	690
	10	—	670	75	1,200
B_2	5	—	31	2,500	2,600
	7	—	130	5,200	8,000
	10	—	2,400	49,000	300,000
C_1	2	21	115	430	115
	3	410	400	660	660
	5	—	220	20,000	2,700
	7	—	23	740	6,900
	10	—	—	84	8,000

6. TESTS WITH RANDOM SAMPLES

The results quoted above depend upon expressions for the variances which are only true if the sample size is very large. To test whether the conclusions might be influenced by applying them to small samples, calculations have been made on 40 random samples each of 100 patients, half of which were drawn up in accordance with model A_2, the other half with model B_2. Three tables of random numbers were used for compiling any one sample; one table for determining the month of first treatment within the four-year period, a second for determining the patients cured, and a third for allocating the survival times to the uncured patients. Boag (1960b, c) has drawn up samples of a similar nature. Estimates of five-year survival rate were made on samples 1–20 (model A_2) at a time 6 years after the first patient was treated, while estimates of three- and five-years rate were made on samples 21–40 (model B_2, in effect model B_1 for the estimate of p_3) at 4 years and 6 years respectively after the first patient was

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TABLE 5

Mean and variance of estimates of $\hat{p}_T$ from random samples

	Method of estimate	Parametric maximum likelihood		Actuarial								Product limit	
				One period		Two period		Yearly		Six-monthly			
		Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance	Mean	Variance
Samples 1–20	Samples	0·399	0·00363	0·348	0·00251	0·403	0·00288	0·405	0·00377	0·402	0·00397	0·402	0·00406
p_5	Expected	0·403	0·00302	0·349	0·00278	0·404	0·00294	0·403	0·00344	—	—	—	—
Samples 21–40	Samples	0·336	0·00436	0·390	0·00176	0·362	0·00388	0·345	0·00394	0·336	0·00482	0·328	0·00537
p_3	Expected	0·337	0·00361	0·379	0·00299	0·352	0·00386	0·345	0·00398	—	—	—	—
	Samples	0·287	0·00416	0·228	0·00190	0·283	0·00290	0·292	0·00351	0·293	0·00360	0·295	0·00351
p_5	Expected	0·280	0·00243	0·219	0·00196	0·271	0·00238	0·279	0·00254	—	—	—	—

Note:—The means and variances of the actual values of $\hat{p}_T$ in the samples are given below, the expected values of the variance of 20 samples being shown in brackets.

Samples	*T*	*Mean* $\hat{p}_T$	var ($\hat{p}_T$)
1–20	5	0·397	0·00286 (0·00239)
21–40	3	0·341	0·00367 (0·00224)
	5	0·291	0·00318 (0·00206)

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treated. The estimates were made by means of the one-period, two-period, yearly and six-monthly actuarial analyses, and also by the method of maximum likelihood using the method of calculation given by Boag (1948), but with the patients grouped in intervals of $\sigma/4$ instead of $\sigma/2$. The mean estimates obtained on each set of 20 samples by each method are listed in Table 5, together with the variances of these means.

The results bear out the general conclusions of the theoretical analysis, namely that:

(a) the variance of the actuarial analysis increases with the number of sub-intervals;
(b) the variance of the yearly actuarial method and the method of maximum likelihood are similar. (In samples 21–40, the variance of the yearly actuarial method is actually less than that of the method of maximum likelihood);
(c) the variance of the one-period actuarial method is less than that of the method of maximum likelihood, but that its bias makes it of little practical use.

A further point of interest was noted. The quantity p_T was also estimated in these samples by means of the product limit method described by Kaplan and Meier (1958). In this method the division points are chosen so that no interval contains both deaths and withdrawals. The estimate is then unbiased, but as may be seen by the results in Table 5 the variances of the estimates of p_5 in A_2 and p_3 in B_1 are further increased over that obtained by a six-monthly analysis. The more detailed computation involved in the product limit method does not therefore appear to be justified in cases similar to these. For the calculation of p_5 in B_2, the product limit variance is similar to the yearly and six-monthly. Reference to the arithmetic shows that, because of the small sample size and the few deaths from cancer in the period 2–5 years when withdrawals take place, the average number of sub-intervals used to obtain the product limit estimate was 7·8 compared with 4 and 7 for the yearly and six-monthly analyses respectively. On the other hand, in the product limit estimate of p_3 in B_1 the average number of sub-intervals was 17, as compared with 3 and 6 for the yearly and six-monthly analyses respectively. The use of the product limit estimate may therefore be preferable in small samples when the entry is limited so that most withdrawals occur during a period when the number of deaths from cancer is small.

7. Effect of Deaths from Intercurrent Disease

As has already been stated, in the case where entry for treatment is continuous throughout the period studied, deaths from intercurrent disease will be only a few per cent. of the total withdrawals. In the case of a limited entry, deaths will cause withdrawals during the first sub-interval and hence introduce bias in the estimation of τ_1, which so far has been assumed to be unbiased.

To assess the magnitude of this bias we may assume some law for the probability of dying from all causes other than cancer at the site in question in a normal population having the same age and sex distribution as the patients under investigation. (The assumption is also made that a person with cancer is neither more nor less liable to die from other causes than a person without cancer.)

The force of mortality from all causes tends to increase with age in an exponential manner (Gompertz, 1825), but a simpler law which will serve the present purpose is that deaths from intercurrent disease alone would cause an equal fraction, f, of the initial group of patients to die in every succeeding year after treatment, i.e. that $p_T = 1-fT$ if no deaths occur from cancer. This leads to a rising force of mortality with time, but not so rapidly as the exponential law found in practice. The extent of

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the approximation over a ten-year period may be seen in Table 6, which shows the fraction of the normal population in a given age group in 1946–50 dying from all causes of death within a subsequent period of ten years. The figures were computed from the values of ${}_5q_x$ quoted in McKenzie and Case (1957). The fraction dying in the period from the fifth to the tenth year is about 40 per cent. higher than in the first 5 years, but since the value of f is small, this discrepancy from a constant fraction law is not likely to affect the following discussion.

TABLE 6

Fraction of normal population dying from all causes within 10 years

Age group	*Sex*	*Fraction dying in period*		*Mean fraction dying per year*
		0–5 *years*	6–10 *years*	
50–	*M*	0·052	0·079	0·013
	F	0·033	0·043	0·008
55–	*M*	0·084	0·121	0·021
	F	0·049	0·068	0·012
60–	*M*	0·131	0·175	0·031
	F	0·077	0·109	0·019
65–69	*M*	0·190	0·236	0·043
	F	0·122	0·172	0·029

Suppose now that N patients are treated and all are followed up for at least T years, i.e. there will be no withdrawals in the period 0 to T due to limited follow-up. Using the same reasoning as before, it may be shown that the number of intercurrent deaths in the period 0 to T is

$$w_T = Nf\left\{cT+(1-c)\int_0^T q_{t'}\,dt'\right\}.$$

The deaths due to cancer in the same period are the total deaths during the period, i.e. $N(1-p'_T)$, less w_T, where p'_T, the proportion of the group alive at time T, equals

$$\{c+(1-c)q_T\}(1-fT).$$

w_T and p'_T may be computed for a given value of f, and hence the expected value of τ obtained from the expression

$$E\{\hat{\tau}\} = \{N(1-p'_T)-w_T\}/(N-\tfrac{1}{2}w_T).$$

Most groups of cancer patients have a mean age ranging from 55 to 65 years, and from Table 6 it may be seen that a value of $f = \cdot025$ is therefore fairly representative of the normal state of affairs. It should be noted that the table gives fractions dying from all causes, while f is the fraction dying from all causes other than cancer at the site studied, and f will always therefore be slightly less than the tabulated values. Values of the bias introduced by deaths from intercurrent disease when $f = \cdot025$, and the one-period actuarial method is used to estimate p_T, have been calculated for the

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three sets of parameters used in the models, and are listed in Table 7, together with the maximum sample sizes for which this bias would be less than 10 per cent. of var $(\hat{p}_T)$. These latter figures are to be interpreted in a similar way to those in Table 4. It is evident that in a number of cases, particularly when T is large, the bias precludes the use of a single period estimation except on small samples. Similarly the maximum

TABLE 7

Bias of one-period actuarial estimator when all N patients are followed for at least T years and withdrawals are only due to death from intercurrent disease. Figures in brackets are maximum values of N for which $B < 10\%$ of standard error of $\hat{p}_T$

Parameters as in model	*T (years)*				
	2	3	5	7	10
A_1	—	−0·0028 (330)	−0·0041 (150)	−0·0042 (140)	−0·0024 (400)
B_1	−0·0028 (320)	−0·0033 (210)	−0·0021 (500)	+0·0010 (1900)	+0·0071 (40)
C_1	−0·0008 (4000)	+0·0002 (50,000)	+0·0042 (150)	+0·0096 (28)	+0·0191 (7)

values of sample size quoted in Table 4 for models A_2 and B_2 using the one-, three-period and yearly actuarial method will be optimistic, and Table 8 shows the values obtained for the yearly estimator when account is taken of intercurrent deaths in the first interval (0 to $T-3$) at a rate determined by $f = 0{\cdot}025$/year.

TABLE 8

Maximum sample sizes for which $B < 10\%$ of standard error of $\hat{p}_T$ for yearly actuarial estimator applied to models A_2 and B_2, when withdrawals due to death from intercurrent disease may occur in first sub-interval (0 *to* $T-3$)

Model	*T (years)*		
	5	7	10
A_2	2,500	500	210
B_2	4,100	480	1,600

8. ALTERNATIVE METHODS OF ALLOWING FOR WITHDRAWALS

Because of the small variance obtained with the one-period actuarial method, it is tempting to consider the possibility of a substitution for the factor $\frac{1}{2}$ in the formula for $\hat{\tau}_j$ which would reduce the bias. Littell suggested for his model a linear function of $\hat{\tau}$, which reduces the bias considerably until $p_T < 0{\cdot}3$. No such possibility exists for the three-parameter model studied in the present work, because B is not a single-valued function of p_T, but depends on all three parameters μ, σ and c. There appears in fact, to be no simple alternative that is better than using the factor $\frac{1}{2}$, and a sufficient number of sub-intervals for the sample size being handled.

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Stocks (1950) advises that patients "lost sight of" in the jth interval should be regarded as only being at risk up to the end of the $(j-1)$th interval. A patient to be included in this group would be one who was examined and found to be alive and well, say 7 months before the date of the survey, and was not due for a further annual examination until 5 months later. If the last examination occurred x whole years after

TABLE 9

Bias of one-period actuarial estimator using two different approximations for effective number at risk

T (*years*)	*Model* A_1		*Model* B_1	
	Method 1	*Method* 2	*Method* 1	*Method* 2
2	—	—	−0·047	−0·021
3	−0·037	−0·015	−0·041	−0·006
5	−0·035	−0·003	−0·017	+0·025
7	−0·026	+0·013	+0·006	+0·059
10	−0·010	+0·031	+0·030	+0·064

Method 1: Effective number at risk $= N_j - \frac{1}{2}w_j$.

Method 2: Effective number at risk $= N_j - \sum_{k=1}^{k=w_j} \frac{T_j - T_k}{T_j - T_{j-1}}$

where T_k is the time at which a withdrawal occurs in the jth interval.

the patient was first treated, then the procedure when yearly sub-intervals are being used would be to omit this patient completely from the number at risk during the $(x+1)$th year. This method does not however avoid bias, because other patients, who similarly would have been lost sight of during the $(x+1)$th year if they had not died of cancer, may die from cancer in the $(x+1)$th year but before the date of the survey, i.e. they die in the first four months of the $(x+1)$th year. Since these patients are usually under almost continuous medical care during the last months of their lives, their deaths will almost certainly be known to the treatment centre at the time of the survey, and will be included in the record. Similarly the fact that the patient "lost sight of" for 7 months has not been seen since his last regular examination, almost certainly means he is alive, unless he has moved to another area. The quantity τ_{x+1} will therefore be overestimated unless, as suggested by Dorn (1950), the whole cohort who would, if alive, have withdrawn during their $(x+1)$th year are omitted from the calculation of τ_{x+1}, whether they in fact withdraw or die from cancer in that year. This method is unbiased, but leads to some information being rejected and the variance of the estimate being increased. The increase is greater for small values of T, and, when yearly intervals are used, the variances of p_5 for models A_1 and B_1 will be about 30 per cent. higher than the values listed in Table 3. For model C_1 the corresponding increase will be 20 per cent. When sample sizes are such that the bias of the estimate can be ignored, the procedure of taking the effective number at risk as $N_j - \frac{1}{2}w_j$ therefore seems preferable to Dorn's method. But for very large samples where the variance will be small in any case, the use of the unbiased estimator is more appropriate.

Boag (1948) in his example of the use of the actuarial method allowed each withdrawn patient to contribute to the number at risk during the interval in proportion to the fraction of the interval for which he was observed, e.g. a patient withdrawn at 3 months from the beginning of a 12-month interval counted as one-quarter of a

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patient at risk. This procedure also introduces bias, the extent of which is shown in Table 9 for one-period analyses on models A_1 and B_1.

Under these conditions the expected number to be deducted from N to obtain the effective number of patients at risk is given by

$$n\left\{\frac{cT}{2}+(1-c)\int_0^T q_{t'}\,dt'-\frac{(1-c)}{T}\int_0^T Tq_{t'}\,dt'\right\}.$$

To obtain the values in Table 9 the second integral of this expression was computed by approximate integration using a sufficient number of sub-divisions to ensure an accuracy of ·001 in B.

It may be seen that in some cases the bias is less and in other cases greater than that resulting from the $N_j-\frac{1}{2}w_j$ assumption, and the more detailed calculations involved in Boag's method do not appear to be warranted.

9. CONCLUSIONS

The investigation described in this paper has been restricted to the three-parameter model first suggested by Boag (1948), which has been shown by him and also by Sørensen (1958) to be a reasonable fit to the survival data of many treated cancer series. Consideration has also been confined to certain sets of values of the parameters which are representative of those found in practice. With the proviso, therefore, that the data do not deviate markedly from the model, or from the range of parameters studied, the main conclusions to be drawn from the results presented in the preceding sections are as follows:

(i) The actuarial method, in which the effective number at risk in a period is taken to be the number entering the period *minus* half the number of withdrawals during the period, results in a biased estimate of the T-year survival rate when the estimate is made $T+1$ years after the first patient was treated.

(ii) The bias in a one-period analysis is so large as to make such a simple estimation of little value.

(iii) The bias decreases as the number of sub-intervals used in the analysis increases, and for series consisting of a few hundred cases, division into yearly intervals will be sufficient to reduce the bias to less than 10 per cent. of the standard error, except at small values of T. For $T\leqslant 3$ years it is better to use smaller sub-intervals, say six-monthly.

(iv) For very large series where the variance of $\hat{p}_T$ is small, it is better to deal with withdrawals by omitting from the analysis in any one sub-interval the whole cohort of patients who would have withdrawn in that sub-interval, whether they do in fact withdraw or die from cancer. The estimate is then unbiased, and the increased variance can be tolerated.

(v) For samples in which the entry of patients is limited to a period of T_0 years ($T_0<T$), the period 0 to $T+1-T_0$ may be taken as the first sub-interval, and the remaining period from T_0 to T divided into yearly sub-intervals. By this procedure the bias is less than 10 per cent. of the standard error even when intercurrent deaths occur in the first sub-interval, provided that the sample size is not greater than the values shown in Table 8.

(vi) The variance of the maximum likelihood estimator is never less than 87 per cent. of that resulting from the yearly actuarial method. In view of the labour involved by the iterative procedure, and the fact that when estimations are made at early values of T the iterations often fail to converge unless σ is arbitrarily fixed (Boag, 1948,

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1949; Haybittle, 1959), the use of maximum likelihood seems to have little to recommend it in the problem of estimating T-year survival under conditions in which the actuarial method may be used. Maximum likelihood can, of course, be used to predict T-year survival rate in series where no patients have been followed up for as long as T years, and the actuarial method is inapplicable.

(vii) The product limit estimate will in general have a higher variance than other actuarial estimators, except in the cases where the proviso that no sub-interval should contain both deaths from cancer and withdrawals leads to comparatively few sub-intervals. This may occur with small groups of patients where the entry is limited to a period less than T. In these cases the product limit estimate can be the most suitable to use.

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I should like to thank Professor P. Whittle of the University of Manchester Statistical Laboratory for the advice and help he gave me while in Cambridge on a number of points that arose in the course of this work. I am greatly indebted to Mrs P. C. Williams for her assistance in drawing up the random samples and performing many of the computations thereon. My thanks are also due to Dr H. F. Freundlich of Bristol Royal Hospital, for arranging to have the maximum likelihood estimates on some of the samples done by an I.B.M. computer in the course of a programme of work he is undertaking in conjunction with Dr J. W. Boag. Finally I should like to thank Professor J. S. Mitchell for his interest and encouragement in this work.

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Reviews

PAGE

1.—*Kendall, M. G. and Stuart, A.* The Advanced Theory of Statistics. Vol. 2—Inference and Relationship . . 284
2.—*David, F. N. and Pearson, E. S.* Elementary Statistical Exercises 286
3.—*Tortrat, A.* Principes de Statistique Mathématique . . 287
4.—*Runnenburg, J. Th.* On the Use of Markov Processes in One-server Waiting-time Problems and Renewal Theory . . 288
5.—*Kopal, Z.* Numerical Analysis 288
6.—*Armitage, P.* Sequential Medical Trials 289
7.—*Maxwell, A. E.* Analysing Qualitative Data 289
8.—*Solomon, Herbert* (Ed.). Studies in Item Analysis and Prediction 290
9.—*Solodovnikov, V. V.* Introduction to the Statistical Dynamics of Automatic Control Systems . 291
10.—*Kauffman, A. and Cruon, R.* Les Phénomènes d'Attente—Théorie et Applications . 292
11.—*Krupp, Sherman.* Pattern in Organization Analysis: A Critical Examination . . 293
12.—*Petersen, William.* Population . 294
13.—*O.E.E.C.* Demographic Trends, 1956–76, in Western Europe and the United States . . 295
14.—*Moser, C. A. and Scott, Wolf.* British Towns: a Statistical Study of their Social and Economic Differences . . 295

PAGE

15.—*International Labour Office.* Family Living Studies: a Symposium . . . 298
16.—*Council of Europe and Overseas Associated Countries.* Statistical Data 300
17.—*U.K. Interdepartmental Committee on Social and Economic Research.* Census of Production Reports . . . 301
18.—*Peacock, A. T. and Wiseman, J.* The Growth of Public Expenditure in the United Kingdom . 302
19.—*National Bureau of Economic Research.* Public Finances: Needs, Sources and Utilization 303
20.—*Rees, Albert.* Real Wages in Manufacturing, 1890–1914 . 305
21.—*National Bureau of Economic Research.* Output, Input and Productivity Measurement: Studies in Income and Wealth 305
22.—*Schumpeter, Elizabeth Boody.* English Overseas Trade Statistics, 1697–1808 . . . 306
23.—*Sraffa, Piero.* Production of Commodities by Means of Commodities . . . 307
24.—*Klaman, Saul B.* The Postwar Residential Mortgage Market 309
25.—*Michal, J. M.* Central Planning in Czechoslovakia. Organization for Growth in a Mature Economy 310
26.—*Dewhurst, J. Frederic, et al.* Europe's Needs and Resources: Trends and Prospects in Eighteen Countries . . 311

1. The Advanced Theory of Statistics: Vol. 2—Inference and Relationship. By M. G. Kendall and A. Stuart. London, Griffin, 1961. ix, 676 p. $9\frac{3}{4}''$. 132*s*.

The book opens with three chapters on point estimation. Fundamental ideas are first developed, omitting the concepts of completeness and minimal sufficiency, which appear later in connection with composite hypotheses. The method of maximum likelihood is explained, with up-to-date proofs but a range of applications still strongly influenced by the spectacular advances of 1921. The section ends by discussing least squares at length

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and other methods in less detail. Potential users of minimum chi-squared will be discouraged by seeing it applied to find an awkward estimator of the Poisson mean.

Two chapters on interval estimation come next. In the first, on confidence intervals, there is a good discussion of situations leading to fragmentary intervals. Tolerance intervals are included, but simultaneous confidence intervals are deferred to Volume 3, and it is difficult to appreciate most selective intervals without a theory of testing hypotheses. About half of the chapter entitled Fiducial Intervals is taken up with confidence interval solutions of the problem of two means, and with Bayesian intervals. In the remainder, the authors are very helpful about the principles of fiducial theory, and describe the limitations which Lindley has placed on it by requiring it to be consistent with Bayesian methods.

There follow four chapters on testing hypotheses. After an introduction concerned with simple hypotheses, the modern theory of testing composite hypotheses is presented, the treatment leaning heavily on two papers by Lehmann and Scheffé. General results are derived for the likelihood-ratio method, and applied to testing linear hypotheses, non-central distributions appearing as required. This will prove essential reading for the statistical consultant. Methods for the comparison of tests, mainly concerned with asymptotic relative efficiency, conclude the section.

Four chapters on statistical relationship begin with defensive remarks on correlation. Several coefficients receive attention, some of them vintage specimens of essentially historical interest. The theory of partial and multiple correlation follows, Moran's derivation being given for the non-null distribution of R. After a treatment of the analytical theory of regression, the general linear regression model enters, together with standard procedures and a method of constructing confidence regions for an entire regression line. The field of functional and structural relationships is surveyed, and the failures of maximum likelihood lend added interest to the other methods presented.

A group of four chapters is largely concerned with distribution-free procedures. Tests of fit are comprehensively reviewed, and followed by tests for independence in a bivariate population, for randomness against trend alternatives, and for the hypothesis that two populations are identical. Permutation tests based on variate-values, on ranks, and on the expected values of normal order-statistics are all analysed with the authority conveyed by the authors' special interest in this field. Both non-parametric and parametric problems are solved by the use of individual order-statistics, and the section is completed by a chapter on categorized data. Coefficients of association and partial association are defined, and some of them are given probabilistic interpretations, but such reasons as the authors give for discussing them at length are unconvincing. A measure of association which is not a test criterion should now be regarded as superannuated. The chapter ends with a most careful discussion of procedures for the analysis of contingency tables.

A useful account of sequential methods brings the book to a close, but the authors deliberately provide nothing on the theory of decisions. On the last page, they leave the reader to make up his own mind on the extent to which statistics is the science of decision-making "with such aid from the theory of decision functions as he can get". This was a mistake. When all exaggerated claims on behalf of decision functions have been fully discounted, there remains a mathematical theory of some interest and, as the authors admit, cases where the losses are directly measurable.

The index covers 20 pages and the reviewer is informed that it took three weeks to prepare. It is truly valuable. In addition, there are 20 pages of references, tables of all the standard probability distributions, plenty of examples throughout the text, and over 400 exercises. The quality of printing can safely be compared with the highest standards available.

The bare facts of the book, listed above, make impressive reading. Its scope, its size, the reputations of its authors, and the existence of Volume 1, are by themselves a sufficient recommendation: it is clearly an essential work of reference for anyone seriously interested in the theoretical basis of statistical methods. On inspection, the whole work is seen to be characterized by an easy flowing style, which indicates the motivation of the argument

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without displaying undue haste and is from time to time illuminated by some memorable phrase. A consequence has been the occasional loss of complete mathematical rigour, but the reader cannot have it both ways, and the authors have contrived, by the exclusion of rebarbative reasoning, to produce a book which is eminently readable and likely to attract recruits to statistics. Only considerations of weight preclude recommending it for the bedside. Contrariwise, the problem of organizing the different topics has not always been happily solved, and the choice of subjects treated fully is sometimes disconcerting. There is a definite tendency to linger in the past, and the rigid exclusion of any theory of decision functions is, by contrast, unreasonable. A more ruthless policy of rearrangement would have eliminated sterile material on correlation and association which has unfortunately survived from the earlier Volumes 1 and 2. However, the balanced view must be that here is a book of outstanding importance, which surveys and integrates an immense field of knowledge. We are deeply indebted to the industry of the authors, and look forward with keen anticipation to the appearance of Volume 3.

R. L. PLACKETT

2. Elementary Statistical Exercises. By F. N. David and E. S. Pearson. Cambridge, University Press, 1961. viii, 108 p. $10\frac{1}{2}''$. 13*s*. 6*d*.

This book was originally published in 1952 by University College, London, when it was reproduced by offset-litho process from typescript. It is now available more generally in a more permanent form. It contains exercises in practical statistics, and covers the syllabus for the first year of the B.Sc. Special (London) examination in Statistics. It is intended to provide data for numerical work both for teachers and for students working alone.

The book is divided into three main parts. The first and by far the largest (82 pages) consists of 41 chapters, each devoted to a different topic with several appropriate exercises. The subjects covered include: calculation of moments; fitting of binomial, Poisson, negative binomial, normal distributions to given data; approximation to the binomial by the normal distribution; regression; use of the x^2-distribution; and significance tests on means and variances of large and small sample data. Part 2 consists of 42 examination questions from recent papers for the one-year Ancillary and the B.Sc. General examinations of London University in Statistics. Answers to Part 1 are given in Part 3.

Great stress is laid on the determination of sampling distributions, both by exact methods and by taking large numbers of small samples from given data. Five chapters are devoted to the investigation of the distribution of the quantity referred to as

$$\phi^2 = \Sigma \frac{(\text{Observed} - \text{Expected})^2}{\text{Expected}}$$

and its relation with the x^2-distribution. Many exercises are also devoted to curve fitting; this will illustrate to many students who tend to think otherwise that not all experimental distributions are binomial, Poisson or normal. On the other hand, less emphasis than is usual in elementary statistics courses is laid on the ideas of inference—in particular estimation and the testing of hypotheses.

The notes given in each chapter are intended as a guide to the methods involved in solving the problems, and would be used by the student as a complement to a text book. A teacher will find the book chiefly useful as a source of data for examples in his own statistics course, and will find that in many cases they can be used as illustrations of various techniques beside the ones given. The examples are chosen from a wide variety of fields ranging from a large number in anthropology to diverse industrial applications and the inevitable horse kicks in the Prussian army corps.

The inclusion of answers to the numerical examples is useful, and provides a great improvement (apart from a very few misprints) on the first edition. It is unfortunate, however, that these have been given to such a spurious degree of accuracy; e.g. the mean

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of 83 measurements stated to the nearest integer is given to six decimal places. Students starting numerical statistical work often find it difficult to determine the degree of accuracy to which answers should be stated, and some guidance could profitably have been given on this point. Apart from this, it is sometimes impossible to obtain the given number of places on the usual types of hand machine available to students.

The price is very reasonable and the book will be a useful addition to the library of anyone who is either teaching or studying elementary statistics.

SHIRLEY HITCHCOCK

3. Principes de Statistique Mathématique. By A. Tortrat. Paris, Dunod, 1961. xiv, 165 p. $6\frac{1}{2}''$. 16 *NF.*

There is an increasing demand in universities for short courses designed to introduce specialists in other fields to the basic ideas and methods of statistics. This monograph is based on such a course given by the author in the University of Paris. It appears in a series on scientific topics which includes Vajda's *Theory of Games and Linear Programming* and Yaglom and Yaglom's *Probability and Information.*

The first chapter introduces the reader to frequency distributions and their associated measures of location and dispersion, including methods of calculation. An unusual feature of the second chapter on the elements of probability theory is that it opens with a discussion of the idea of a random variable. This makes a more satisfactory link with the descriptive work of the first chapter but presupposes some knowledge of what a probability is. The normal, binomial and Poisson distributions are described in Chapter 3, but only for the latter are any practical examples of the occurrence of the distribution given and even then no data are provided. This is to be regretted because beginners often fail to see the point of studying these standard distributions. The two chapters on estimation and hypothesis testing are the best in the book. The logic of tests and confidence intervals is explained more carefully than is usual in books at this level. Although the field covered is necessarily limited, these chapters form a good introduction to more advanced works. The last chapter on "Correlation of two random variables" is less satisfactory. Presumably written with psychologists in mind, it includes a section on factor analysis (actually principal component analysis) and a fairly detailed discussion of correlation. Regression gets only a very brief mention.

The mathematical level is roughly equivalent to G.C.E. "O" level and a few proofs involving calculus are deferred to appendices. The tables at the end compress a remarkable amount of information into a very small space and there are several references to standard text books. In the latter the *Advanced Theory of Statistics*, Volumes I and II, is attributed to D. Kendall! Exercises are included at the end of each chapter but for the most part they are concerned with dice and cards rather than with practical problems. There is a short index which is far too brief to be useful.

The least satisfactory feature of the book is the poor quality of the printing of the mathematics. There are numerous slips such as $(x'-\mu)\,\sigma$ for $(x'-\mu)/\sigma$ on p. 62 and $\binom{n}{i}$ for $\binom{n}{i}$ on p. 63. Suffices are printed in type of varying size and on different levels—one at least as a superfix. Matters are made worse by the author's use of a bar over the symbol for a random variable to denote its expectation. It is thus extremely difficult to distinguish between $E(X^2)$ and $[E(X)]^2$ and between $E(X, Y)$ and $E(X)\,E(Y)$. In some cases, as at the bottom of p. 125, printing errors have made "confusion worse confounded". These points would cause little difficulty to an experienced statistician but seriously undermine the book's value for those to whom it is addressed. Teachers of statistics will find the book interesting and perhaps useful. English-speaking beginners would be better advised to use one of the well-tried introductory texts in their own language which can be obtained at the same price.

D. J. BARTHOLOMEW

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4. On the Use of Markov Processes in One-server Waiting-time Problems and Renewal Theory. By J. Th. Runnenburg. Klein Offsetdrukkerij Poorpers. N.V. 1960. 138 p. $9\frac{1}{2}''$. $3.

This book is the author's thesis for the degree of Doctor of Mathematics and Physics in the University of Amsterdam. Its main purpose is to extend results of queueing theory beyond the usual assumption that the instants of arrival to the system, the input process, form a recurrent process. The thesis is a detailed study of the case when the input process is a Markov process.

The first chapter is a useful summary of the book. The second chapter is devoted to a generalization of Lindley's theorem on the existence of a limiting distribution of waiting time in the single-server queue. An application of this generalization is given to a queueing system with "late arrivals", where the nth arrival is scheduled to occur at time $n\mu$ but may be late by an amount u_n where the process $\{u_n\}$ is a Markov process with $u_{n+1} \geqslant u_n - \mu$, this inequality ensuring that arrivals occur in their natural order.

The third chapter considers renewal processes where the successive life-times form a Markov process and contains a generalization of Feller's fundamental theorem on recurrent events.

The fourth and last chapter consists of two parts. The first part is an exposition of Pollaczek's method for the "first come—first served" single-server queue and the application of this method to derive various integral equations for the case of a recurrent input process. The second part of the chapter generalizes this method and is devoted to obtaining integral equations for the single-server queue with Markov input process. The solution of these integral equations leads to the generating function of the Laplace transform of the distribution function of the waiting time of the nth customer.

It would be unfair to criticize this thesis as if it were an exposition of certain aspects of queueing theory. It should be said, however, that the main emphasis is on existence theorems and not on results which can be used by those persons confronted by a practical queueing problem. For those interested in the mathematical aspects of queueing theory and related topics the thesis is stimulating and entertaining. The third chapter, in particular, points the way to some interesting problems and, as the author states, "Research in this direction has only been started as yet." We can look forward to the author's continuation of the research started in this thesis.

P. D. FINCH

5. Numerical Analysis: with Emphasis on the Application of Numerical Techniques to Problems of Infinitesimal Calculus in a Single Variable. By Zdenek Kopal. 2nd ed. London, Chapman & Hall, 1961. xvi, 594 p. $8\frac{3}{4}''$. 70*s*.

This is the second edition of a book which first appeared in 1955. One new chapter and an appendix have been added while the remainder of the book is substantially the same. The new chapter is entitled "Operational Methods in Numerical Analysis" and includes the material in one of the appendices in the first edition together with new results on rational function approximations yielding formulae for finite difference processes. A number of misprints in the first edition have been corrected, but several slips appear in the new material.

It would be nice to offer as warm a welcome to this new edition as the book received at its first appearance (for example, Tocher's review in this *Journal* (1956), **119**, 212), but it is necessary to be more qualified in the book's praise. There were criticisms of the notation used by the author and at the time of the first edition there were so few books on numerical analysis of the calibre of this one that a certain amount of latitude could be permitted. Since then the adoption of the notation for finite differences given, for example, in *Modern Computing Methods* (H.M.S.O.) has become much more widespread and students familiar with other texts will find the appearance of the formulae in Kopal's book strange.

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A valuable feature of the first edition were the bibliographical notes at the end of each chapter. In these, the author had given most useful suggestions for further reading; unfortunately, these notes have not been brought up to date. The author comments that he feels it is of much less immediate importance now to continue these notes when referencing and abstracting journals such as *Mathematical Reviews* are readily accessible. Useful as these sources are to the research worker, I cannot agree that they are as valuable to the advanced undergraduate or research worker requiring the assistance of numerical analysis as a suggested list of references selected by such an expert as the author. Guidance through the literature is even more necessary now.

In spite of these comments this book is still welcome because it contains one of the most complete accounts of numerical analysis for problems in differential and integral calculus in one variable.

E. S. PAGE

6. Sequential Medical Trials. By P. Armitage. Oxford, Blackwell Scientific Publications, 1960. [6], 105 p. $8\frac{3}{4}''$. 20*s*.

The sequential type of experiment has a special appeal for the clinical research worker since it complies much better with ethical requirements than a fixed sample size trial, and it needs little or no arithmetic. It is particularly satisfactory that Professor Armitage has now provided a sound text book which the clinician can use as a guide to the selection and interpretation of sequential experiments. There is plenty of room in the market for such a book and I know of no other which gives such a comprehensive and lucid account of the subject. Wald's book would undoubtedly put the clinician off for life, and the pamphlets of the Columbia University Statistical Research Group must now be difficult to obtain. Apart from these, sequential techniques are described only in isolated chapters of a number of statistical text books where it is difficult to deal with them in the detail they merit.

The field covered by this book is deliberately limited to sequential analysis of single experiments in which the results only regulate the number of observations and not the treatments applied in the course of the experiment. As Professor Armitage remarks, all experimentation is sequential to some extent, and it is clearly necessary to set some limit on the scope of an expository book for non-specialists. Within these limits the treatment is essentially complete, though, if they turn out to be of use in clinical work, I do not think that future editions should exclude the Robbins–Munro methods of stochastic approximation. Restricted sequential procedures, in which the maximum sample is fixed, are of considerable use in medical work and full details of a number of such designs are given.

Particular pains have been taken to discuss, in a non-mathematical way, the underlying theory of the methods and to point out their virtues and limitations and this is not a mere cookery book of recipes. It can be unreservedly recommended as a manual for the clinician and the statistician who requires an account of the subject without the theoretical details.

C. C. SPICER

7. Analysing Qualitative Data. By A. E. Maxwell. London, Methuen; New York, Wiley, 1961. 163 p. $7\frac{1}{2}''$. 16*s*.

In spite of all the criticisms which can be levelled against it, this book should quickly find its place on the bookshelf of many practising statisticians. Dr Maxwell aims at presenting material on χ^2 not adequately dealt with elsewhere (apart from the source papers).

The first two chapters outline the standard treatment of contingency tables. For the 2×2 tables, Fisher's exact test is also given, and for a more general contingency table, the exact mean and variance of χ^2 is given for use in small samples. Succeeding chapters deal with partitioning degrees of freedom and testing for trends in contingency tables, and the analysis of 2^n tables where the data are proportions. In a chapter on "goodness of fit",

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Cochran's results on testing linear functions of deviations are given. The last two chapters give an introduction to item analysis and classification procedures. Two other chapters deal with rank correlation and miscellaneous tests such as the sign test, Cochran's Q-test, etc. Numerical examples are worked for all methods described, and although none of the theory is given, the source papers are always listed, and at the end of each chapter is a short list of references for further reading. The book should satisfy a real need for some practising scientists (particularly psychologists) for a simple outline of the use of recent techniques.

A number of criticisms could be made, of which the most serious is the treatment of multiple contingency tables. Bartlett's test of the $2 \times 2 \times 2$ contingency table is not given. Dr Maxwell presents the analysis of 2^n tables using a model additive on the logit scale, and maximum likelihood estimation. However, the proposed method of solution follows the probit method, and the solution of the likelihood equations is a good deal more simple than Dr Maxwell says (see Anscombe (1956)).

The chapter on partitioning degrees of freedom in contingency tables uses Kimball's formulae which make the χ^2's additive. The reviewer considers that there is little point in especially arranging the χ^2's to be additive, since this usually results in degrees of freedom which (to say the least) are extremely difficult to interpret, and for which the calculations involve adding together frequencies which may be significantly different. Some critical discussion of this type would have improved the presentation.

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ANSCOMBE, F. J. (1956), "On estimating binomial response relations", *Biometrika*, **43**, 461–464.

G. B. WETHERILL

8. Studies in Item Analysis and Prediction. Edited by Herbert Solomon. Stanford, Calif., Stanford University Press, 1961. London, Oxford University Press. xii, 310 p. $9\frac{1}{4}''$. 70*s*.

This is a collection of research papers concerned with three main topics: theoretical statistical problems arising in psychological testing, the problem of discrimination (or prediction) with dichotomous variables and the distribution of the linear discriminant function when the variables are normally distributed. Most of the work has not been published before. There are altogether 19 papers by the following authors: R. Sitgreaves, G. Elfving, H. Solomon, H. Raiffa, R. R. Bahadur, P. F. Lazarsfeld, E. Paulson, M. V. Johns, D. Teichroew and A. H. Bowker.

In Part I Sitgreaves begins by investigating the properties of the reliability and validity coefficients in terms of a specified model. The so-called attenuation paradox is elucidated (that is, the fact that a reliability coefficient of one does not imply a validity coefficient of one). She goes on to suggest the use of a coefficient related to the mean square deviation of the predicted variable, which is far easier to interpret. She investigates the properties of this coefficient, and discusses the problem of choice of test items (in terms of the proportion passing them) to maximize the coefficient. The other papers in this section deal with the problem of the selection of items for predicting a criterion variable when the factor structure (i.e. the factors and their loadings) of the items and the criterion are known. The theory is developed in three papers by Elfving. The papers in this section are essentially theoretical in their approach. With reference to the earlier papers, it will in practice often not be clear that the relevant assumptions about the model can be made; with the latter, it is only rarely that the factors and their loadings are known. The papers should, however, be of considerable interest to anyone concerned with the theory of test design, and should provide a useful point of departure for anyone concerned with the related practical problems.

Part II represents a concerted attack on the problem of the analysis of dichotomous variables. In a paper entitled "The algebra of dichotomous systems" Lazarsfeld introduces a method of summarizing the 2^n results of n variables, using an extension of a measure of association for two variables to several. Much of his discussion is in terms of sociological

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results; no mention is made of the need for significance tests and he has a tendency to over-interpret his algebraic results. However, some interesting relationships are revealed, in particular, the effect of combining several values of one variable on the measures of association between the others. Bahadur presents a somewhat more sophisticated approach, and shows how the probability distribution can be expressed as a linear function of high order correlations and how an approximation to the distribution can be made. He also examines the relationship between the true distribution and that of the total score in terms of these correlations. He goes on to discuss the classification problem in terms of his coefficients, and obtains a measure of effective distance between two groups, and the type of approximation that can be made in some circumstances. This enables a comparison to be made with the efficacy of the usual linear discriminant function. This is in fact done in the following paper for a particular case with six items. The problem of item selection for prediction cannot be completely solved without the introduction of costs, risks, and prior probabilities. The two following papers by Raiffa and Johns respectively discuss the problem in terms of decision theory. The former analyses in detail the situation when there are just three items, and also discusses the selection of a few items from a large battery. In the latter paper, entitled "An empirical Bayes approach to non-parametric two-way classification" the prior probabilities are assumed unknown initially, but information about them is obtained from the sample. The last paper in this section, not connected with the others, is "A non-parametric solution for the h-sample slippage problem" by Paulson. The papers in this section are a valuable contribution to the subject, and are well worth studying.

The papers in Part III tackle the problem of finding the distribution of the linear discriminant function (called here the Wald–Anderson classification statistic W). Sitgreaves obtains a series representation when the two samples are of equal size; Teichroew and Sitgreaves describe a method of obtaining an empirical sampling distribution; Elfving gives a method of approximating to the distributions of Student's t and of W in the one-dimensional case; Bowker shows how Hotelling's T and W can be expressed in terms of simple statistics, and finally Bowker and Sitgreaves obtain an asymptotic expansion for the distribution of W using these results. Unfortunately the results of these papers are all rather unwieldy and more work will have to be done before the theory can be used to give guidance to a statistician who wishes to calculate the probability of misclassification using the discriminant function.

This collection of papers is prefaced by an excellent introduction by the editor. He discusses the implications of the models used and the results obtained. A somewhat tenuous connection is drawn between the three parts by pointing out that in Part I there is an underlying bivariate model, in Part II both test and criterion are dichotomous and in Part III the "test" variables are multivariate normal and the criterion variable dichotomous. Few readers will wish to study the whole book, but it will serve as a very useful reference for anybody interested in test construction and classification procedures.

MONICA A. WALKER

9. Introduction to the Statistical Dynamics of Automatic Control Systems. By V. V. Solodovnikov (Translation edited by J. B. Thomas and L. A. Zadek). New York, Dover Publications, [1960], [London, Constable]. xx, 307 p. 8″. 16*s*.

This book first appeared in Russian in 1952 and since this time the market has been flooded by a stream of American books on the subject, e.g. *Random Processes in Automatic Control* by J. H. Lanning and R. H. Battin and *An Introduction to the Theory of Random Signals and Noise* by V. B. Davenport and W. L. Root, to mention only two. Whereas these later books take the subject somewhat further than the book under review, it is clear that they have drawn very heavily for their material on this book. However, they lack the thoroughness and orderliness which characterize this Russian text and it is clear that it will remain a classic for some time.

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The editors mention in their introduction that the book in its original form contained "many examples of improper terminology and stylistic imperfection". One is forced to conclude that, even if this remark is correct, many further examples have been introduced during translation and editing. Thus, in Chaper 2, the interquartile distance is consistently called the median of a distribution and the mean deviation incorrectly called the arithmetic mean. This chapter is not of great relevance to the main theme, however, since it consists of an introduction to basic statistical and probabilistic concepts for the engineering audience at which the book is mainly directed.

Chapter 1 contains the most detailed exposition of the properties of linear dynamic systems that the writer has seen, so detailed in fact that the development almost becomes boring and mathematically turgid. It is clear, however, that nothing is left to the imagination of the Russian student and western book writers should take note.

Chapter 3 gives an excellent engineering introduction to stationary random processes—except for the problem of estimation of autocorrelations and spectral density from finite lengths of record which is treated in a very crude manner as it seems to be in all engineering texts of this kind.

The later chapters are concerned with the determination of the optimum transfer functions for what may be described as open-loop operations such as prediction, smoothing, and differentiation of signals in the presence of noise. Very little attention is given to closed-loop problems involving feed back. These chapters are very thorough also and the examples are worked out in extreme detail. The main criticism which one has to make of this book is that at no stage is one given any realistic case studies—except possibly the treatment of the effect of noise on a conical scanning radar in Chapter 3—and even this was taken from an earlier American text.

G. M. Jenkins

10. Les Phénomènes d'Attente—Théorie et Applications. By A. Kaufmann and R. Cruon. Paris, Dunod, 1961. xiv, 274 p. 9½″. 56 *NF*.

Everyone who is familiar with the vast literature on queueing theory will realize that it is impossible to give a comprehensive account which covers the whole field. Consequently, it is important to bear in mind the readers for whom a book is intended not only to decide on the degree of mathematical complexity but also to decide what they will want to know. The authors state that this book is written both for engineers and managers, and for mathematicians. It is intended to contain all the methods employed in the solution of queueing problems as well as examples to illustrate the type of problems which will be worth studying and amenable to solution.

The general mathematical foundations are laid in the first chapter. Topics considered include Markov chains, the Chapman–Kolmogorov equations, the Poisson process and the χ^2 distribution with an even number of degrees of freedom. This is followed by a short, general chapter about queues, in which their structure is discussed and the statistics of greatest practical interest are introduced. The next four chapters are all concerned with queues in which the arrivals form a Poisson process and the service time has an exponential distribution. In the first of these chapters, there is assumed to be a single server. The differential equation method is developed for the queue size distribution and the integral method is used to obtain the waiting-time distribution when the process is stationary. Chapter 4 contains an extension of the problem to the case of more than one server and in Chapter 5 the population is assumed to be finite. The majority of results only relate to the stationary process but the time dependent equations are included. There is a long, interesting discussion of linear stochastic networks in Chapter 6 but very few results are available. This is a topic on which the authors would obviously like to see more research effort devoted. Problems with more general inter-arrival-time and service-time distributions are considered in Chapter 7. Erlang's staging device and Kendall's imbedded Markov chain are both introduced and some further results using the integral method are derived. The

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effects of customers having priorities are considered in Chapter 8 and those of a customer baulking are briefly studied in Chapter 9. The simulation method of obtaining a numerical solution when it is impossible to obtain an analytical solution is discussed at great length in Chapter 10. The section on generating random variables from a given distribution is disappointing; the obvious rather than the best methods are given. There are 18 nomograms and many references.

The authors' material is logically presented and carefully written, with few misprints, so that the book is very readable. However, many non-mathematicians will find difficulty with some chapters. For mathematicians this book is a good introduction to queueing theory but, with the possible exception of the chapter on systems of queues, the theoretical research worker is unlikely to find the book useful because much recent work is ignored.

The book is excellently produced but the high price may deter many prospective purchasers.

A. MERCER

11. Pattern in Organization Analysis: A Critical Examination. By Sherman Krupp. Philadelphia, New York, Chilton Co., 1961. xiv, 201 p. 8″. $5.

This work, written in the American language, "is a study . . . in some current theories through which the business organization is perceived. . . . It is not the behavior of the business firm that is sought, but the theories about this behavior".

From the point of view of organization theory, a firm is a social system with decision-making processes directed towards a definite goal. The author distinguishes this theory from economic analysis, which relates the firm to the surrounding economic structure, and from scientific management (F. W. Taylor) and administrative design theory, which derive from an earlier management science. Organization theory "with roots in logical positivism and the behavioral sciences" considers the firm as a self-contained unit and is more closely connected with social theory and with the human-relation-in-industry approach. Seen from any angle, the aspect will be restricted, and organization theory is subject to the criticism that it considers the firm in isolation from its environment. It stresses the themes of harmony and of cooperation and has thus "reduced sensitivity to the process of power in the distribution of social-economic resources that power brings about" (p. xii). Also, it "neutralizes the economic and social problems of bigness" (p. 184).

Chapter 3, entitled Relations in Industry and the Concept of Equilibrium, contains a critical analysis of the study of the manufacturing plant of the Western Electric Company in Hawthorne, carried out from 1927 to 1932 and concerned with the effect on output of single changes in the physical environment. The purpose of the study was the investigation of cooperation, and "harmony, an equilibrium condition for the plant as a social system, characterized the desires, goals and behavior of the operatives" (p. 44). What this means is made clear by the disclosure that during the investigation two girls, less cooperative than others, were replaced by managerial action. The bias introduced by such devices needs no stressing.

In Chapter 4, The Boundaries of a Theory, the author classifies the composition of a theory, or of a model, according to five categories: expository (e.g. describing the equilibrium position), analytic (e.g. labour theory of classical economics), empirical (stressing the legal and other boundaries of a firm), inductive (by laboratory experiments) and normative (susceptibility to control).

The gist of the examination of organization theory is contained in the next four chapters: (5) Organization Theory and the Theory of Administrative Design, (6) Organization Theory as an Expository and Analytical Framework, (7) Content and Behavior: Small Group Theory, (8) Content and Behavior; Simon and March. Here an evaluation is made of the teleological bias in the work of Mary Parker Follett in the 1920's and of the more recent work of Chester I. Barnard, James G. March and Herbert Simon. The small group theory of George C. Homans is criticized on the grounds that by isolating a

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group from its surroundings a great part of its life is ignored and that a bias towards social harmony is artificially introduced. A more fundamental criticism of all theories of organizations is the fact that "values penetrate theoretical systems in the social sciences . . . values circumscribe a theory, theories order observations, and these observations and theories reflect and reinforce these values".

In the last chapter, Authority, Conflict, Power and Resource Allocation—Conflicting Perspectives, it is suggested that a theory dealing with the concepts mentioned in this title rather than with equilibrium and stability might prove valuable.

S. VAJDA

12. Population. By William Petersen. New York, The MacMillan Company, 1961. xx, 652 p. $9\frac{1}{4}''$.

This is an account of changes in the size and structure of the United States population and of the principal determinants of these changes. There is also a great deal of information of population development in other parts of the world. To some extent this has been done before, for example by the Taeubers, Bogue, and Kingsley Davis. But if the author did only this he would have the advantage of lucid style and a capacity for bringing history to life. However, the book goes a great deal further than this. Though the strictly methodological content is small, efforts are made to describe the influences determining populations in such a way as to introduce the reader to the fundamental concepts of population statistics and of sociology. In following the author's narrative the reader finds himself mastering the language and recognizing the tools of the demographer.

Mr Petersen's principal task is to show how population phenomena intrude on our everyday life and in doing so to help us to understand what is happening in our own society and in others, what are the important population problems which society needs to solve and the methods that might be employed to solve them.

Some of the author's classifications are difficult to follow. The distinction between formal demography as the "gathering, collating, statistical analysis and presentation of population data" and "population analysis" as the "systematic study of population phenomena in relation to their social setting" seems entirely artificial. The separation of emigration from immigration as the third and fourth main causes (after fertility and mortality) of population change is not a simplification since the two are only different in sign and often operate simultaneously so that only the balance can be measured. The distinction between "early Western" and "modern Western" population types is an over-simplification since bounds cannot be set to distinct stages in the continuous development of industrialized society. Certainly it is impossible to accept that "early Western society is exemplified by England". Other points of criticism are the over-emphasis of the violent discontinuities in totalitarian societies which are dwarfed by the larger historical movements (there is, for example, a quite unnecessary horrific photograph of British troops bulldozing a heap of dead bodies into a grave at Belsen), an apparent obsession with Malthus, and rather scant attention to socio-economic differentials. The author has biases and needs to be read critically.

The book successfully attempts a wider coverage of population movements and the underlying determinants than any of its predecessors; it ranges from U.S. immigration policy to the ethnic composition of the U.S.S.R., from the industrial revolution in Britain to the Second Bolshevik revolution, from urban U.S.A. to rural India, from historical growth to future projections, and from reproduction rates to life tables. Though so concentrated it makes interesting and easy reading. An extensive bibliography is provided at the end of each section for those who may wish to make a deeper study of particular aspects. The book itself, in paper, printing, presentation and binding, is an attractive production.

B. BENJAMIN

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13. Demographic Trends, 1956-76, in Western Europe and in the United States. Paris, O.E.E.C., [1961], [London, H.M.S.O.]. 151 p. 10½″. 12*s.* 6*d.*

In 1956 the O.E.E.C. published a study on *Demographic Trends in Western Europe 1951–1971*. This study was resumed in 1959 and followed closely the methods used in the previous one. A questionnaire was sent to member and associated countries asking for a report of an analysis of differences between the actual population trends and the figures given in the earlier projections indicating to what extent these differences arose from variations in fertility, mortality or migration. The questionnaire also called for two kinds of projections regarding future population, the first being based on mortality and fertility alone without regard to migration; the other showing the effect of migration on the natural development and structure of the population. In addition the countries were asked to show the population of working age and to indicate likely variations in activity rates in the future as the result of changes in school attendance, variations in the age of retirement, migration, changes in the employment of women or from other causes. The replies have been brought together in a comprehensive report which was prepared by Mr Louis Henry of the National Institute for Demographic Studies in Paris. The main conclusions are that the population in all countries except Ireland will increase from 1956 to 1976. The increase in the male population of working age will on the whole be less than the increase in total population. In the aggregate the population of working age should become slightly younger and differences between countries should be reduced. The tendency is not general and in Spain, Ireland, Italy and Portugal the population of working age is older in 1976 than in 1956. In 1976 the number of old people per thousand persons of working age is higher in the aggregate than in 1956 in every country except Turkey. In 1976 the number of young people per thousand persons of working age is higher in the aggregate than in 1956 but the difference is appreciable only in Germany, Austria, Belgium and Spain. There is a fairly appreciable decline in the Netherlands, Portugal and Sweden whilst elsewhere there is little variation either way. Little migration has been forecast apart from that expected from Ireland and Portugal.

B. BENJAMIN

14. British Towns: a Statistical Study of their Social and Economic Differences. By C. A. Moser and Wolf Scott. Edinburgh and London, Oliver & Boyd, [1961]. xii, 169 p. 10″. 30*s.* (Centre for Urban Studies, Report No. 2.)

Towns are no more homogeneous than the people of which they are composed. It is natural, therefore, that sociologists should extend their study of the structural patterns of society to that of the developmental and environmental patterns of the urban communities into which British society is aggregated. Have we any right to speak of the urban dweller, the urban way of life unless we can discern a typical dweller or typical way of life to give substance to these expressions? A Centre for Urban Studies was established at University College London in 1958 with the avowed object of contributing to the systematic knowledge of towns, especially of British towns, of studying urban development, structure and society, and of linking academic social research with social policy. The Centre has a priority interest in identifying urban characteristics as a means of classifying towns in distinct groups; in finding out the ways in which towns resemble or differ from each other. The report under review should command the interest of all those who are concerned with town planning and administration. For the characteristics observed are the end effects of urban growth, and the starting points for future change. The report is necessarily heavily statistical, but it is readable and the middle part is an essentially simple treatment of the wide range of variation of towns not only in size and growth but also in housing, jobs, consumer demand, economic conditions, political interest and educational achievement.

The task of reducing this mosaic of urban complexes into some kind of order that will permit on the one hand of summary description of towns as groups and on the other of

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profitable study of factors influencing urban profiles is formidable. Before the question "What makes Little Mudford what it is, and so different from Great Fordmarsh?" can be answered, there has to be some way of describing Little Mudford and all the other towns in some standard language. The language must be objective; it must be in terms of characteristics that are not only measurable but have in fact been measured. The statistician in this field can rarely command original data specific to his requirements on a sufficiently general scale. More often he must take the world as he finds it and make the best of such statistics as are available from the census or from vital registration or other general sources. The authors have assembled, for the main analysis, some 57 variables. Seven of these cover sex, age and marital condition distribution; 8 are concerned with population growth and birth rates; 15 relate to household size and housing conditions; 14 variables cover industrial activity, occupational groupings, journey to work and retail sales; there are 5 voting statistics; 6 variables relate to mortality or sickness; 2 are concerned with education. "The central idea of this study has been to unravel the relationship between a host of urban characteristics, and measure them precisely, rather than to study in detail any single feature."

What do the authors mean by "relationship" and why is it so important? Why, indeed, go to all this trouble? (The calculations involved would be too formidable without the aid of an electronic computer.) The reason is quite simple. Any one of the 57 indices could be used to group together towns, similar in this one report, but this same grouping would mask implicit similarities in other respects because the variables are not independent of one another. If towns with high proportions of professional and managerial workers are selected they will tend also to be towns with low density housing, low infant mortality, high educational standards, etc. Where the association between two indices is very strong then grouping by one is the same as grouping by the other and, to say the least, it is a waste of time to carry out the procedure since the second exercise adds no information to the first. What the statistician tries to do is to find out how much really new information can be gleaned by referring to additional variables and in order to do this he must ascertain the degree of independence of variables or of groups of variables. It is much more economical and far less perplexing to classify on four or five really independent axes than to unravel a tangled skein of associations with a multiplicity of interrelated variables. But it is this ascertainment of independence of variables that makes all the work and calls for somewhat complicated statistical techniques. And where, as in this case, the variables are not tailor-made but have to be selected from a whole battery of available indices then, arduous or not, the task is all the more imperative. When it has been performed the results are easily understandable. The authors have been able to present their main findings (tentatively in view of the pilot nature of the study) in about half a dozen pages, and these are in the first chapter.

There is another important advantage to be gained from this kind of study. Quite apart from the determination of the "independence" of indices, the analysis helps in the evaluation of the meaningfulness of indices. In deciding which is the best index of, say, overcrowding the statistician has to rely upon his experience and judgement. This judgement is likely to be greatly improved by this kind of study of the behaviour of indices. One gets a better feel of their content and significance.

The study covers 157 towns in England and Wales with populations in 1951 of 50,000 or more. They include all but 3 of the 80 County Boroughs, 64 Municipal Boroughs, 12 Urban Districts and the Administrative County of London, and in all they embrace slightly over half the population of England and Wales. It is important to remember that by "town" we mean "local authority area". One great deficiency of British statistics is the lack of any statistics of towns as integral communities; that is, of the populations of distinct urban clusters of dwellings (street formations or scattered buildings which are not separated by more than a specified distance) representing localities or groups of people with a measure of social and economic interdependence in their daily lives. Such localities tend in their development to leave local authority boundaries behind and a single locality

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may sprawl across more than one local authority area. Even with a very rough identification of such urban clusters it was found at the 1951 Census of England and Wales that 320 local authority areas contained only 148 distinct communities in this sense. Here again, therefore, the statisticians have to "make do and mend" with local authority areas since all census and vital registration statistics are based on these administrative units. All this injects heterogeneity into the picture and makes the tangled skein more tangled; for every town is on the average only half a community. The necessity for this compromise seems even to have driven the authors to cloaking it with virtue. "Continuity of bricks, lime, and playing fields is irrelevant for any meaningful definition of a town. The better approach is one of functional analysis, treating one complex at a time." This appears to suggest a preference for treating parts of a conurbation as a primary rather than a secondary unit of classification and runs the risk of pre-judging the results of any analysis of clusters as whole units. In a densely populated country like England and Wales clusters are necessarily large (more than one-third of the whole population is concentrated in the six major conurbations) but their size does not altogether defeat their population unity. As a primary basis of identification it is size which is irrelevant not continuity of bricks and mortar. The latter is a primary criterion though it may be overridden at subsequent stages of analysis. The Hydra must be recognized as a unit before its many heads are separately contemplated.

This is not just an aside; it is matter critical to the choice of variables made by the authors. Two important variables introduced by the authors—the job ratio, first developed by Ruth Glass, i.e. the ratio of the population working in the area to the occupied population living there, and the commuting ratio, i.e. the ratio of the gross journeys to work across the local authority boundaries to the occupied resident population—are relevant to the very fact that the local authority area does not embrace the whole of the economically interdependent community. Some part of this dormitory movement does extend beyond the built-up area to other built-up areas separated by open country. Thousands of Londoners travel to the East and South coasts to sleep (they have little time to do more) and in countries with labour shortages, e.g. Western Germany before the refugee flood, the commuting from cluster to cluster can be substantial. But in general in England and Wales the commuting outside clusters as distinct from local authorities is inconsiderable.

Taking towns as they find them and variables as they find them, the authors attempt to "discern a systematic pattern for all, or groups of, towns both in the common and contrasting elements". They do this by "component analysis", the mysteries of which are lucidly explained by means of an interesting diagram. In brief, and speaking loosely, the technique consists of recognizing mathematically the most pronounced areas of overlap (components) and, within these areas, the most significantly contributing variables (to give a label to the component). Four main components are identified: (1) social class (a term here reflecting all variables obviously related to socio-economic conditions or occupational structure such as infant mortality, educational attainment); (2) population change between 1931 and 1951 (an index of age of the town); (3) population change between 1951 and 1958 (an index of recent development or redevelopment); (4) housing density (overcrowding, small houses, poor water supply, etc.). These components permit the following classification of towns (number of towns in brackets):

1. Mainly resorts, administrative and commercial
   (a) Seaside resorts (10)
   (b) Spas, professional and administrative centres (10)
   (c) Commercial centres with some industry (16)
2. Mainly industrial towns
   (a) Railway centres etc. (14)
   (b) Ports etc. (10)
   (c) Textile etc. (16)
   (d) North-east seaboard, mining towns of Wales (11)
   (e) More recent metal manufacturing etc. (14)

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3. Suburbs and suburban type
   (a) Exclusive residential (9)
   (b) Older type of mixed residential (12)
   (c) Newer type of mixed residential (11)
   (d) Including light industry (9)
   (e) Older type working class and industrial type (10)
   (f) Newer type of industrial
4. Not allocated (London A.C. and Huyton with Roby).

Since this classification may offend local pride (who wants to admit to living elsewhere than in an "exclusive residential" district?) let it be emphasized that this classification is experimental and illustrative. But it is automatic. It was virtually made by the computer after the raw material of indices had been supplied to it; the criterion of classification (though not the labels) emerged from the analysis itself.

The analysis shows that though Chigwell has nearly the lowest proportion of the population in the working age range and Carshalton nearly the highest; while Chigwell has a less than average and Carshalton a higher than average proportion of the population housed at more than 1½ persons per room; both are ranked low for job ratio (i.e. they are dormitories), low in the provision of local shopping centres, low for mortality, high for proportion of males in professional and managerial occupation, high for pursuit of higher education and low for voting left at elections. They have much more in common (as accepted newer residential suburbs) than in contrast. This much, it may be argued, can be seen without refined analysis. Such simple cases are unfortunately rare. There are many other pairs of towns for which the distinction between what is in contrast and what is in common is not so evident, and the separation of sets of towns—the typology—certainly could not have been achieved without this patient and arduous study. The authors modestly regard their work as a reconnaissance in depth; future studies will undoubtedly be stimulated and facilitated as a result.

Looking ahead to more advanced studies it is possible to speculate on the uses to which the typology might be put. The design of social policy is bound to be improved by the achievement of a nice balance of regard for the individual character of a town and its kinship with others; the planning of local services and indeed the adjustment of local authority boundaries must take into account not only current growth but also the history and function of the town and the general level of living. There are many spheres of social study—town planning, communication, epidemiology, market research, education—where research has been hampered by a lack of classification of different types of conditions sufficient to permit the study of changes of social structure following different kinds of action. As this is written the Ministry of Housing and Local Government Report for 1960 has been issued. It makes the 12 New Towns a special topic and complains of a lack of neighbourliness in what were intended to be new neighbourhoods. A suggested remedy of denser housing must be empirical until the hypothesis has been tested. In what kinds of town is neighbourliness at a high level? What kinds of town are there? So we are back to urban differentiation.

In this country four out of every five live in towns. Though many take part in the frantic effort to escape to open country, the magnitude of the rush defeats its object and the open areas rapidly become filled. This is a crowded island. Most of us have not too few neighbours but sometimes too many. We must come to terms with the situation and the social problems which it poses. A bold programme of study such as Moser and Scott have begun is the ineluctable prerequisite of any sufficient solution.

B. BENJAMIN

15. Family Living Studies: a Symposium. Geneva, International Labour Office, 1961. [4], 280 p. 9½″. 15*s*.

In recent years there has been much discussion in meetings of statisticians organized by the United Nations and the specialized agencies of the concept of levels of living and

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the indicators that can be chosen to measure changes in these levels. Many of the indicators suggested at one time or another have not stood the test of critical examination, but nobody has doubted that a considerable part of the information required in order to make some assessments of levels of living must come from surveys. Within the division of labour applicable to international agencies, family living surveys fall to the International Labour Office and this symposium is described as "one of a series of I.L.O. contributions to the study and development of family living studies". It follows discussions at the I.L.O's Conference of Labour Statisticians and the report of a working group of experts who reviewed the purposes and objectives of family living studies. An introduction written by I.L.O. staff is followed by 15 chapters each describing specific surveys. The contents are well summarized in the introduction from which the following is a quotation (except that authors' names have been substituted for chapter numbers):

> "Six of the chapters in the Symposium describe family budget surveys, that is studies concerned primarily with family income and expenditure patterns. These include two classical family budget inquiries in industrially advanced countries, namely an ad hoc survey in Sweden (von Hofsten) and a continuous survey in the United Kingdom (Fowler and Moss); a continuous budget survey in the U.S.S.R. (Postnikov); a family budget survey in Greece, a less industrialised country (Reisz); a study of family budgets in agriculture in the Federal Republic of Germany (Kötter and Lunze); and a regional survey concerned with international comparisons in the countries belonging to the European Coal and Steel Community (Wagenführ).
>
> "A second group of chapters relates to specialised family living studies other than family budget surveys: a food consumption survey in Great Britain (Hollingsworth and Baines), a survey of underemployment in Puerto Rico (Jaffe), a population study in the Mysore State of India (Chandrasekaran), a health survey in Japan (Soda), and repeated surveys of consumer finances in the United States (Morgan).
>
> "The third series of chapters deal with family living studies of a more comprehensive type, or multi-subject surveys. These studies include a survey undertaken for the appraisal of community development programmes in the Philippines (Bantegui), the Current Population Survey in the United States (Eckler), a household sample survey of levels of living in a region of the Ivory Coast (Boutillier and Causse), and finally the National Sample Survey in India (I.L.O. staff)."

The effort that the I.L.O. staff and the contributors (most of whom are hard-pressed Government officials) have put into this volume has been well spent. The chapters are well balanced and, as a whole, they provide a very good survey of a fairly wide range of enquiries. There is always too little written by busy people on how they tackle their problems and it is an advantage that many of the contributors have been frank about their lack of success in some aspects of their work. Descriptive material of this kind, especially as alternative methods are discussed in some of the contributions, is of great value to those who have the responsibility for initiating new surveys or extending the scope of surveys undertaken in the past. As material for students and "apprentices" in survey organizations it is excellent.

One would wish for an introduction rather less coloured by cautious generalizations, but a much more important unfulfilled wish is for greater knowledge of the National Sample Survey of India. The chapter on Indian experience planned by the editors could not be written "because of unforeseen circumstances" and the most they could do was to prepare a summary based on official Indian documents, particularly one published towards the end of 1952. This is a regrettable gap because those planning surveys in under-developed territories have much to learn from Indian experience, including the problem of absorbing the mass of material collected in multi-purpose surveys. A lesser gap is that recent experience in Africa is represented by one chapter only.

Each of the chapters in this volume could be discussed like a paper at a meeting of the Society and most of them make a reviewer wish they could be treated in this way. The

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outstanding impression remaining after reading the whole volume is the efficiency of surveys that are continuous (although changed gradually) and made flexible by the inclusion of special subjects from time to time. A start must be made, however, and conditions are not always favourable for continuous surveys even though the lack or shortage of comparative figures for past periods gives a relatively small return for all the energy that is poured into *ad hoc* surveys.

"The Continuous Budget Survey in the U.S.S.R." describes aims and methods of approach different from those with which most of us are familiar and many readers will be facing for the first time the problems of international comparisons discussed in the chapter on the budget survey made by the European Coal and Steel Community. Amongst the specialized family living studies, the one on underemployment in Puerto Rico is an extreme example of the complexity of the social conditions under study; hence the likelihood that each investigator will seek a somewhat different solution.

Finally there is nothing in the symposium capable of dispelling doubts concerning the advisability of a multi-purpose enquiry planned as a single survey. It is to be hoped that the Philippines' study of community development has achieved the status of a continuous survey.

W. F. Searle

16. Statistical Data: Population, Employment, Agriculture, Industry, Trade, Transport, External Trade, Finance. (Member Countries of the Council of Europe and Overseas Associated Countries.) Strasbourg, Council of Europe, Documentation Section, 1961. cxviii, 459 p. 9½″. 30*s*.

This bilingual volume consists principally of a series of tables giving, for recent years, the data on population, employment, trade, finance, prices, etc. for the 15 countries forming the Council of Europe, data taken from the existing statistical yearbooks of the U.N., I.L.O., F.A.O., O.E.E.C., etc. Where the data are in absolute numbers (and in some cases when the data are percentages) a total is given for Council of Europe countries as well as one for the six countries of the European Economic Community and in some cases for the seven countries of the European Free Trade Association (called here the E.F.T. *area*). This is done for those "qui ont besoin de données générales pour mésurer les dimensions réelles de l'Europe et de ses problèmes". Since the data are not complete for all these countries and for each year and since the scope of the data often differs from country to country, this object is only partially attained. In some of the tables it is stated that the figures should be treated with reserve but no attempt is made to make these aggregates more comparable and it is doubtful if, for some of the tables, much value can be attached to aggregates which are doubly incomparable.

The volume, however, is marked by a large number of errors in transcribing, in spelling, in translation (from the French) and in incorrect or incomplete footnotes and references to sources. A perusal of the tables on population, employment and wages alone reveals a number of errors. In Table 222 the figures for the Netherlands consist in part of data for Malta; in Tables 232 and 258, the data for the Federal Republic of Germany are for West Berlin and Eastern Germany respectively; in Table 254 the figures for Norway are those of Poland. It is easy to check how these errors have arisen. The copyists (called in the preface "the compiling statisticians") have taken the adjacent line above or below the true figures of the original source given in alphabetical order of countries. In Table 236, the "Council" total is given as 72,500 instead of 61,500. The most inexcusable errors are in the section on wages (called "salaries"), which are given in currency units: the figures for the United Kingdom and Ireland are at first sight incomprehensible. We read, for the United Kingdom, in 1953, that wages were:

Building industry	"£.s.d. *per hour* 3/9/7"	(Table 257)
Transport industry	"£.s.d. *per day* 3/6/2"	(Table 258)
Textiles industry	"Shillings, pences" [sic] 3/9/8	(Table 260)
Agriculture	"*per week*" 14/2/6	(Table 263)

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The correct figures are:

Building	3s 9·7d per hour
Transport	3s 6·2d ,, ,,
Textile	3s 9·8d ,, ,,
Agriculture	142s 6d ,, week

Tables showing time series are described as "trends in . . .", whether or not they show any trends; no trends are in fact computed. Even Table (39) on the *level* of employment in 1958 (1953 = 100) is headed "Industrial employement [sic] trends". The titles of the sources are often incorrectly quoted, e.g. Yearbook of *International* Accounts Statistics (instead of *National*), and Annuaire *Statistiques* (instead of *Statistique*).

It is to be hoped that these signs of hasty preparation, and bad (or absence of) proof-reading and supervision will be remembered in the next edition.

J. W. Nixon

17. Census of Production Reports. U.K. Interdepartmental Committee on Social and Economic Research. London, H.M. Stationery Office, 1961. vi, 86 p. 9¾″. 5*s*. (Guide to Official Sources, No. 6.)

This report, published in September 1961, has been prepared by the Board of Trade as a guide to the statistics published in the Census of Production reports and Inquiries carried out under the Import Duties Acts. A historical survey is included showing how the Census of Production has developed since 1907. Chapters describe the coverage of the Census reports; sampling techniques are discussed and a chapter entitled "Bibliography of Census of Production Reports" gives a useful contents list for each Census.

Chapters 5 and 6, showing the coverage of each Census by industry and by subjects, are particularly valuable, and will save much of the time one has had to spend in looking among the plethora of information available for detail relating to particular topics. These two chapters alone would justify the publication of the report. The appendix covering the bibliography of associated publications suggests that the Royal Statistical Society is one of the few bodies that really considers these Censuses critically.

This publication is factual and clearly written, and will obviously be useful, but it suffers from being subject to the usual official attitude which is clearly expressed in the paragraph discussing the Advisory Committees, where the following sentence appears: ". . . there can be little doubt that it is beneficial for the Board, before embarking on these extensive and sometimes complex inquiries, to explain and justify their proposals to a group of people with a knowledge of business, but who for the most part are not statistical experts." It is noteworthy that the emphasis is on the Board explaining and justifying their proposals to the outside world, and not on the Board trying to appreciate the needs of the outside world.

The practice, common to railway and air lines, and increasingly to statisticians working for the various sections of the U.N., of putting asterisks and suffixes into tables and giving the reader an obstacle race to find their meaning, is adopted in some of the tables in this report.

The long delay in the publication of the Census of Production reports is a subject on which industry feels strongly, and one looks in vain for any information about the timing of publication.

The secrecy guaranteed for the Census under the Statistics of Trade Acts surely does not cover the Register of Establishments mentioned on p. 28 "which is now being developed to provide an up-to-date record in terms of other statistical units . . .", i.e. other than the Register of Establishments, provided by the Ministry of Labour. This Register can obviously be of considerable value to a wide range of economists and statisticians but "the information contained in the census register cannot be made available outside the Government service". No explanation is given for this inability.

S. V. Cunliffe

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18. The Growth of Public Expenditure in the United Kingdom. By Alan T. Peacock and Jack Wiseman, assisted by J. Veverka. National Bureau of Economic Research. Princeton, University Press, 1961. [London, Oxford University Press.] xxxii, 213 p. 9″. $5.

The study covers the years 1890 to 1955, surely one of the most dynamic periods in history. Not the least outstanding feature of the period is the growth of public expenditure. The economic aspect of the behaviour of the public sector does not readily lend itself to treatment by the conventional methods of economic analysis, a defect which becomes more serious with the growth in scope and volume of governmental activities, and attempts to resolve this difficult situation will be generally welcome. The study makes a contribution towards this resolution. Its specific aims are to reveal the statistical facts about British Government expenditure, to relate it to the history of the period and to offer such explanations of its behaviour as can be discovered.

Charts and tables illustrating the growth of public expenditure show its extreme irregularity, an irregularity which persists irrespective of whether the figures are shown in money terms or in real terms, in total or per head of the population. More than anything else they show how the peak expenditures of the two world wars have been followed by permanently raised levels of peace-time expenditures in the subsequent periods. These higher levels of peace-time expenditures seem to have their origins in a mixture of war-time and pre-war factors; in some cases, such as the servicing of war debt, they are inevitable, while in others they are desirable (and may have been so for many years) and have become possible in the light of current notions about taxable capacity which have been revised as a result of war-time experience. In this way an explanation is offered for the tendency of large-scale social disturbances such as major wars to shift public revenues and expenditures to permanently higher levels, a tendency which the authors call the "displacement effect".

The next generalization to emerge from the study is the "concentration process", a term used to designate the forces which tend to shift responsibility for public expenditures from local to central government. Although these shifts have occurred at various times, the defences of local autonomy tend to be breached most seriously at times of large-scale social disturbance, hence the displacement effect is an important factor in the timing of the concentration process.

By discussing the concentration process chiefly in terms of shifts in responsibility for expenditure from local to central government the authors have restricted its field of influence. In an "applied" study of this character it may have been the most fruitful course, but the wider theoretical approach is more satisfying. By a shift of emphasis it is possible to view the concentration process as a basic theory covering all transfers to higher authority. It would then embrace every shift from the private sector to the public sector and every shift in the public sector from a lower to a higher organ of government. Indeed, it is sometimes difficult to separate these shifts; the hospital service was shifted partly from private enterprise and partly from local government. The theory can be extended further; contemplation of N.A.T.O. and the European Common Market together with the possibility of further developments in international cooperation suggests that some surrender of national sovereignty may become an accomplished fact, that the authority of supra-national organs will grow and that the concentration process will result in the transfer of other functions upwards from the central government itself.

From the theoretical point of view the displacement effect is an unattractive feature, being the outcome of a mixture of factors including irrationality in human behaviour and weaknesses in human institutions, with which theoretical methods are least suited to deal. Apparently, tremendous pressures are needed to push our form of democratic government from the easy path of lethargic conservatism on to the rough road where latent social demands become effective. A parallel study covering between 20 and 25 other countries now being undertaken by the authors will probably show the same phenomenon for other

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western democracies since it is, after all, merely a reflection of the imperfections in markets where vague directives such as those of public opinion and the ballot box take the place of clear-cut specific monetary demands.

The fact that the treatment of public corporations is, for all practical purposes, isolated in Chapter 7 is a compromise. On the one hand the authors wished to bring them into the public sector, partly for the sake of completeness and partly in order to complement an earlier study entitled *The Growth of Public Employment in Great Britain* by Moses Abramovitz and Vera Eliasberg, while on the other hand they acknowledge that the gross payments of government trading bodies cannot be included in government expenditure. The desire to include the public corporations may have been misconceived. There was no significant transfer of industry to public ownership after the First World War (when, indeed, the coal mines and the railways were handed back to private enterprise), so that they are a weak illustration of the displacement effect. Moreover, at least one recent event has served as a reminder that the government's control over nationalized industries is not absolute, so that they are not an ideal example of the concentration process in the wider sense of the term mentioned above. Since the government can finance and subsidize any industry, whether privately or publicly owned, a case can be made out for omitting public corporations from the public sector in principle as well as in fact, and merely treating the loans, grants and subsidies paid to them as part of the government's expenditure on economic services, the subject matter of Chapter 5. For the purpose of this study the provision of capital for the nationalized industries by government loans under Section 42 of the Finance Act, 1956, instead of by the issue of stock may be more significant than the original acts of nationalization. The borderline between the public and private sectors is not clear cut; there are degrees of government intervention, so that it might be more useful to adopt a definition of the public sector appropriate to each specific problem rather than to adhere to a unique definition for the sake of uniformity.

Government expenditure in each of the main functional and economic categories is dealt with in some detail and this paves the way for a consideration of possible future trends in government expenditure. The basis for this projection consists of a series of explicit assumptions which, at the very least, offer a good starting point for discussion and, indeed, seem to be very reasonable in the light of present knowledge. The occurrence, timing and magnitude of social disturbances cannot be forecast, but in the absence of such events, something useful can be said in a general way about future trends in public expenditure. Broadly, it is concluded that the scope for the expansion of government expenditure in Great Britain in the next quarter century may be much greater than is commonly believed. The authors insist that this conclusion should be treated as no more than an interesting possibility but, in spite of their caveats, readers may consider this chapter to be the most significant part of the study.

There are numerous charts and tables bringing together the statistics of public expenditure between 1890 and 1955, and an explanatory text covering sources and definitions is also provided. The agricultural subsidy figure for 1971 given in Table 23 and the date of the penny post quoted on page 122 are presumably editorial slips.

T. M. Ridley

19. Public Finances: Needs, Sources and Utilization. A Conference of the Universities—National Bureau Committee for Economic Research. A Report of the National Bureau of Economic Research, New York. Princeton, University Press, 1961. [London, Oxford University Press.] xiv, 512 p. 9″. $10. (Special Conference Series, 12.)

The economic problems of government provision of goods and services have always seemed to fit uneasily into the framework of "traditional" Pigovian welfare analysis and it is only with the revival of interest in the works on public finance of Wicksell and Lindahl

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(which only the latter lived to see) that economists have examined more closely the economics of collective decision-making through the political process. Once it is accepted that the political process is a worthy alternative to the price system as a method of expressing choices about the use of resources, some very important problems arise about the expression of these choices in quantifiable form. How is a valuation to be made at the margin between defence and education expenditure? Are there suitable criteria for deciding on the amount and composition of public investment and how do these differ from the criteria considered relevant in the private sector? Once standards of public provision for any service are laid down, how can costs of provision be minimized? These and other tricky problems of the same sort are the subject of this exceedingly interesting book which records the conference proceedings attended by several of the best-known American experts on public finance.

The first set of papers is concerned with projections of U.S. government expenditure. Gerhard Colm and Manuel Helzner, for example, consider the effect of a full employment rate of growth on government expenditure. Growth will depend on growing government expenditure on environmental services. It will also cause growth in public expenditure itself because growing real incomes will call for improved standards of provision, for example, in public health and social security benefits. A projection for 1970 yields a considerable rise in real *per capita* expenditure on government, but a fall in the percentage of government expenditure to G.N.P. from 28·5 to 25·9 per cent. It is obvious that in a growing economy the built-in flexibility of the tax system will take care of revenue resources to meet the absolute rise in government expenditure, but a major policy problem revealed by the projection is that the rise in government expenditure will be much greater at the State level, and State revenue yields are unlikely to be elastic enough to meet the rise in expenditures.

A second set of papers deals with the vexed question of the "proper" division of economic functions as between the Federal and State governments and with the efficient execution of these functions once the appropriate organization has been chosen. Richard Musgrave's highly ingenious attempt to "mathematicize" the various possible types of federal grants to lower levels of government deserves particular mention, as does Julius Margolis's paper on Metropolitan finance problems which shows clearly the conflict between the difficulty of overcoming local resistance to the financing of improved services and the reluctance to see these improved services financed entirely by higher levels of government.

The delay in publication of the conference proceedings has meant that the third set of papers which covers programme evaluation in government must be reviewed alongside the extensive literature on the subject which has poured forth over the last year or so. The simulation of the price system in evaluating government services is obviously difficult if the utility of these services to the legislature cannot be quantified and the services themselves are by their very nature indivisible. Roland McKean sees a "second best" solution in the improvement of presentation of defence estimates so that they reveal the approximate cost of "end-product" programmes. Although Otto Eckstein's survey of the theory of public expenditure criteria deals with a wide range of problems, particular interest in this country should attach to his discussion of the use of "planning" rates of interest in valuing public investment programmes. As he shows quite conclusively, there is nothing in economic analysis which proves that rates chosen by planners are "superior" to those of the community at large who will probably seek quick returns to investment. As he remarks, in underdeveloped countries "early benefits may have morale effects which yield extra production and generally add momentum to the development effort". In other words, the "myopia" of the general public may not necessarily be a hindrance to the general aim of the planners themselves.

A catalogue of only a few contributions does less than justice to the parade of useful techniques, perceptive analysis and realistic judgement displayed in this volume, and to the high standard of the recorded discussion. It needs emphasizing that discussion of similar

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problems in the United Kingdom has a long way to go before it will be raised to the level found in these contributions.

ALAN T. PEACOCK

20. Real Wages in Manufacturing, 1890-1914. By Albert Rees, assisted by Donald P. Jacobs. A Study by the National Bureau of Economic Research. Princeton, University Press, 1961. [London, Oxford University Press.] xvi, 163 p. 9″. $5.

This slim monograph will be of interest to a wider audience than those concerned with the immediate object of the investigation on which it reports. The study grew out of an enquiry into trends in U.S. wages and productivity and is a typically careful product of the National Bureau of Economic Research. Past enquiries had indicated a puzzling temporary break in the persistent upward trend in U.S. real wages. In the quarter of a century preceding the First World War real wages seem to have been essentially stationary. A similar phenomenon was deduced from the real wage estimates for other countries—the United Kingdom was a striking example. A variety of explanations—none of them conclusive or complete, and some inconsistent with each other—have been advanced for this curious international stagnation in real earnings. The authors set out to reconsider the statistics.

It is surprising, in view of the hazardous character of all historical estimation, how long it takes for suspicion to fall upon the statistics themselves. The laborious business of building up new approximative (never definitive) series from scratch discourages the hardiest analyst. It is much easier and more exciting to accept the series published by reputable scholars and to look for ingenious explanations for their unexpected implications. So the false trails tend to be well worn and the faulty series to be continually sanctified by printed repetition.

The villains in this tale are the price statistics. The authors recalculated the relevant wage series for the U.S. in considerable detail and found that, although there were differences in level, in year-to-year movements and in industrial components between their new series and the most careful previous estimates, the discrepancies in trend were negligible. When they came to process the price statistics, however, they found that their new cost of living index yielded a much smaller rise than any of its predecessors and that this muted deflator converted the apparent stagnation in real wages into an appreciable rise. Their cost of living index has its deficiencies but it has effectively demolished the case for stagnation. No one who has tried to deflate historical series will be surprised by this dénouement. It remains to explain away the more plausible explanations for the apparent stagnation!

It remains also to reconsider the statistics for the other countries where this phenomenon has been observed—in particular to ask whether the stagnation in United Kingdom real wage rates which has been attributed to "the late nineteenth century climacteric" stands up to a re-examination of the basic statistics. This would make a tidy project of research in itself. Meanwhile, however, we are justified in regarding the familiar deflators with sharpened suspicion. It is worth bearing in mind, for example, that whereas the Bowley cost of living index shows a rise of nearly a third between 1900 and 1913, the price index derived from Prest's study of consumers' expenditure rises by under 9 per cent. over this period.

P. M. DEANE

21. Output, Input and Productivity Measurement: Studies in Income and Wealth. Vol. 25. By the Conference on Research in Income and Wealth. A Report of the National Bureau of Economic Research. Princeton, University Press, 1961. x, 506 p. 9″. $10.

This book is concerned with theoretical and practical problems involved in measuring output, the input of labour and capital and their productivity. It is not concerned with

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"input–output". It contains 14 papers that were prepared for the 25th Conference on Research in Income and Wealth held in New York in October 1958, together with comments of some of the participants. A very useful introduction to the book is provided by John Kendrick, who also edited it.

The papers included in the book cover a wide range of topics. For example, papers read by Irving Siegal, George Stigler, Kenneth Boulding and Richard and Nancy Ruggles deal with methodological and conceptual problems; those by Neal Potter, Francis Christy, Jnr., Leon Greenberg and Almarin Phillips are devoted mainly to reviewing various data on output, employment and productivity already available in the United States; and papers by V. R. Berlinguette and F. H. Leacy, and by Jack Alterman and Eva Jacobs, present new estimates of the national product in real terms based on output data.

The paper by Berlinguette and Leacy (both of the Canadian Dominion Bureau of Statistics) presents estimates of the Canadian product in real terms by industry of origin, and that by Jack Alterman and Eva Jacobs (both of the Bureau of Labor Statistics) presents similar estimates for the United States. Both papers contain a useful discussion of the conceptual problems involved in making the estimates; much of it, however, would not be new to people working in the field in Britain. Both papers give estimates of net output at constant prices for different industries obtained by the "Geary", or "double deflation", method. An interesting sidelight on this is provided by Almarin Phillips who has developed Kuznets' original suggestion that net output can be directly deflated by a price index formed by taking a weighted average of the difference between a gross output price index and an input price index. In his paper, he demonstrates that this method would theoretically give the same results as that obtained by deflating separately both output and input but, because of statistical imperfections in the data, the results differ in practice. Both the Canadian and the United States real product figures seem to reconcile very well with official estimates of the gross domestic product at constant price derived by revaluing the various items of final expenditure. So far, the Canadian and United States real output estimates have not been published with the official national income figures.

Milton Gilbert and Wilfred Beckerman in a useful paper discuss some of the theoretical problems involved in making international comparisons of national products. They argue that international differences in taste are probably less important than differences over long time periods in the same country. They feel that conceptual limitations of inter-country comparisons of national product are no greater than those which apply to comparisons over time and are not great enough to affect seriously the order of magnitude of the results.

In another paper, Edward Denison discusses the measurement of labour inputs. He argues that man-hours worked are a better measure than employment of the real cost of labour in terms of disutility, and that man-hours is the appropriate input measure for deriving measures of the productivity of labour. However, he considers employment to be a better measure than man-hours of effective labour input in terms of the contribution labour makes to production, as output per man will vary less than output per man-hour with changes in hours at all points above 30 or so a week.

Although several papers in the book are devoted to reviewing American statistical data and probably would not be of very much interest to British readers, most of the papers should prove to be useful to students and workers in the field in the United Kingdom.

L. S. Berman

22. English Overseas Trade Statistics, 1697–1808. By Elizabeth Boody Schumpeter. Oxford, Clarendon Press, 1960. vii, 72 p. $9\frac{3}{4}'' \times 13\frac{3}{4}''$. 45*s*.

This book consists of 47 tables relating to the foreign trade of England (Great Britain towards the end of the period), together with an outstandingly useful introduction by Professor T. S. Ashton. The tables will undoubtedly be of extreme value to students of English economic growth in the eighteenth century. Indeed, the ultimate aim of Mrs

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Schumpeter was to use the voluminous trade statistics of that period in helping to analyse the early process of economic growth in the first industrial nation, but she first found it necessary to bring some useful order to the official statistics of trade. Trade at that time was recorded separately for each English port, subdivided by nearly 40 countries, colonies or regions with which that trade was carried on. Under each country, etc., the trade was recorded in an alphabetical list of commodities. This arrangement, as Professor Ashton points out, was determined partly by the fiscal regulations of the time, and partly also by the current interest in the balance of trade with particular countries. It is, however, of little use for economic analysis requiring a study of changes by commodity rather than country, since commodity totals could be arrived at only by a very large number of additions. This labour has been performed by Mrs Schumpeter and, with this volume, a comprehensive set of commodity statistics of English foreign trade in the eighteenth century is now available to all students of the period.

It is by now well known that the early statistics of English foreign trade were recorded at fixed official valuations, so that they represented volume, not value, movements. The recording of exports at values declared by the exporter was adopted in 1798, while for imports and re-exports, declared values were not used until the later nineteenth century. One would assume that any volume series lasting for a century or more would be distorted by the use of a single set of weights, particularly if those weights were fixed, as in this instance, at the beginning of the period. Professor Ashton refers to some unpublished calculations by Mr W. A. Cole which showed that "between 1703 and 1798 the volume of exports increased fourfold and that of imports to almost the same degree", whether trade was valued at prices of the beginning of the period or at those of the end. This is, on the face of it, a surprising result and the publication of Mr Cole's detailed calculations would be welcome.

Professor Ashton discusses the main qualifications to the figures with considerable insight and authority. Particularly useful to intending users of the figures are his remarks on under- and over-declaration and on smuggling. Over the eighteenth century as a whole, unrecorded imports of smuggled goods may have amounted to as much as one-fifth or one-sixth of total imports and, of course, they were heavily concentrated on goods subject to a high rate of duty, like silks, brandy, wine, tea, coffee and spices.

Many of the commodities traded at that time have an unfamiliar ring to present-day ears. Frize, perpets, and kerseymeres, for example, are included in the list of exports of woollen goods, as well as penistones frized and penistones unfrized. It would have helped the reader unfamiliar with the details of eighteenth-century commerce to have had a glossary of such terms. Again, several of the quantity units shown are obscure in meaning. How much, for example, was a goad, the unit used for "Cottons Kendal" and "Cottons Welch plains" (both of which were, apparently, made of wool)? How much, too, was a bag of Spanish wool imports or a fodder of lead and shott exports? A conversion table showing present-day equivalents of bygone quantity units would greatly assist the interpretation of many of the series.

But these are small criticisms of presentation to set off against a work of lasting benefit to students of the history of British foreign trade. Though Mrs Schumpeter died before she could use the series she had compiled in an economic analysis of early industrial growth in Britain, she has undoubtedly provided a major statistical tool for others to use in this task.

A. MAIZELS

23. Production of Commodities by Means of Commodities. By Piero Sraffa. Cambridge, University Press, 1960. xii, 99 p. 10″. 12*s*. 6*d*.

This highly abstract work examines the determination of prices in a stationary self-reproducing economy under certain simplified assumptions about production and distribution. The economy turns out a number of products, some at least of which can be

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used either for consumption or as means of production. Enough means of production are produced each year and used the following year to ensure an unchanging annual rate of output.

The author begins by assuming a multi-industry subsistence economy in which each industry produces a single product, the whole output of which is used as means of production either in the industry producing it, or, as a result of exchange, in some other industry. Subsistence consumption by labour is treated as a means of production. Prices of all products are determined simultaneously by exchanges between industries so as to restore the original distribution of resources and to enable the process to be repeated indefinitely.

In the second stage, output is assumed to be more than enough for replacement purposes, yielding an overall surplus. Just as uniform prices are assumed to emerge from the system, so now a uniform rate of profits on the resources used is determined simultaneously with prices by the process of exchange. The total surplus is assumed to be divided, more or less arbitrarily, between wages and profits. Labour now appears as a means of production instead of subsistence, the latter merely constituting the limit below which wages cannot fall.

Output is always assumed to be unchanged. The only thing that is assumed to change is the distribution of the surplus between wages and profits. To begin with, wages are treated as the variable and assumed to vary between nil and 100 per cent. of the surplus, profits, which constitute the remainder, changing inversely. The question then is: how do these changes affect relative prices? This depends on the proportion of labour (receiving wages) to means of production (yielding profits) not only in the production of the commodities in question but also in the production of the means of production used in their production, and so on *ad infinitum*. Relative prices must change so as to "redress the balance" (p. 15) and restore uniformity of profits as between different industries.

The same question of the effect on relative prices of a change in the distribution of the surplus between wages and profits is treated again by a different approach, with profits taken as the independent variable and assumed to be determined from outside the system, e.g. by the money rate of interest (p. 33). Means of production are reduced to dated labour incorporated in their production (reminiscent of the Austrian theory of capital) and prices are determined by the cost of the labour applied at various dates compounded for the relevant period at the given rate of profit. A rise in the rate of profit implies a reduction in the cost of labour (wages) at each stage but an increase in the rate at which it is compounded (profits).

Not every commodity enters into the production of all other commodities. Those that do are described as basic, and only these enter into the mutually price-determining part of the system.

Prices are expressed in terms of a composite standard commodity, made up by combining all industries producing basics in such proportions that every commodity appears in the same ratio among the inputs as among the outputs. This combination is called the standard system. The price of the composite standard commodity is unaffected by changes in relative prices resulting from changes in the ratio of wages to profits: it therefore provides a conveniently invariant standard of value in which to express wages or the national income. The ratio of all the outputs to the inputs in the standard system gives its rate of surplus, and is the same as the maximum rate of profits in the actual system (i.e. the rate of profits when wages are zero).

In the third stage, more than one product is assumed to be produced by each industry. This results in an excess of unknowns (prices) over equations (industries) and a further assumption has to be introduced that there are alternative processes (additional equations)

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by which the same combination of products can be produced, though in different proportions: the alternative processes can be combined in such a way as to yield the desired proportions of the products.

The use of fixed capital is brought in under the multiple product system by treating depreciated fixed equipment as part of the output of the industry using it, produced jointly with its output of finished goods.

"Land", defined as a means of production not produced by the system, is simply a non-basic commodity, not entering into the price determination of the rest of the system. Its own price depends on its "scarcity", the better qualities, being more "scarce", getting more rent; only those processes using marginal "no rent" land enter into the general price-determining system. Obsolete machines which are still used but no longer produced are treated as "land".

The main part of the book (Parts 1 and 2) is concerned only with the question of how prices are affected by changes in the distribution of the surplus between wages and profits. Part 3 concentrates on the assumption that more than one method is available for producing each product and is concerned with the question of how the profitability of different methods (and hence the replacement of one by another) is affected by changes in the rate of profit.

Such is the general subject-matter of the book. As far as this reviewer can see it is a logically self-consistent system: the only question to be asked is how relevant the basic assumptions are for elucidating problems in the real world.

The assumption of a God-given unchanging rate of output seems peculiarly unreal, particularly once a surplus of outputs over replacement needs is postulated. In the ordinary economist's mind this immediately raises the question of how much of this surplus will be consumed and how much will go to net investment to expand output for the future. Mr Sraffa on the contrary assumes that however great the surplus, and however it is distributed between wages and profits, it is always 100 per cent. consumed.

Similarly, what most people would regard as the important question of what determines the proportions in which the surplus is distributed between wages and profits is simply conjured away by assuming that either wages or profits are varied by a wave of the wand, and that the other has to vary inversely.

Demand appears to play no role in the determination of prices. This is because everything that is produced that is surplus to replacement needs is simply assumed to be demanded for consumption purposes, and to be demanded in exactly the proportions in which it is produced. The possibility that people might wish to consume some of the products needed for replacement is ruled out of consideration as producing an unviable economic system (footnote p. 5). The opposite possibility that at least some of the surplus is devoted to net investment is not even mentioned in a footnote. The effect of changes in taste or techniques on relative prices is of course also ruled out by assumption. The only thing that is allowed to change is either the rate of profits or wages: nothing else.

Mr Sraffa's logic seems to be impeccable. It is the relevance of his assumptions to the real world and the interest and usefulness of the particular questions he aims at answering that are likely to leave most practical-minded economists dissatisfied.

M. F. W. Hemming

24. The Postwar Residential Mortgage Market. By Saul B. Klaman. National Bureau of Economic and Social Research. Princeton, University Press, 1961. [London, Oxford University Press.] xxxi, 301 p. 9″. $7.50.

This report continues the series of books produced by the National Bureau of Economic Research on postwar developments in the United States' capital market. With the aid of 51 tables and 36 charts, Dr Klaman traces changes in the volume and structure of mortgage

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debt on American housing between 1946 and 1956, analysing the source and destination of funds, fluctuations in interest rates, the gross and net flows of capital and their relation to developments in other sectors of the capital market.

British readers should begin with the last three chapters which present a lucid description of the history, policies and functions of the financial institutions involved. The American system of housing finance is a product of the operations of life insurance companies, mutual savings banks, commercial banks, mortgage companies and savings and loan associations. The latter associations, holding a quarter of the outstanding mortgage debt, are the nearest equivalent to our own building societies. This market depends (to an even greater extent than our own) upon the policies and resources of government. Unfortunately for the British, the author assumes a wider knowledge of American public administration than most of us possess. The development of the Federal National Mortgage Association is briefly traced, but the vital contributions of the Federal Housing Administration and Veterans Administration, though frequently mentioned, are never explained in detail.

Armed with some knowledge of this institutional framework, the reader can then turn to the opening summary of findings and four other chapters which present detailed statistical analyses of trends and phases in the postwar mortgage market. Students of housing economics and the capital market will find a great deal to interest them in these data.

They and others may regret, however, that Dr Klaman presents no general conclusion or comment on the social, political or administrative implications of his research. He describes the "policies" of lenders in detail. But when government intervenes on a grand scale to finance or guarantee the operations of lenders, it would be interesting to be told something, however speculative, about the types of borrowers who benefit from these policies. Do they differ in age, family structure or income from those who would be served by a "free" market? Are the traditions of this market, devised originally for a privileged few, appropriate for the larger numbers of people who have since gained access to it? It can be assumed that the borrowers want a house to live in: do they also want to acquire a major capital asset, and when (around the early 1970's) vast numbers of them acquire this asset, how much purchasing power will be released for other purposes? What implications does this method of house purchase have for the design and layout of housing estates in which every inch of land must be allotted to an individual purchaser?

Dr Klaman has done an outstandingly useful job demanding a great deal of arduous labour, and it would be unfair to ask much more. But since he must now have a better grasp of his complex and important material than anyone else, it is to be hoped that he will explore some of its implications—possibly in publications that are not subject to such awe-inspiring procedures for inspection, approval and the exclusion of controversy as those of the National Bureau of Economic Research.

D. V. DONNISON

25. Central Planning in Czechoslovakia. Organization for Growth in a Mature Economy. By J. M. Michal. Stanford, Calif., Stanford University Press, 1960. xii, 274 p. 9¼″. 46*s*.

Michal's meticulous commentary on the Czechoslovak statistical abstracts for 1957–1959 bears little relation to his title (on his final page we are told that an examination of planning technique "has not been the aim of the present book"). He seeks to give his study a wider significance by comparing the economic efficiency of a centrally planned economy and of a market economy "when a high level of industrialization has been achieved" (p. 3), but he effects this by straightforwardly juxtaposing output, manpower, productivity and consumption statistics for Czechoslovakia and 10 other countries, and by listing some of the elements which an investigation of Czechóslovak factor efficiency would require (pp. 45–46 and 223 and Tables 2.11 and 10.5). His conclusion is certainly wrong that a *per capita* rate

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of growth (1948–1957) equal to those of Austria, France and Western Germany has been derived from much greater inputs of both labour and fixed capital assets (p. 238); French expansion was much slower, as was that of its inputs, but Austria and Germany obtained the Czechoslovak rate with higher inputs of labour and, in Germany at least, of capital.

The mistitling should not diminish the book's worth as statistical exegesis. The author carefully reviews virtually every major series in the official abstracts (annual and monthly), whose order of presentation he follows, in studies appearing over the past decade in Czechoslovak economic journals, and in the annual *Surveys* and quarterly *Bulletins* of the United Nations Economic Commission for Europe (E.C.E.). His view that there are "no bogus figures for publication only" (p. 5) agrees with the consensus of opinion of those studying eastern European statistics, and in particular with Professor Grossman's recent analysis in *Soviet Statistics of Physical Output of Industrial Commodities* (Princeton University Press, 1960).

Among specific series he focuses on the global production aggregates which the eastern European statistical offices use in preference to the net or value-added computations familiar in this country. The global product of industry (Marx's $c+v+m$) exceeds value-added by the cost of inter-enterprise exchanges within the branch, and net-product by the sum of depreciation changes. The rates of growth of global product and of value-added will diverge with changes in the degree of integration or branch-interrelatedness of the economy or in the mean material-intensity of production. Michal wrongly assumes (p. 30) that producers' goods are more material-intensive than consumers' goods (Table 7–28 of the 1959 Czechoslovak abstracts shows the opposite) and hence, because the former has had above-average expansion, that the official global index has risen more in the period of planning than would an index of value-added. His criticism of the E.C.E's use of net product (from the Czechoslovak table of national income by origin) as a surrogate for value-added appears misplaced. In agriculture, on the other hand, he is right to notice (p, 66)—where the present reviewer (*Income and Wealth IX*, p. 152) did not—that global output comprises inter-enterprise transfers, contrary to the general eastern European practice which aggregates the output of commodities, and not that of farms.

While faults can be found in his interpretation of statistical or Marxist concepts, he thoroughly describes both the published series and their lacunae. Among his attempts to fill the latter, the most important is his calculation of gross national product (although his use of the term "factor cost" will appear strange to the non-Marxist reader since he tacitly assumes a unique factor, labour). His method is to enlarge the official "material product" with his own estimates for services and for capital consumption, and to balance the result against income by use. More such endeavours to make western and eastern European national accounts comparable are needed.

M. C. KASER

26. Europe's Needs and Resources: Trends and Prospects in Eighteen Countries. By J. Frederic Dewhurst, John O. Coppock, P. Lamartine Yates, and associates. New York, Twentieth Century Fund, 1961. London, Macmillan & Co., Ltd. xxvi, 1198 p. 10″. 84*s*.

What is one to say about a book like this? Financed by the Twentieth Century Fund, it contains 900 double-column pages of text with nearly 500 tables and charts, together with another 250 pages of appendices. In this space the three author–editors and numerous associates, assistants and independent contributors range over the past, present and future of the Western European economy. Recent and current developments are discussed at length and detailed numerical projections made for almost every major economic magnitude for 1970. The book opens with a short chapter sketching the post-war economic history of Western Europe. Then follow 20 chapters on demand and supplies, most of which are divided into two parts, the first part of the chapter dealing with past trends while the second sets out the 1970 projections for the particular item. Population and manpower

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are treated first and trends in labour-input derived. These are combined, in Chapter IV, with productivity projections to give gross product for 1970. The next 10 chapters discuss trends in the various components of consumer, government and investment expenditures. These are followed by five chapters on the various resources available to Western Europe, and the likely calls on them; and two chapters on international trade, payments and currency problems. The book ends with five general, non-quantitative chapters on very broad subjects (e.g. technology, "capitalism in post-war Europe") and two summarizing chapters. So much for the bare outline of the book; but how does one go about discussing it?

One might begin, as the authors (and the financial backers) might have done well to begin, with the demand rather than the supply side of the book. Whose needs is it intended to meet? Whose resources of time and energy are going to be adequate to cope with it? After even quite a cursory look, the professional economist or statistician is likely to wonder whether it is really intended for him. The chapter on "Output and Expenditures" starts with several pages of very elementary explanation of what gross national product is; at the beginning of the section on medical care we are taken for a three-page run through Hippocrates, Galen, Paracelsus and lime-juice for British sailors. Journalistic clichés, superficial, stale and/or misleading comments on inflation, the free world, capitalism and the trend towards self-service stores crop up again and again—often in the middle of quite sophisticated analysis. The material on past trends is voluminous and often interesting. But the professional is unlikely often to find the answer to his particular question: he will usually want to compare slightly different years, or to use current instead of constant prices (or vice versa); and for most things will probably prefer to consult the raw statistical data—in the O.E.E.C., E.C.E. and U.N. publications particularly. One help that might have been provided is a comprehensive bibliography, but there is none.

If this book is not for the professional, whom is it for? Business men, journalists and politicians are likely to find it indigestible. Worse, they will find it out of date. Unavoidably, there was a long gestation period and, though valiant efforts have been made to bring the book up to date, it remains the case that the base-year for all the projections is 1955; and even the potted economic history does not go beyond 1958 in any serious way. For people engaged in practical affairs this is bound to be unhelpful—all the more so since, as the authors are at pains to point out, the Western European economy is changing very fast.

But perhaps with this kind of book it is pointless to discuss the question of demand: libraries and institutes will always validate Say's Law. What about the substance of the book, the projections for 1970? The key to these is the projection of output per man-hour. Unfortunately the method turns out to be the familiar one of extrapolation with cold feet, i.e. it is assumed that things will go on much as they have been doing, but not quite as fast. Of course there is nothing more difficult than forecasting aggregate productivity, and perhaps there is little that can be done except some sort of qualified extrapolation. But if so one would welcome in a work of this size a good deal more disaggregation both by country and by sector. All the evidence and argumentation for the productivity forecast is given in less than 15 pages. (It is true that there is a long chapter near the end of the book by Jean Fourastié on Productivity; but unsurprisingly the editors have not drawn on it at all. It is easily the strangest and most idiosyncratic chapter in the book—full of remarks such as "happiness is a new idea in Europe" and "Man, who considered himself made for dreaming and praying . . . gradually discovered action . . ."—but unhelpful for reaching any quantitative conclusions about trends in productivity.) Moreover, should not the purpose of a book of this kind be, not to forecast every detail of the European economy in 1970 but to explore the implications of various projections, to see what internal consistencies or inconsistencies emerge from them?

But this is just where the book is weak. There is little serious discussion of alternative assumptions at the global level. All that is said about trends in prices in general is contained in a footnote on page 116: "No assumption has been made in this survey regarding the future price level except that violent inflationary and deflationary movements will be

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avoided." It is doubtless a consequence of this attitude that Europe's terms of trade are nowhere *mentioned*, let alone discussed—surely a curious omission, particularly since one of the authors' conclusions is that Europe is going to become increasingly dependent on the rest of the world for her raw materials.

Figures are now available for the first five years of the 15-year period under discussion, providing many demonstrations of the crudity of the forecasting methods adopted. One instance must suffice. It is stated on page 468 that "there is an element of 'habit' in the saving-investment process which is unlikely to be radically changed in a period as short as the one under consideration here (1955–70)". On the basis of this rule the percentage of G.N.P. taken by fixed non-residential investment is forecast to change little in most countries: in the U.K. it is to rise from 11·5 to 13·0. But the percentage had already risen to 13·0 by 1960. "Habit" has proved relatively easy to break.

The maddening thing about this book is the fact that an enormous amount of work and thought—much of it of a high quality—has gone into it. Many of the chapters on individual resources or elements of demand—particularly the chapter on energy and parts of those on the various components of consumption—amass a great deal of detail, handle it well and develop quite ingenious and sophisticated methods of projection. But the good pieces tend to get lost in the unwieldy formlessness of the whole.

C. W. McMahon

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Current Notes

United Kingdom Official Statistics

1. *Unemployment*

A report on the manpower situation by a working party of the National Joint Advisory Council is published in the *Ministry of Labour Gazette* for February 1962. It contains a summary of the results of a special enquiry made by the Ministry into the characteristics of the unemployed. Figures are given with regard to the proportion classified as difficult to place for various reasons, and of those considered to be good placing propositions. The extent to which the unemployed have received some training is measured as is also the proportion requiring industrial rehabilitation, separate estimates being given for men and for women. Other analyses cover the proportion of disabled persons and the extent of geographical mobility. Detailed figures are promised for future issues of the *Gazette*.

2. *Domestic Food Consumption and Expenditure:* 1959

The tenth Annual Report of the National Food Survey Committee, entitled *Domestic Food Consumption and Expenditure:* 1959, was published in June 1961 (H.M.S.O., 8*s.* 6*d.*). The Report follows the same general arrangement as that for 1958, published in 1960. The main sections describe the diet of the average household in Great Britain and in each region and type of area, and the diets of households of different income levels and of different family composition. One of the trends examined in the Report is the rising demand for "convenience" foods (labour-saving, prepared or semi-prepared products), expenditure on which accounted for nearly a fifth of total household food expenditure in 1959. A rise of 4 per cent. in the real value of food purchases between 1955 and 1959 is almost entirely attributable to increased expenditure on such foods.

The Report includes the results of three special studies. The first of these, which examines the effect of the housewife's age on the household diet, shows, *inter alia*, that food expenditure per head rose fairly sharply as the age of the housewife increased from 20 to 40, but thereafter declined with increasing age; the younger women also spent more on "convenience foods" than the older housewives. The diets of selected groups of old-age pensioner households are also discussed. Between 1957 and 1959 (pension rates were increased in January 1958), their average expenditure on food rose by 10 per cent. compared with an average rise of 4 per cent. for all other households; the general level of retail food prices rose by about 3 per cent. during this period. Thirdly, in Appendix E of the Report, various factors influencing the consumption of fish are examined, including the effect of proximity to ports and markets. Since 1953 the average consumption of all types of fish has remained fairly constant at about 6 oz. per person per week, but there have been some interesting changes in the demand for certain varieties, notably the decline in fresh herrings and kippers. Quick-frozen fillets and fish fingers accounted for 10 per cent. of domestic purchases of fresh white fish in 1959. For many individual foodstuffs the estimated price elasticities of demand, which were published in the Annual Report for 1958, have been recomputed from more recent data and these results are given in another Appendix.

United Nations Publication

The Russian translation of the *Directory of International Standards for Statistics (including a bibliography on methods)*—Statistical Papers, Series M, No. 22, Rev. 1—was issued in January 1962. Copies may be obtained, free of charge, from the Statistical Office

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of the United Nations, New York. The English, French and Spanish versions may be obtained through the regular distributors of United Nations publications (U.S. $0.75, 5*s*. stg, Sw. fr. 3.00.)

United Nations Seminar on External Trade Statistics

A Seminar on External Trade Statistics, organized and conducted by the United Nations Economic Commission for Africa, was held in Addis Ababa from November 29th to December 7th, 1961. It was attended by 29 statisticians from the Government statistical offices and customs administrations of 21 countries, as well as by two United Nations technical assistance experts in Africa. An expert from the Statistical Office of the United Nations attended; the Statistical Office of the European Communities was also represented.

The Seminar considered the application of the recommendations for the improvement of external trade statistics made by the United Nations Economic and Social Council and its Statistical Commission. Recognizing that the recommendations on the compilation of external trade statistics were designed primarily to increase the usefulness of their statistics to the compiling countries themselves as a basis for policy-making, the Seminar considered ways of applying the recommendations in the African countries so that a regular flow of current data would be available. It was decided, in so far as possible, to base commodity-by-country statistics on the 1960 revision of the United Nations Standard International Trade Classification (SITC, Revised). A number of African countries avoid the necessity of reclassifying customs data by basing their customs nomenclatures directly on the SITC, Revised; representatives of countries planning shortly to revise their customs nomenclature expressed the intention of following this precedent.

Also discussed were ways of improving the coverage of the statistics by including a larger portion of the trade passing over land frontiers between adjacent African countries. Because the frontiers are long and inadequately manned, a considerable quantity of goods —many of them non-dutiable—is not recorded by the customs administration and thus escapes the statistics. It was furthermore found that certain other movements, such as governmental transactions, were not universally subject to customs clearance and that special measures would have to be taken to include them in the statistics.

Besides discussing commodity classification and coverage, the Seminar also considered methods of valuation and methods of determining the country of origin of imports and the country of destination of exports.

UNESCO World Survey of Education—Secondary Education

The *World Survey of Education* is a reference work which gives in systematic form descriptive and statistical information on education in all countries and territories of the world.

The first volume of the *World Survey*, which appeared in 1955, was of a general character. Volume II, published in 1958, was devoted more particularly to primary schools studies within the general context of their respective national educational systems. Volume III, now published (UNESCO, paper: U.S. $31.50; 157*s*. stg; 110 *NF*; cloth: U.S. $34; 171*s*. stg; 120 *NF*), follows the pattern established in Volume II. It presents, in relation to each educational system as a whole, a detailed analysis of secondary education: this term is interpreted as covering all types of education—general, technical, vocational, teacher training and other—for young people between the ages of approximately 12 and 18 years.

The book is divided into two parts. In the first part UNESCO has attempted a world view and a summing up of current trends and problems in secondary education. These international studies are based on the detailed information given in the second part, which consists of 200 national monographs written or revised by the educational authorities of the various countries. The main headings in these monographs are: the educational

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system; the development of secondary education; types of secondary education; trends and problems. Statistical tables, diagrams and bibliographies are appended to individual entries.

Research Group in Biometric Medicine, Aberdeen University

The Scottish Hospitals Endowment Research Trust has made a grant to Aberdeen University for a five-year period for the establishment of a Research Group in Biometric Medicine within the Department of Statistics. The Group is intended to consist of three statisticians, who will study biometric and computational methods in relation to a variety of medical problems, endeavouring to maintain a balance between fundamental approach and relevance to current medical research. It is hoped also that the Group can provide a consultative statistical service for medical research in Scotland.

Professor Bartlett's Inaugural Lecture

Probability, Statistics and Time, the Inaugural Lecture delivered by Professor M. S. Bartlett at University College, London on May 15th, 1961, has now been published (London: Lewis, 1961, 21 p. 2*s*. 6*d*.). Professor Bartlett remarks that "one of my own general aims was to keep [the] development [of stochastic processes] properly integrated with the more classical methods of statistical analysis". In this lecture he mentions briefly two examples of statistical inference in stochastic models, for sequences of different types of sedimentary rocks and for the numbers of entrances to caves. The lecture is mainly concerned with the place of probability and information theories in modern physics. Professor Bartlett discusses a number of fundamental questions, such as the conflict between our intuitive view of the irreversibility of time and the reversible role it assumes in quantum theory.

New Statistics Department at the Johns Hopkins University

As a result of recent action by the Board of Trustees, the Johns Hopkins University has established a Department of Statistics in its Faculty of Philosophy. The new department will offer a curriculum leading to the Ph.D. in mathematical statistics. Dr Geoffrey S. Watson has been appointed Professor of Mathematical Statistics in the department, and he will hold a joint appointment as Professor in the Department of Biostatistics of the School of Hygiene and Public Health. Drs David B. Duncan and Allyn W. Kimball, at present professors in biostatistics, have also been appointed professors in the Department of Statistics. Additional appointments are expected in the near future.

The new department is recognized as essential for the expansion and strengthening of statistical research activities in all branches of the university. In order to ensure that the activities are coordinated and that maximum use is made of available resources, Dr Kimball will serve as chairman of both departments. The Ph.D. in mathematical statistics augments the Sc.M. and Sc.D. degree programmes at present available in the Department of Biostatistics. In addition to those already mentioned, the biostatistics staff includes Drs Margaret P. Martin, John J. Gart, Helen Abbey, Richard P. Bland, Earl L. Diamond, Mr Victor Chew, and Dr N. T. J. Bailey, Visiting Professor from Oxford University.

Fellowships, made possible by a training grant from the National Institutes of Health, U.S.P.H.S., are available for students wishing to pursue studies in biostatistics, and it is expected that assistantships in the Department of Statistics will be available for the 1962–63 academic year. Further information about either programme may be obtained by writing to Dr Kimball at 615 North Wolfe Street, Baltimore 5, Md.

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Biometric Society

Dr C. I. Bliss, Connecticut Agricultural Experiment Station, has been elected President of the Biometric Society for 1962.

Abstracts of Statistical Computer Routines

Abstracts of Statistical Computer Routines, by Alanen, Andrew, Leone and Qureishi, was completed in November 1961, and includes over 450 abstracts of statistical computer routines. The classification follows that of *The International Journal of Abstracts: Statistical Theory and Methods*. This publication is an outgrowth of some of the work of the Mathematical Tables Committee of the Institute of Mathematical Statistics, and was sponsored (in part) by the National Science Foundation. Copies of the Abstracts may be obtained by writing to the Computing Center, Case Institute of Technology, Cleveland 6, Ohio. The cost is $3 per copy.

Appointments

Mr D. G. Kendall has been appointed to the newly established Chair of Mathematical Statistics in the University of Cambridge.

Mr A. A. Walters, formerly senior lecturer in the Department of Econometrics and Social Statistics in the University of Birmingham, has been appointed to the newly established Chair of Econometrics and Social Statistics in that University.

Dr M. H. Quenouille has been appointed to a Readership in Statistics at the London School of Economics and Political Science.

Mr W. Lutz, formerly of the Department of Mathematics, Witwatersrand University, has been appointed Lecturer in Statistics in Sheffield University.

Dr C. C. Spicer has been appointed Chief Medical Statistician to the General Register Office.

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List of Books Received for Review

(These books are available to Fellows in the Society's Library)

Ady, P. & Courcier, M. Systems of national accounts in Africa. [Paris, O.E.E.C.], 1960. [London, H.M.S.O.]. 232 p. 9½″. 12*s.* 6*d.*

Attwood, C. Six-figure logarithms, antilogarithms and logarithmic trigonometrical functions. Oxford, London, [etc.], Pergamon Press, 1961. 139 p. 7¼″. 7*s.* 6*d.*

Barna, Tibor. Investment and growth policies in British industrial firms. Cambridge, University Press, 1962. viii, 74 p. 9″. 12*s.* 6*d.* (N.I.E.S.R. occasional paper **20.**)

Bartlett, M. S. Essays on probability and statistics. London, Methuen, [1962]. viii, 127 p. diagrs. 8½″. 21*s.*

Berge, Claude. The theory of graphs and its applications . . . translated by Alison Doig. London, Methuen, [1962]. x, 257 p. diagrs. 9¾″. 36*s.*

Bryant, Edward C. Statistical analysis. New York, [etc.], London, McGraw-Hill, 1960. ix, [3], 303 p. tables, diagrs. 9¼″. 54*s.*

Church of England. *Central Board of Finance, Statistical Unit.* Facts and figures about the Church of England. London, Church Information Office, 1962. 92 p. graphs. 11¼″ × 13½″. 21*s.*

Cole, Dorothy, & Utting, J. E. G. The economic circumstances of old people. Welwyn, Codicote Press, [1962]. 139 p. tables. 8½″. 12*s.* 6*d.*

Cox, Richard T. The algebra of probable inference. Baltimore, Johns Hopkins Press, [1961]. [London, Oxford University Press.] xii, 114 p. 9″. 40*s.*

Cramer, J. S. The ownership of major consumer durables: a statistical survey of motor-cars, refrigerators, washing machines and television sets in the Oxford Savings Survey of 1953. Cambridge, University Press, 1962. xii, 80 p. tables. 9½″. 22*s.* 6*d.* (Department of Applied Economics, monographs, **7.**)

David, Martin Heidenhain. Family composition and consumption. Amsterdam, North-Holland Publishing Co., 1962. xii, 109 p. tables, diagrs. 8¾″. 20*s.*

Desai, G. R. Office premium and average policy value, all durations. . . . Ahmedabad, (India), Swastik Society, 1961. [2], 2, xl, 111 p. 8½″. n.p.

Federation of British Industries. Export incentives: a study of the problem and of policy. London, 1962. viii, 54 p. 8½″. 10*s.*

Ferguson, George A. Statistical analysis in psychology and education. New York, [etc.], London, McGraw-Hill, 1959. viii, 347 p. tables, diagrs. 9″. 54*s.* 6*d.*

Fetter, Robert B. & Dalleck, Winston C. Decision models for inventory management. Homewood (Ill.), Richard D. Irwin, 1961. x, 123 p. tables, diagrs. 9″. $5.75.

Fisher, Franklin M. *A priori* information and time series analysis: essays in economic theory and measurement. Amsterdam, North-Holland Publishing Co., 1962. [12], 168 p. 8¾″. 36*s.* (Contributions to economic analysis **xxvi.**)

Fisher, Franklin M. & Kaysen, Carl. A study in econometrics: the demand for electricity in the United States. Amsterdam, North-Holland Publishing Co., 1962. [12], 190 p. tables. 8¾″. 36*s.* (Contributions to economic analysis **xxvii.**)

Florence, P. Sargant. Post-war investment, location and size of plant. Cambridge, University Press, 1962. viii, 54 p. tables. 9″. 12*s.* 6*d.* (N.I.E.S.R. occasional paper **19.**)

Food and Agriculture Organization. Trends and forces of world sugar consumption; by A. Viton and F. Pignalose. Rome, 1961. [London, H.M.S.O.] viii, 88 p. tables, diagrs. 11″. 5*s.* (Commodity bulletin series **32.**)

The world sugar economy in figures: 1880–1959. [Rome, 1961.] [London, H.M.S.O.] [10], 137 p. tables, 11″. 7*s.* 6*d.* (Commodity reference series **1.**)

Gershenson, Harry. Measurement of mortality. [Chicago], Society of Actuaries, 1961. xii, 340 p. diagrs. 9″. n.p.

Gordon, A. H. Elements of dynamic meteorology. London, English Universities Press, [1962]. xxii, 217 p. diagrs. 8½″. 25*s.*

Hall, P. G. The industries of London since 1861. London, Hutchinson, 1962. 192 p. maps, diagrs. 7¼″. 12*s.* 6*d.*

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Indian Society of Agricultural Economics. Seminar on cost studies in agriculture. Bombay, [1961]. 177 p. frontis. 9¾″. 8 *Rs.*

International Labour Office. Employment objectives in economic development: report of a meeting of experts. Geneva, 1961. [London.] xii, 255 p. tables. 9½″. 16*s.* 6*d.*

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Likelihood Inference and Time Series

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Summary

It is argued that, when, on the evidence of a given set of data, the plausibilities of various hypotheses are to be compared, then the primary inference is provided by the likelihood function. The suggestion is made that the practice of looking at the whole course of the likelihood function should receive much more attention. Some examples are given to illustrate the usefulness of this procedure, including especially some arising in problems of inference from time series.

1. Introduction

According to Schopenhauer a philosophy is like an organism with no beginning and no end. He therefore recommended that any book on philosophy should be read at least twice. We hesitate to claim such indulgence for ourselves, but in trying to expound the likelihood approach to some of the problems of statistical inference we have met with similar difficulties. In trying to make things simple at the outset we have sometimes made broad statements which are limited and qualified later on. At the same time we recognize that we have been following a path more or less arbitrarily chosen through the area covered and that there are many byways which we could with profit have explored but which we have not approached. The more important of these byways have been designated $\mathfrak{B}$ in the text.

Suppose we are tossing a penny with the idea of finding out something about the probability θ of "heads", and we observe 3 heads in 20 throws. What does this tell us about θ? According to what is perhaps the majority view nowadays we should consider the result "3 heads" as one of a set of 21 possible results, 0, 1, 2, ..., 20, heads, obtainable in 20 throws; for each of these 21 results the probability, for a fixed θ, is given by

$$\Pr\{x \text{ heads} | 20 \text{ throws}\} = f(x, \theta) = \frac{20!}{x!(20-x)!}\theta^x(1-\theta)^{20-x}, \tag{1.1}$$

and we can use this fact in various ways to make "inferences" from the given result about θ. We may, for instance, insert the observed result $x = 3$ into the function $f(x, \theta)$, and so obtain a function of θ,

$$f(3, \theta) = 1140\,\theta^3(1-\theta)^{17}, \tag{1.2}$$

the *likelihood function*, given the observed result. It is so called because it gives a measure of relative plausibility of different values of θ, so that we can say that $f(x, \theta_1)/f(x, \theta_2)$ is the odds ratio for θ_1 versus θ_2, on the basis of the result x, or the weight of the evidence for θ_1 as against θ_2, provided by x. The likelihood function

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takes its maximum value at $\hat{\theta} = 0{\cdot}15$, and the second derivative of its logarithm is

$$-\{3/\theta^2 + 17/(1-\theta)^2\} \tag{1.3}$$

which, when $\theta = \hat{\theta}$, takes a value whose reciprocal square root is $-\sqrt{\{\hat{\theta}(1-\hat{\theta})/20\}}$, or $-\sqrt{(51/8000)}$ or $-0{\cdot}08$, nearly. We may express these facts by saying that, given the result 3 heads in 20 throws, the maximum likelihood estimate of θ is 0·15, with "standard error" 0·08.

Another way of making an inference about θ is to sum the probabilities of getting 3 heads or fewer, $f(1, \theta) + \ldots + f(3, \theta)$, and find that when $\theta = \frac{1}{2}$, this comes to 0·0013, while it is less than 0·05 whenever θ is greater than 0·35. We may express these facts by saying that the hypothesis $\theta = \frac{1}{2}$ is rejected at the 0·0013 level of significance, and that the interval $0 < \theta < 0{\cdot}35$ is a 95 per cent. confidence interval for θ.

However, in saying that we have observed 3 heads in 20 throws, we have said nothing about the "stopping rule" we used—we might have intended to go on throwing until 3 heads were observed, and then stop, instead of going on until we have made 20 throws, as we have tacitly assumed above. If we intended to go on till 3 heads were observed, then our result "20 throws" would have been one of an infinite set of possible results, $3, 4, 5, \ldots$, *ad inf.*, throws. The probability of y throws, for fixed θ, is

$$g(y, \theta) = \frac{(y-1)!}{2!\,(y-3)!}\,\theta^3(1-\theta)^{y-3}, \tag{1.4}$$

and the likelihood, given 20 throws, is now

$$g(20, \theta) = 171\,\theta^3(1-\theta)^{17}. \tag{1.5}$$

This is not the same as (1.2), but the two functions are in a constant ratio, 20/3, to one another, and since we use likelihoods always as ratios, the two likelihood functions are equivalent. (From now on when we say two likelihood functions are the same, we shall usually mean that they are the same but for a constant multiplier.) In particular, (1.4) gives exactly the same maximum likelihood estimate, 0·15, and exactly the same "standard error", $\sqrt{(51/8000)}$. On the other hand, to find the significance level of our result, relative to the hypothesis $\theta = \frac{1}{2}$, we have to sum $g(x, \frac{1}{2})$ over $x = 20, 21, 22, \ldots$, *ad inf.*, giving the result 0·0004, while the set of values of θ for which

$$\sum_{x=20}^{\infty} g(x, \theta) < 0{\cdot}05$$

is the interval $0{\cdot}30 < \theta < 1$. Thus the significance level and the confidence interval, $0 < \theta < 0{\cdot}3$, are not the same as before. To define precisely the significance level or the confidence interval, we need to know the totality of other possible results with which the given result is to be compared. To define the likelihood function we do not need to know more than that this *reference set* does not depend on θ, the parameter of interest.

According to the likelihood principle, which we shall be concerned to defend and illustrate, the result "3 heads in 20 throws" should provide the same inference about θ, regardless of which stopping rule was used, because both stopping rules give the same likelihood function for the given result. The significance test and the confidence interval procedures fail to accord with the likelihood principle.

To state the principle in a general form, we shall restrict ourselves for the present to cases where the probability distributions involved are discrete. That no real loss

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of generality is involved in this is argued at the end of the paper. We therefore consider experimental situations describable in terms of a set of possible experimental results, called the *sample space*, a set of possible parameter values, called the *parameter space*, and a function of two variables, called the *kernel*, the first variable ranging over the sample space, the second ranging over the parameter space. For each fixed parameter value, the kernel function gives the probability function of the experimental result. Two such situations, for which the set of possible parameter values is the same, can be represented by the triples (S, Ω, f) and (T, Ω, g), S and T representing the two sample spaces, and $f(x, \theta)$, $g(y, \theta)$ representing the two kernel functions, while Ω represents the common parameter space. In the coin tossing example S is the set $0, 1, 2, \ldots, 20$, while T is the set $3, 4, 5, \ldots$. Ω is the set $0 < \theta < 1$ (excluding the extreme values 0 and 1, which would correspond to a double-headed or double-tailed penny), while f and g are, respectively, given by (1.1) and (1.4) above. The result x from (S, Ω, f) and the result y from (T, Ω, g) will give equivalent likelihood functions $f(x, \theta)$ and $g(y, \theta)$ if a number c exists independent of θ, such that

$$f(x, \theta) = cg(y, \theta), \tag{1.6}$$

and the likelihood principle says that, in this case, the inference from x about θ would be the same as the inference from y about θ.

In advocating the likelihood principle as a basic proposition in the theory of inference, it is necessary to emphasize that it is not proposed that it should be used uncritically unless the model is known very precisely. Suppose, for example, that the results from two independent sequences each consisting of 100 Bernoulli trials indicated 50 successes in both cases. If, however, the successes occurred haphazardly in one sequence and in the form 0101 ... 1 in the other, the inference for the two sequences would be very different despite the fact that the likelihoods were the same; one would investigate the systematic pattern of 0's and 1's in the second sequence.

In general, in order to write down a likelihood function, it is necessary to *assume* that the model is known apart from some parameters which are to be estimated. Some of these parameters may characterize lack of independence amongst the errors. If the model has to be guessed, then the nature of the inference becomes much less precise than is suggested by the formal statement of the likelihood principle. In fact, many problems in experimental design are concerned with hypotheses about the functional form of the likelihood function.

When the distribution of the errors is not known, one would be interested in the *robustness* of the likelihood function with respect to changes in the form of the error distribution and in the corresponding robustness of any estimation procedures one would care to advocate. We shall have a little to say about this later, but there is much research here remaining to be done.

Another important qualification which should be borne in mind throughout this paper is that we are exclusively concerned with the inference problem, not with the problem of experimental design. We consider only the situations which arise when the experiment (S, Ω, f) or the experiment (T, Ω, g) has been carried out, not with which experiment of the two should be carried out if we have the choice. In the design problem it is obvious that the whole of the sample space must enter into consideration, because it is just this which is in fact being chosen when we choose an experimental design. To quote a phrase used by one of us (Barnard, 1949), *before* the experiment is carried out (i.e. in the design problem) probabilities are relevant, *after* the experiment likelihoods are relevant. We shall be concerned in this paper with the situation after

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the experiment. This is not to suggest that the dichotomy between the two attitudes should be complete; indeed, a recognition of the essence of the inference problem is necessary to a proper appreciation of the design problem (cf. for example, the discussion of the paper by Kiefer (1959)), which can often be summed up best by saying that we should aim the design to produce the most informative likelihood function.

The likelihood principle is an immediate consequence of the Bayesian approach to statistical inference. For in this approach we suppose that our initial state of knowledge is represented by a prior probability function $p(\theta)$ defined over Ω, so that, after the experiment (S, Ω, f) has given the result x, the new state of knowledge is represented by the posterior distribution

$$f(x, \theta)\, p(\theta) \Big/ \int f(x, \theta)\, p(\theta)\, d\theta. \tag{1.7}$$

If, on the other hand, the experiment (T, Ω, g) were performed, and gave the result y, the new state of knowledge would be represented by

$$g(y, \theta)\, p(\theta) \Big/ \int g(y, \theta)\, p(\theta)\, d\theta, \tag{1.8}$$

and if the condition (1.6) were satisfied the constant c would cancel top and bottom in (1.8), giving (1.7). Thus either x or y would give the same posterior distribution, and therefore the same inference.

It may be noted that this Bayesian argument requires only that $p(\theta)$ should exist; it does not have to be the same from one person to another, nor from one time to another for the same person. Regardless of subjective or objective changes in p, from one occasion to another, on any given occasion it remains true that the result x or the result y would give the same inference. Thus, subjective Bayesians have asserted that all that need be reported from an experiment is the likelihood function, since this will enable anyone to form his own posterior distribution from his own prior distribution. However, for those who, like the present writers, find themselves unable to accept the idea that every state of knowledge about a parameter θ is expressible in a probability distribution ($\mathfrak{B}$), this Bayesian argument does not imply the universal applicability of the likelihood principle, although it does imply its appropriateness in cases where prior distributions exist.

Reference should perhaps here be made to the arguments due to Savage and Lindley (Savage *et al.*, 1962), according to which, speaking somewhat loosely, we must *behave* as if we were Bayesians, if we are to behave rationally. Such arguments again imply the universal validity of the likelihood principle, for those who (unlike the present authors) adopt a behaviouristic philosophy of inference.

The term likelihood, as used here, and the recognition of the importance of the concept for statistical inference are due, of course, to Fisher. In his classical series of papers (1922, and especially 1925 and 1934) he showed how useful it was to consider the behaviour of the likelihood function in connection with problems of estimation. The arguments which we shall present for the likelihood principle owe a great deal to Fisher, as will be seen. We should add that Bartlett (1936, 1953 for example) has been one of the few authors who have consistently used the likelihood function and its properties as a guide to statistical inference, though his emphasis on frequency interpretations has led him to ascribe more relevance to the sample space than we give it here. But before we proceed to give these arguments, it may be helpful to

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illustrate a little further how the likelihood function can be used in making statistical inferences. In our illustrations we shall sometimes take for granted a somewhat extended form of the likelihood principle: two likelihood functions which are *approximately* equivalent yield *approximately* the same inference (𝔅).

2. Illustrations of Likelihood Inferences

We begin by considering what is, in a certain sense, a standard form of experiment: the measurement of an unknown θ with a measuring instrument having no systematic error, and random errors normally distributed with standard deviation σ. For this set of possible observations is the real axis R, $-\infty < x < +\infty$, the parameter space is also R, and the kernel is

$$\mathcal{N}_\sigma(x-\theta) = \{1/\sqrt{(2\pi)}\,\sigma\}\exp -(x-\theta)^2/2\sigma^2. \tag{2.1}$$

Table 1

Binomial likelihood and various normal approximations

θ	(1)	(2)	(3)	(4)	(5)
0	0·00000	$0{\cdot}0^{1}3648$	0·0000	$0{\cdot}0^{1}1718$	0·0000
0·02	$0{\cdot}0^{5}5675$	$0{\cdot}0^{1}5659$	0·1003	$0{\cdot}0^{1}2947$	0·1926
0·05	$0{\cdot}0^{1}5227$	$0{\cdot}0^{1}9723$	0·5376	$0{\cdot}0^{1}5885$	0·8880
0·10	$0{\cdot}0^{3}1668$	$0{\cdot}0^{3}1751$	0·9525	$0{\cdot}0^{3}1362$	1·2243
0·15	$0{\cdot}0^{3}2130$	$0{\cdot}0^{3}2130$	1·0000	$0{\cdot}0^{3}2130$	1·0000
0·20	$0{\cdot}0^{3}1801$	$0{\cdot}0^{3}1751$	1·0288	$0{\cdot}0^{3}2250$	0·8005
0·25	$0{\cdot}0^{3}1174$	$0{\cdot}0^{1}9723$	1·2080	$0{\cdot}0^{3}1606$	0·7313
0·3	$0{\cdot}0^{1}6281$	$0{\cdot}0^{1}3648$	1·7220	$0{\cdot}0^{1}7744$	0·8110
0·4	$0{\cdot}0^{1}1083$	$0{\cdot}0^{5}1583$	6·8429	$0{\cdot}0^{5}5552$	1·9511
0·5	$0{\cdot}0^{6}9537$	$0{\cdot}0^{7}1431$	66·62	$0{\cdot}0^{7}8294$	11·498
0·6	$0{\cdot}0^{7}3711$	$0{\cdot}0^{10}2696$	1,376	$0{\cdot}0^{9}2581$	143·78
0·7	$0{\cdot}0^{9}4430$	$0{\cdot}0^{13}1058$	41,859	$0{\cdot}0^{12}1673$	2,647
0·8	$0{\cdot}0^{12}6711$	$0{\cdot}0^{18}6711$	775,750	$0{\cdot}0^{16}2260$	29,697
0·9	$0{\cdot}0^{17}7290$	$0{\cdot}0^{22}1473$	494,770	$0{\cdot}0^{21}6358$	11,466
1·0	0·00000	$0{\cdot}0^{28}5228$	0·0000	$0{\cdot}0^{26}3727$	0·0000

The columns represent: (1) Binomial likelihood $\theta^3(1-\theta)^{17}$. (2) Normal approximation centred at $\theta = r/n$. (3) Ratio of (1) and (2). (4) Normal approximation centred at $\theta = (r+1)/(n+2)$. (5) Ratio of (1) and (4).

The likelihood for θ, given x, has the form of a normal density curve, centred at x, with standard error σ; we in fact tend to think of θ as being, in some sense, normally distributed about x with standard error σ. When, as in the coin tossing example above, we say that the maximum likelihood estimate of the unknown θ is 0·15, with standard error 0·08, we are, in effect, saying that our information about this θ is approximately the same as if we had measured it with an instrument having no systematic error and standard error 0·08 and had obtained the reading 0·15. For the likelihood function would in that case have been

$$\mathcal{N}_{0\cdot08}(\theta - 0\cdot15) \tag{2.2}$$

which we have tabulated, alongside the likelihood (1.2) above, in Table 1. The third column of the table gives the ratio of expression (2.1) to expression (1.2). It can be seen that this ratio is fairly constant except for high and for low values of θ, and these high and low values may tend to be discounted, since they would make what has

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been observed rather improbable. Had there been 200 throws, with 30 heads, the agreement between the true likelihood function and the normal approximation would have been very much better over the range of the reasonably possible. As it is, the agreement is only fair, and the interpretation of the data in terms of an estimate of 0·15 with standard error 0·0792 is somewhat misleading. The overall agreement with the normal likelihood is improved if we move the normal distribution up a little, centring it at 0·182 instead of at 0·15. This corresponds to the fact that we usually do well to think of estimates of binomial probabilities from r occurrences out of n as $(r+1)/(n+2)$.† The inspector of a batch of goods who finds no defectives in a sample of 10 would do better, on the whole, to think of his "best estimate" of the true proportion defective as 1/12 rather than as zero (though, of course, no single estimate should be wholly relied on). The truth is that the binomial likelihood, for small samples, is not really well represented by any normal likelihood, so that it is better to have regard to the whole course of the likelihood function, rather than to the parameters of any normal approximation. This is, of course, especially true in the cases corresponding to "0 heads in 20 throws" and "20 heads in 20 throws", even more than in the one we have considered in detail. Looking at the likelihood function we see that while 0·15 is our "most likely" value for θ, deviations in an upward direction from this are more likely than equal deviations in a downward direction. The representation of this asymmetry in the possible errors is one of the advantages of the likelihood function over the estimate with standard error approach.

Another simple but instructive example is provided by an exercise we often use for class demonstrations. A whole number θ is fixed—say, as the number on a rail ticket—but not told to the class. Cards are then drawn, with replacement, from a shuffled pack, and the number on the card is added to θ (J, Q, K counting 11, 12, 13) and the results are announced. After 10 such announcements, the class is required to say what they know about θ. It is easy to see that, of the 10 announced numbers, we need pay attention only to the lowest L and the highest H, say, because given these numbers, all possible arrangements of the remaining 8 numbers, between these limits, are equally probable,‡ whatever the value of θ. Alternatively, we may note that as each number N is announced, it conveys the information that θ lies between $N-1$ and $N-13$, all values of θ in the possible range being equally likely. But unless N is higher than any number so far announced, we shall know that $\theta \geqslant N-13$ already; while unless N is lower than any number so far announced, we shall know already that $\theta \leqslant N-1$. Thus unless N is either highest or lowest, it gives us no new information. It is then apparent that θ cannot be larger than $L-1$, and cannot be smaller than $H-13$, while there is nothing to choose between any two values for θ within this range. The likelihood function in this case takes the value zero outside the range from $H-13$ to $L-1$, and is constant within this range; its relative values thus perfectly express what we know from the sample about θ. In particular, with good luck we shall have $H-13$ equal to $L-1$, and then we can be sure that their common value is θ. On the other hand, with extremely bad luck (probability $(1/13)^{10}$), all announced numbers will be equal and we shall be no better off in estimating θ than if we had had only one number announced.

† The choice of 1 and 2 in the numerator and denominator is made on grounds of simplicity—the correction is of the right order of magnitude only. It is derived by forcing a correspondence between the first moments of the two likelihood functions, which is reasonable provided that we are measuring the parameter on a natural scale.

‡ The possibilities of ties complicates the algebra, but not the argument.

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This example is instructive because there is evidently no sense here in taking a single maximum likelihood estimate, with a standard error, to represent our information. And if we use confidence intervals we find, for example, that if (for convenience in arithmetic) we choose a confidence coefficient $1-(6/13)^{10}$, then a "best" upper confidence bound B for θ is got by setting B equal to $L-1$ or $H-7$, whichever is the smaller. The guaranteed frequency of correct statements of the form $\theta \leqslant B$ is then achieved by often making a statement which is certainly correct, viz. $\theta \leqslant L-1$, and sometimes making a statement which is, as like as not, false, viz. $\theta \leqslant H-7$. Such a procedure might be called "inductive behaviour", or some less politely qualified behaviour, but it could hardly be called "inference".

As another example, let us consider single observations x from continuous distributions having densities

$$\frac{k}{2\Gamma(1/k)}\sqrt{\left\{\frac{\Gamma(3/k)}{\Gamma(1/k)}\right\}}\exp-\left|(x-\theta)\Big/\sqrt{\left\{\frac{\Gamma(3/k)}{\Gamma(1/k)}\right\}}\right|^k \tag{2.3}$$

in $-\infty < x < +\infty$. As k ranges from 2 to infinity, we have a continuous transition from likelihood functions which are exactly normal to likelihood functions which are continuous analogues of the rectangular likelihoods considered above. In the range $k > 2$, therefore, the usual procedure for getting standard errors of maximum likelihood estimates will exaggerate the likelihood of moderately large errors in the estimate, the exaggeration being worse for larger k. Similarly, in the range $0 < k < 2$, the usual procedure may lead us to under-rate the likelihood of moderately large errors.

As a more practical example of the application of ideas of likelihood, we discuss a question which has already been examined by Anscombe (1961), under the title "What constitutes a 99 per cent. guarantee of 99·95 per cent. reliability?" We refer to Anscombe's paper for a very penetrating discussion of many of the issues involved, but for our present purposes we may state the problem as one of providing a sequential procedure for testing a piece of equipment which either succeeds or fails at each of a number of trials. If θ denotes the probability of failure at a single trial, it is laid down that the test procedure must be such that, for any equipment that is accepted, it is "99 per cent. certain" that θ is less than 1/2000. It had been suggested that an ordinary Wald type sequential test procedure would meet this requirement, if the hypothesis $\theta = 1/4000$ were tested against the hypothesis $\theta = 1/2000$, with risks of error 1/100. The equipment would be accepted if the test procedure ended with acceptance of $\theta = 1/4000$, while it would be rejected otherwise. If r failures and s successes are observed in $r+s$ trials, the likelihood for θ is

$$\lambda(\theta) = \theta^r(1-\theta)^s, \tag{2.4}$$

and so the Wald procedure involves computing the ratio

$$\lambda(1/4000)/\lambda(1/2000)$$

at each stage, and terminating with acceptance of the equipment as soon as

$$\lambda(1/4000)/\lambda(1/2000) > 99/1. \tag{2.5}$$

Anscombe suggests against this a Bayesian procedure starting with a prior distribution for θ with density

$$p(\theta) = 1401(1-\theta)^{1400}, \quad 0 < \theta < 1 \tag{2.6}$$

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and accepting when the posterior odds in favour of $\theta < 1/2000$ are 99/1, i.e. when

$$\int_0^{1/2000} p(\theta)\,\lambda(\theta)\,d\theta \Big/ \int_{1/2000}^{1} p(\theta)\,\lambda(\theta)\,d\theta = 99/1. \tag{2.7}$$

The difference between Anscombe's acceptance rule (2.7) and the Wald procedure (2.5) is not as great as might be thought, however. If we adopt a general "weight function" $w(\theta)$, and integrate $\lambda(\theta)$ with respect to it,

$$\int \lambda(\theta)\,w(\theta)\,d\theta,$$

we can make this as close as we like to a value $\lambda(\theta_0)$ for any θ_0 in the range of integration, by taking $w(\theta)$ to have a sufficiently high "spike" at θ_0, and to be sufficiently near to zero elsewhere. In particular, if we take

$$w(\theta) = \{\mathcal{N}_\sigma(\theta-(1/4000)) + \mathcal{N}_\sigma(\theta-(1/2000)+\sqrt{\sigma})\}/2$$

for sufficiently small σ, we can make the condition

$$\int_0^{1/2000} w(\theta)\,\lambda(\theta)\,d\theta \Big/ \int_{1/2000}^{1} w(\theta)\,\lambda(\theta)\,d(\theta) = 99/1$$

approximate as closely as we like to the Wald condition (2.5). But this condition is of exactly the same form as (2.7), with $w(\theta)$ in place of $p(\theta)$.

It is true that the Wald procedure implies a definite rejection rule, in addition to the acceptance rule, while, as Anscombe points out, the Bayes rule (2.7) does not require a rejection rule to be specified. But the definiteness of the Wald rejection rule is really associated with the fact that *both* risks, of "wrong acceptance" and of "wrong rejection", have been specified, and this specification of both risks is not really called for in the problem. By suitable adjustment of the risk of wrong rejection we can obtain a wide variety of Wald rules all consistent with the acceptance rule (2.5).

Our main contention is that both Anscombe, and those he criticizes, are in effect summarizing the whole likelihood function $\theta^r(1-\theta)^s$ into a simple ratio, 99/1, and this is really too brutal a method of summarization. We are convinced that those to whom the statistician is reporting would be able to appreciate the meaning of the whole likelihood function were it given to them; but even supposing they were not, and that some simplification were essential, it would seem much more reasonable to approximate to the likelihood function by a step function taking, say, four different values. To do this we would choose four intervals in θ, reasonably relevant to the problems in hand—such as $(0, 1/4000)$, $(1/4000, 2/4000)$, $(2/4000, 3/4000)$ and $(3/4000, 1)$, which could here be considered as corresponding to "very good", "just good", "just bad" and "very bad", and we would then approximate to $\lambda(\theta)$ by its mean values in these ranges:

$$\lambda_1 = 4000\int_0^{1/4000} \lambda(\theta)\,d\theta, \quad \lambda_2 = 4000\int_{1/4000}^{1/2000} \lambda(\theta)\,d\theta,$$

$$\lambda_3 = 4000\int_{1/2000}^{3/4000} \lambda(\theta)\,d\theta, \quad \lambda_4 = (4000/3997)\int_{3/4000}^{1} \lambda(\theta)\,d\theta,$$

and the ratios $\lambda_1 : \lambda_2 : \lambda_3 : \lambda_4$ can then be thought of as the odds, on the basis of the trial, that the equipment is very good, good, bad or very bad. Thus, for example,

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Anscombe gives the results $r = 0$, $s = 7{,}800$ and $r = 4$, $s = 21{,}800$ as both leading to acceptance. For the first result we have $\lambda_1 : \lambda_2 : \lambda_3 : \lambda_4 = 12 \times 10^5 : 17 \times 10^4 : 24 \times 10^3 : 1$ while for the second the ratios are $82 \times 10^5 : 45 \times 10^5 : 2 \times 10^5 : 1$. While the first result gives stronger evidence in favour of the equipment being very good, the second result gives greater assurance against its being really bad. The summarization into the single odds ratio (2.7) thus can be seen to involve a rather arbitrary choice of weights as between "good" and "very good", and between "bad" and "very bad"; we think it part of the duty of the statistician in this situation to make it clear to the decision makers that they must choose some weighting of this sort, and that, in particular, their original demand for a 99 per cent. guarantee could not really be met in any case.

Another advantage we would claim for the likelihood approach is that it does not involve the arbitrariness which is a necessary feature of the Bayesian approach. Anscombe's choice of $p(\theta)$ is arrived at by first restricting consideration to $p(\theta)$ of a certain form *purely on grounds of mathematical convenience*. He then argues that for functions of this special form certain common-sense requirements can be met only with a $p(\theta)$ rather similar to the one quoted above. However, this argument suffers from the fatal flaw of being based *primarily* on mathematical convenience. In fact, Anscombe's common-sense considerations can easily be met, by $p(\theta)$ very different from the one he takes, if the considerations of convenience are given up. His assumed form for $p(\theta)$ can be thought of, from the likelihood point of view, as tantamount to assuming that 1,400 trials have been made, without a failure, before formal testing begins. It could be that information of this sort, or equivalent to this, was in fact available in a given case, and then, of course, it should be incorporated in the overall likelihood; but in general this will not be so.

According to the likelihood approach, we should first express quantitatively what we really know. In the present instance, this will first of all be given by the likelihood function $\lambda(\theta) = \theta^r(1-\theta)^s$. This may be supplemented by other relevant facts, which may give another likelihood function, $\lambda_0(\theta)$, say, which may perhaps be a little indeterminate because of the presence of other, irrelevant parameters (cf. the discussion of non-normality, below, in connection with moving averages). The combined likelihood function will then be obtained as the product $\lambda(\theta)\,\lambda_0(\theta)$; this simple possibility of combination of all relevant pieces of data is one of the principal advantages of the likelihood function as an expression of information. Given the overall likelihood, $\lambda(\theta)\,\lambda_0(\theta)$, we have to choose our "weights" to arrive at a decision for or against acceptance. The advantage of looking first at the likelihood function, and then considering the "weights", lies in the fact that the likelihood function will often be so near to zero over much of the range of θ, that the weights in these regions can be quickly dismissed from consideration. To take an extreme case, one might have thought that special value should be attached to apparatus which was really perfect, with $\theta = 0$, so that special weight ought to be given to this value of θ. One might waste a great deal of time, in advance of the experiment, considering what weight should properly be attached to $\theta = 0$, and then find that $r \neq 0$, so that the possibility of $\theta = 0$ would be excluded anyway.

3. Likelihood and Power

To the present authors, the interpretation of likelihood, as a measure of credibility of a hypothesis in the light of given data, seems as clear, intuitively, as the interpretation of probability, as a measure of credibility of an event in the light of an hypothesis.

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To say that the likelihood of $\theta = 1$ is 10 times the likelihood of $\theta = 2$ means that the evidence favours $\theta = 1$ as against $\theta = 2$, and the number 10 represents the weight of the evidence, in a rather precise sense (see, for example, Barnard, 1949). But it seems that this is not so for everyone, and in particular there are those who say that they can understand what is meant by probability, but cannot understand what is meant by likelihood. Part of this difficulty arises, we think, from the way in which our human minds shrink from uncertainty. We would like to be given a rule, like "accept H_1 as against H_2 on data E, when the likelihood ratio is 20 : 1 or more"; but the fact is that no such rule can really be justified. The uncertainty is there—H_2 may be true—and we conjure it away by appeal to incantation at our peril. We can, of course, make an analogy with a man who draws a white ball from one of two urns, one of which has nothing but white balls, while the other has only one white ball in 20; we can say that if we chose H_1 rather than H_2 on data E we would be choosing as sensibly as this man would, if he chose to believe that the urn he drew from was all white. But whether such a choice was a justifiable reaction to uncertainty so measured would require specification of many more features of the problem than have been mentioned. In many situations it is not rational or necessary to make a choice.

The truth is that the uncertainty we feel about hypotheses is a quantity *sui generis*, which we claim is measured by the likelihood. The justification of this must proceed along lines similar to the justification of other measures of other quantities, such as mass, length, time, temperature and so forth.

However, there is a concept which is familiar to many statisticians which is so closely related to likelihood that it may serve to bridge the gap of understanding, to some extent. We refer to the Neyman–Pearson concept of power. It is the object of this section to exhibit some aspects of the relationship between likelihood and power ($\mathfrak{B}$).

To proceed by way of a simple example, let us consider the 2×2 table:

	A	not-A	Total
Population I	a	$3-a$	3
Population II	b	$3-b$	3

obtained by taking samples of 3 from populations I and II, in which the probabilities of A are p, p', respectively. For this experiment the sample space is the set of all integer pairs $x = (a, b)$, with $0 \leqslant a,\ b \leqslant 3$, the parameter space Ω is the unit square $0 < p,\ p' < 1$, and the kernel is

$$f(x, \theta) = f((a, b), (p, p')) = \binom{3}{a}\binom{3}{b} p^a (1-p)^{3-a} p'^b (1-p')^{3-b}.$$

To make a test of the hypothesis $p = p'$, in the Neyman–Pearson formulation, we choose a critical region C in the sample space. Suppose we make C consist of the single point $(0, 3)$. Then the power of our test is

$$\Pr\{x \in C | p, p'\} = (1-p)^3 p'^3.$$

This is a function defined on the parameter space and, in fact,

$$\Pr\{x \in C | p, p'\} = f((0, 3), \theta),$$

which shows that, in this case, the likelihood function, given the observation in the critical region, is exactly the same as the power function.

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If, now, our critical region C is chosen to consist of $(0,3)$ and $(3,0)$, then the power function is

$$\Pr\{x \in C \mid p, p'\} = f((0,3), \theta) + f((3,0), \theta),$$

which is the sum, or (since relative values only are involved) the average of the likelihoods given $(3,0)$ and $(0,3)$, the two points in the critical region. In general, as Fisher long ago observed in an important passage (see, for example, Fisher, 1946), the power function is identical with the likelihood function, given not the full result x, but only the information that x is in the critical region.

To put things a little more formally, the Neyman–Pearson procedure of "hypothesis testing" consists in classifying the results of the experiment (S, Ω, f) according to whether they lie within the critical region C, or without. This amounts to replacing (S, Ω, f) by the less informative experiment $((1,2), \Omega, g)$, where the new sample space consists of two points only, 1 corresponding to $x \in C$ and 2 corresponding to $x \notin C$. We shall then have

$$g(y, \theta) = \sum_{C} f(x, \theta) \quad \text{for} \quad y = 1$$

$$= \sum_{S-C} f(x, \theta) \quad \text{for} \quad y = 2.$$

Evidently $g(1, \theta) = 1 - g(2, \theta)$, so that the two likelihood functions obtainable from this less informative experiment are mirror images of each other. Each of these likelihood functions is thus, essentially, the power curve of the test with critical region C.

It is standard teaching, to warn against merely reporting a result as "not significant" without giving the power function of the test used. This amounts to a partial recognition that such information as is contained in the result of the test procedure is really contained in the likelihood function which it yields.

The practice in some quarters of quoting two levels, "significant"(*) and "highly significant"(**), can be seen to be tantamount to reduction to a three-point sample space. This is, of course, a less crude simplification, but the notion of power associated with it involves two functions, not only one. It is noticeable (for example, see Lehmann (1957)) that statisticians of the Neyman–Pearson school are more and more tending to treat of "multiple decision problems", for which the sample space is divided into more and more regions. The logical end of this process is, of course, to divide the sample space into its separate points, when the "power functions" associated with each region would coincide with the likelihood functions.

It may be noted that, from the purely logical point of view, we can make probability comparisons only between different events, on the basis of the same hypothesis. Whenever we make comparisons of probabilities of the same event on different hypotheses, as, for example, in the definition of an unbiased test:

$$\Pr\{x \in C \mid \theta = \theta_0\} \leqslant \Pr\{x \in C \mid \theta \neq \theta_0\},$$

we are really making likelihood comparisons.

The procedure of replacing (S, Ω, f) with $(\{1,2\}, \Omega, g)$ involves, of course, a loss of detail, but in return there may be a gain in simplicity. More generally, we may map the sample space S onto another space T, by a mapping $y = t(x)$, and so obtain the less informative experiment (T, Ω, g), where

$$g(y, \theta) = \sum_{t^{-1}(y)} f(x, \theta),$$

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$t^{-1}(y)$ denoting the set of all x for which $t(x) = y$. The likelihood function $g(y, \theta)$, given y only, will normally be less informative than the original likelihood function $f(x, \theta)$; the lost information will be represented by the likelihood function of x when y is known—the information on θ given by knowing which of the x's in $t^{-1}(y)$ in fact gave rise to y. In many cases the lost information will be slight, and its loss may be outweighed by a gain in simplicity. An example of this will occur later on.

The gain in simplicity achieved by reducing the sample space to the two-point form may well be worth while in connection with the design problem, where a conspectus of many possible sample spaces may be needed. We have already made it clear that we are not discussing the design problem in this paper. But the defects of the two-point simplification are very evident, both in the example quoted above concerning the rail ticket number and some problems discussed below. Essentially, the absurdity arises because we are led, by the formalism, to lump together into a meaningless "average" likelihood functions of very different shapes.

4. Derivation of the Likelihood Principle

We now present an argument which makes no appeal to Bayesian considerations, but which indicates, at least, that the domain of applicability of the likelihood principle is very wide. We start from the principle of sufficiency, which tells us that two possible results x_1 and x_2 of the same experiment (S, Ω, f) give the same inference if the likelihood functions $f(x_1, \theta), f(x_2, \theta)$ are equivalent. For then we can represent the two results x_1, x_2 as ordered pairs, $(x., 1)$, $(x., 2)$ where $x.$ denotes "x_1 or x_2". Since the likelihood functions given x_1 and given x_2 are equivalent, we have

$$f(x_1, \theta) = cf(x_2, \theta)$$

so that, with an obvious notation,

$$f(x., \theta) = f(x_1, \theta) + f(x_2, \theta)$$
$$= (1+c)f(x_2, \theta),$$

while
$$f(1 \mid x., \theta) = c/(1+c)$$

and
$$f(2 \mid x., \theta) = 1/(1+c).$$

These relations imply that, when we know that the result is either x_1 or x_2, the further determination between the two possibilities amounts to the observation of an event whose probability, $f(1 \mid x., \theta)$ or $f(2 \mid x., \theta)$, does not depend on θ at all—that is, of an event which is irrelevant to θ. Thus this further determination can convey no information about θ, so that our inference about θ can be made from $x.$, without reference to whether it is $(x., 1)$ or $(x., 2)$. This implies that the inference from x_1 must be the same as that from x_2.

The sufficiency principle, which is one of the most profound of Fisher's many profound contributions to mathematical statistics, can be justified in many other ways than that indicated here (B). We shall also appeal to what may be called the principle of "moral impossibility", or, as it might well be called, "Bernoulli's principle", leaving some deliberate ambiguity concerning whether we refer to Jakob or Daniel. This is the principle underlying the simple test of significance, according to which events of vanishingly small probability can be considered morally, or practically, impossible. This tendency to disbelief in improbabilities exists, we think, in all of us, and is sufficiently well understood for our present purposes. The standard objection is,

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of course, that the sample space can be cut up into sets all having very small probability, though one of these *must* contain the observed point. The fact is, however, that we can distinguish between some of these small sets as being "relevant", while others are "irrelevant", in any given case ($\mathfrak{B}$).

Let us suppose, then, that we have two different experiments (S, Ω, f) and (T, Ω, g) having the same parameter space Ω, and suppose that the result x of the first gives a likelihood function equivalent to that given by the result y of the second:

$$f(x, \theta) = cg(y, \theta).$$

We aim to show that the inference from x must be the same as the inference from y.

We construct the "mixture" of the two experiments, in the proportion $p : q$ (with $p+q = 1, 0 < p, q < 1$), by first performing an auxiliary random experiment (with a biased penny, or a roulette wheel) which gives the result 1 with probability p, and the result 2 with probability q. If the result of this auxiliary experiment is 1, we then proceed with the experiment (S, Ω, f), while if it is 2, we proceed with the experiment (T, Ω, g). The result of the overall experiment can then be represented as a pair (i, u), where i is either 1 or 2, and u ranges over S if i is 1, and over T if i is 2. The result $(1, x)$ means that we get 1, and then, independently, get x, so that $(1, x)$ has probability $pf(x, \theta)$. Similarly, $(2, y)$ has probability $qg(y, \theta)$. The mixed experiment can therefore be denoted by the triplet $((1, S) \cup (2, T), \Omega, I(i)f + J(i)g)$, where $I(i) = 1$ if $i = 1$ and 0 if $i = 2$, while $J(i) = 1 - I(i)$. In the mixed experiment, $(1, x)$ gives the likelihood function $pf(x, \theta)$, while $(2, y)$ gives $qg(y, \theta)$. But these two likelihood functions are equivalent, since

$$pf(x, \theta) = (p/q)\, cqg(y, \theta)$$

and $(p/q)\, c$ is independent of θ. Therefore, from the sufficiency principle, it follows that $(1, x)$ and $(2, y)$ must yield the same inference.

But in this argument we have not required to specify p and q. We can therefore make p arbitrarily small, say 10^{-10}. Then our mixed experiment is morally certain to be the same as (T, Ω, g), and the result $(2, y)$ the same as y alone. Similarly, we can make q arbitrarily small, making $(1, x)$ equivalent to x. Then, since $(1, x)$ and $(2, y)$ yield the same inference, so must x and y.

To look at the argument of the preceding paragraph in another, perhaps more vivid, way, we can imagine that before we carry out an experiment we consider the possibility of investigating θ by another experimental method. Having weighed the pros and cons, we decide to do the experiment the first way. It seems clear then that, in interpreting the result of the experiment, we should not have to consider the possibility that we might have decided otherwise, and done the experiment the second way. This corresponds to saying that we need not distinguish between x and $(1, x)$.

In case the reader should feel uncomfortable at this point, we hasten to say that all we claim rigorously to have proved is, that the inference from $(1, x)$ must be the same as the inference from $(2, y)$. The argument that the inference from x must be the same as the inference from $(1, x)$ is, we recognize, less secure. Indeed, the argument may, in certain circumstances, fail, and the "continuity", to which we are appealing, as $p \to 1$, may be broken. Birnbaum (1962) has put forward an argument involving mixtures of experiments like our own (and we should here acknowledge our inability to recollect whether we got this idea from him, or whether we all got it from Cox (1958); in any case we owe a very great deal to discussions with Birnbaum). But Birnbaum goes on to

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set up what he calls the "conditionality principle", according to which we must identify the inferences from x and from $(1, x)$. But we are unconvinced of the general validity of this "conditionality principle".

As an example of what we have in mind, we may note that, like almost any other likelihood function, the expression (1.2) above can be transformed, by change of parameter, to coincide exactly with (2.1). We want a $1-1$ function $\theta = t(\alpha)$ such that

$$\log [\theta^3(1-\theta)^{17}] = -\alpha^2/2$$

and this can evidently be found by solving the differential equation

$$t'[(3/t) - 17/(1-t)] = -\alpha.$$

Then if we use this α to describe the penny, instead of θ, and if we happened to find 3 heads out of 20 throws, the likelihood function would be exactly the same as if we had measured α with an apparatus having standard error 1. But it is a special feature of the measurement type of experiment that the parameter space and the sample space are identical, and the kernel is symmetrical in x and θ. (In the case of several measurements there is a sufficient statistic having the same properties as x has, in the single observation case ($\mathfrak{B}$).) These special features enable us, in the absence of other knowledge concerning θ, to use the fiducial argument to derive probability statements about θ. Such special features are not present with α, so that this form of argument cannot be used.

In general, it is only when the triplet (S, Ω, f) can by itself be regarded as specifying all the inferential features of an experimental situation that the likelihood principle applies. If S and Ω are provided with related ordering structures, or group structures, or perhaps other features, it may be reasonable to apply a form of argument which would not apply if these special features were not present. The onus will, of course, be on anyone violating the likelihood principle to point to the special feature of his experiment and to show that it justifies his special argument. The "open", incomplete character of any valid system of statistical inference is very marked at this point, and this must be a disappointment to anyone concerned to mechanize thought processes. We should perhaps note that statistical inference shares this incomplete character with all other live systems of inference, including pure mathematics. A close analogy can be made between our likelihood principle and the principle of extensionality in the mathematical theory of sets. Although this latter principle has wide validity, and indeed was once thought to have universal validity, it has been found necessary to distinguish between "sets", to which it applies, and "classes", to which it does not apply, if contradictions are to be avoided.

To sum up, our position is that two experiments giving the same likelihood function give the same inference, unless one or other has a special feature which justifies the application of a mode of reasoning not applicable to the other. A mere change of sample space does not by itself justify a change of mode.

5. The Estimation of Regression and Autoregression Coefficients

We have argued above that the problem of inference concerning a parameter or set of parameters is usually best treated by plotting the likelihood function for the particular data obtained. This way of looking at things especially illuminates a problem that has developed an extensive literature in the last twenty years; that of inferences about the parameters of an autoregressive series.

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As our main illustration we will consider the one parameter autoregressive system

$$x_{t+1} = \beta x_t + \epsilon_{t+1} \quad (t = 1, 2, \ldots, n) \tag{5.1}$$

which, though simple, in fact presents the main features of the problem. A more general case is

$$x_{t+1} = \alpha + \beta_1 x_t + \beta_2 x_{t-1} + \ldots + \beta_k x_{t-k+1} + \epsilon_{t+1}. \tag{5.2}$$

In these systems we will suppose that the ϵ_t are independently distributed each with a normal distribution with mean zero and known variance σ^2. The distribution of each ϵ_t is independent of the lagged x_{t-1}, x_{t-2}, etc. on the right-hand side of the equation.

We will compare the system (5.1) with the system assumed for simple linear regression which we will write for convenience of comparison as

$$y_t = \beta x_t + \epsilon_t \quad (t = 1, 2, \ldots, n), \tag{5.3}$$

where the same assumptions are made about the distribution of the ϵ_t as above and they are assumed independent of the x_t. The x_t in this case are considered as completely fixed by the experimenter before the set of trials is started.

There are in fact two current theories of estimation of the parameter β in the simple regression model (5.3). On one of these theories, the maximum likelihood estimator is calculated, and its sampling error is found. The other theory is the Gaussian least squares theory. Under our assumptions about ϵ_t both these theories would lead to the same point estimate of β,

$$b = \frac{\Sigma x_t y_t}{\Sigma x_t^2}, \tag{5.4}$$

and this has sampling variance

$$\sigma^2 / \Sigma x_t^2. \tag{5.5}$$

However, if the ϵ_t are not normally distributed the theories may lead to different estimates.

Sometimes, instead of assuming the x_t are fixed beforehand, it is supposed that x_t are random variables, but with the y_t determined from the x_t as before. It is usually accepted that whether x_t are "fixed" or random does not affect the inference to be made from the experiment. If the x_t have a probability distribution, the sampling error quoted would be as in (5.5), using the x_t actually obtained in the particular trials being studied. The expression is not usually averaged over the sampling distribution of the x_t. This rule is a "common sense" one, but it is most easily understood when translated into a corresponding statement concerning the likelihood function. It has an analogue in the study of autoregressive systems, as we shall see.

If we drop the sampling distribution approach to inference, and consider instead that our task is to study the likelihood function, how would we treat observations from the simple regression system (5.3)? For given observations (x_t, y_t) the likelihood function is

$$\begin{aligned} L(\beta) &= \frac{1}{(2\pi)^{n/2}\sigma^n} \exp -\frac{1}{2\sigma^2} \Sigma \epsilon_t^2 \\ &= \frac{1}{(2\pi)^{n/2}\sigma^n} \exp -\frac{1}{2\sigma^2} \Sigma (y_t - \beta x_t)^2 \\ &= \frac{1}{(2\pi)^{n/2}\sigma^n} \exp -\frac{1}{2\sigma^2} (b-\beta)^2 \Sigma x_t^2 \exp -\frac{1}{2\sigma^2} (\Sigma y_t^2 - b^2 \Sigma x_t^2). \end{aligned} \tag{5.6}$$

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We are interested in this as a function of β, so that we can write

$$L(\beta) \propto \exp -\frac{\Sigma x_t^2}{2\sigma^2}(b-\beta)^2. \tag{5.7}$$

Thus b and Σx_t^2 are sufficient for β, and the likelihood function is a function of $(b-\beta)$. It so happens in this case that it has the same mathematical form (but for a multiplying constant which is irrelevant for likelihood functions) as the sampling distribution of b for given β. We wish to regard it as a function of β, however, obtained from the given sample. Provided the specification of the probability structure underlying the observations is correct, all that is known about β from the experiment is given by $L(\beta)$.

Although it is best to specify the exact form of the function, it may be useful to summarize some characteristic by quantitative measures. For example, the function is symmetrical about b, which can be used to specify "location". It is also useful to measure how sharply peaked the function is about b. If we compare the two likelihood functions L_1 and L_2 in Fig. 1, then L_1 represents more precise knowledge about β than does the curve L_2. From the mathematical form of the likelihood functions in

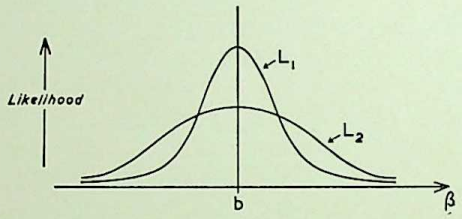

FIG. 1. Likelihood curves for linear regression.

this case, different likelihoods can be ranked uniquely in order of peakedness by the value of $\sigma^2/\Sigma x_t^2$. The smaller this quantity, the more sharply does the likelihood function drop away from its maximum, i.e. the more precisely is β known. It is this constant $\sigma^2/\Sigma x_t^2$ that we have called the "standard error" of b in Section 1.

Note that, as we anticipated above, the x_i's could have been fixed or determined by some random trial. If the latter were true then the likelihood function would merely be multiplied by $p(x_1, x_2, \ldots, x_n)$, the probability density of obtaining the set of x_i actually used in the trial. This does not contain β, and so it would merely appear as an irrelevant constant multiplying $L(b, \beta)$.

We now consider the autoregressive case given in equation (5.1), and we shall still assume that it is known that the ϵ_t are normally distributed, so that the likelihood function can be specified in full. The likelihood function in this case is

$$\frac{1}{(2\pi)^{n/2}\sigma^n} \exp -\frac{1}{2\sigma^2} \sum_{t=1}^{n} (x_{t+1} - \beta x_t)^2. \tag{5.8}$$

This function is of just the same form as the one in expression (5.6), but with x_{t+1} substituted for y_t. It follows that, if we define

$$b = \frac{\Sigma x_t x_{t+1}}{\Sigma x_t^2},$$

then b and Σx_t^2 are sufficient for β as before, and that the likelihood function is of the form (5.7). Thus in constructing the likelihood function, i.e. in making inferences

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from a particular sample series, there is no difference at all between the autoregressive case and the ordinary regression case, provided x_{t+1} is substituted for y_t. For the autoregressive system defined in (5.2) the same is true. The likelihood function is then

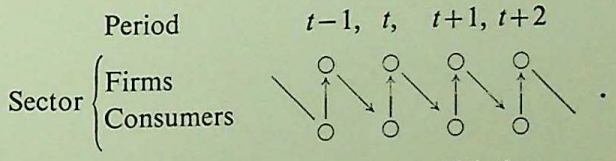
$$\frac{1}{(2\pi)^{n/2}\sigma^n}\exp-\frac{1}{2\sigma^2}\Sigma(x_{t+1}-\alpha-\beta_1 x_t-\ldots-\beta_k x_{t-k+1})^2,$$

which is the same as the likelihood function that would be obtained for a regression of y_t on $x_t, x_{t-1}, \ldots, x_{t-k+1}$. In fact the equivalence is true not only for linear regression, but for any form of regression. All we require is that each ϵ_t be independent of the lagged x_{t-i}.

We may contrast this approach with that of current theory, which treats the autoregressive case as being distinct from the ordinary regression case. In the ordinary regression case, the maximum likelihood estimator is "unbiased" but in the autoregressive case, as noted by Hurwicz (1945), the maximum likelihood estimator may have a bias, which can sometimes be quite considerable. This bias becomes vanishingly small if sufficiently long series are chosen. The reduction in bias for long series has been a consolation for those who have had to make inferences from time series. Yet there has been a worry that the basis for inference has apparently been different for time series than for ordinary regression. For one thing, in econometric work, where autoregressive series are most often used, the time series are often short, perhaps consisting of as few as ten annual observations, say. But there is another difficulty, which it is worth discussing at rather more length.

The workings of an economic system can be thought of as the interaction of a series of separate parts, or sectors. Economists will forgive the extreme simplification necessary to establish the general type of the model. Often the conditions in which a particular sector finds itself are determined by the behaviour of other sectors of the system. Let us take as an example a very simplified (Keynesian type) model of an economic system. There are two principal sectors, consumers and firms. Consumers are paid an income by firms, who employ them. On the basis of this income, which is here treated as an environmental variable, they plan their spending and saving in that period. The money which consumers spend in any particular period becomes the income of firms in that period. Partly on the basis of this income, partly perhaps on how it has been changing in the last few periods, and partly on the basis of technical innovations, etc., firms decide how many people to employ. This expenditure can be specified partly as expenditure for investment, and partly as expenditure to fulfil the consumers' current requirements (consumption expenditure), but whatever it is, it becomes consumers' income in the next period.

The scheme can be symbolized as follows in an arrow scheme:



The arrows represent the reaction of the sector from which they start. The arrows *into* a sector represent a part of the environment in which that sector has been placed.

Now let us suppose we have some hypothesis about how consumers react to their income

$$C_t = \alpha+\beta y_t+\epsilon_t \tag{5.9}$$

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where ϵ_t represents a "random" component arising from "outside" influences which do not persist. We also make the usual assumptions about ϵ_t—those given above.

The question now arises as to whether equation (5.9) is autoregressive or not. y_t is determined by the behaviour of firms. It may, as was suggested in the description above, depend more or less closely on consumers' expenditure in the previous period, C_{t-1}. It is even possible to imagine an economy for which $y_t = C_{t-1}$ so that equation (5.9) would reduce to an ordinary autoregressive series. But in the real world y_t is not usually very closely related to C_{t-1}, because of fluctuation in investment. Yet if we are to aim, for example, at getting *unbiased* estimates of β, it would be essential to know the relation of y_t to C_{t-1}, since this would help to determine the joint probability distribution of the C_t and y_t. The same would be true if we dropped the condition of unbiasedness, but looked for a minimum mean-square error estimator.

Often there is no real basis for specifying the relationship between y_t and the earlier C_t at all, or there is a strong presumption that it changed its form in the period of time which is being considered. This vagueness is of no importance provided we are clear that an inference about the parameters really consists in the plotting of the likelihood function for the particular y_t and C_t which we have actually observed.

Previous authors, and especially those inspired by Wold (1951), have quite rightly felt that inference from recursive schemes should present no special problems. Their efforts, however, have been directed to proving that the bias is small in many autoregressive cases. But the case above, which is a simplified version of one that is all but universal in economics, shows that this line of reasoning is not enough.

The following are examples from two other fields besides economics where similar conditions arise. Consider an agricultural trial, done in sequence in successive years, and suppose the results are presented to the statistician for analysis only after the whole series has been performed. The experiments consist of putting a measured amount of fertilizer on a plot each year (call this amount f_t) and measuring the yield of the crop y_t. We are interested in the relationship between the yield and the amount of fertilizer used, which we will suppose is of the form

$$y_t = \alpha + \beta f_t + \epsilon_t, \tag{5.10}$$

with a similar specification of the ϵ_t to that above.

Now, when we are presented with the results of this trial, suppose we are not told how the f_t were fixed. There are three main possibilities that we might consider:

(a) The f_t were determined completely before the trial began. This determination would, of course, include the order in which the various f_t might be used. It does not matter for the present argument whether the amount and order of application of fertilizer was determined by a randomization process, or by some other means. In this case, the model is one of straightforward regression.

(b) We might have to consider the possibility that the amount of fertilizer used was adapted in some way to the yields in previous years. For example, if the yield were high in any particular year, then the experimenter might cut down the fertilizer used in the next year, and if the yield were low he might increase it. Thus some feedback would be established between f_t and y_{t-1}. Alternatively, another form of feedback might be established if the experiment responded to high yields by increasing the fertilizer put on in later years.

If our choice of estimator and our specification of how good the estimator was were determined by sampling properties, then we would theoretically have to know the form of feedback. On this approach, consistency results proved by Mann and Wald

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(1943) show such knowledge to be irrelevant only in a "long" series of trials, and then only for special forms of feedback. But by insisting that the knowledge from the experiment is given by the whole likelihood function, such knowledge of feedback is strictly (and not only approximately, asymptotically) irrelevant.

(c) Another exactly analogous case arises in control systems, where loops and feedback invariably occur. Suppose there was a system with two "black boxes", A and B, as illustrated in Fig. 2. One may be interested in measuring the response

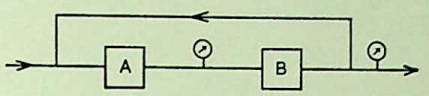

FIG. 2. Feedback loop.

characteristics of B. Does such a measurement also involve a knowledge or a simultaneous measurement of the response function of A? The details are similar to the previous cases, in fact the cases are formally identical (see, for example, Tustin, 1953). The likelihood principle tells us that measurements of the output of A and the output of B are all that we need. The response function of A is irrelevant.

We now note some general points on the measurements of autoregression coefficients.

(a) Much previous work has been based on the fact that the regression estimates are still the maximum likelihood estimates, even in the autoregressive case. But the arguments used did not suggest that such estimates should not be judged by their sampling properties. It was usually thought that the adequacy of the estimate should be judged by the nature of its sampling distribution, not by the nature of the likelihood function, or by a "standard error" such as the one described in Section 1, which summarizes the spread of the likelihood function.

(b) With inferences from a length of autoregressive series, it sometimes happens that a particular length obtained in one trial gives much information about a parameter, and at other times a trial from a series generated by precisely the same stochastic mechanism gives relatively little information. For example, it may happen that, in the system described in equation (6.1), Σx_t^2 happens to be small, since the x_t cluster close to zero. In such a situation the particular length of series we are studying will not contain much information about β. Its likelihood function will be widely spread. On the other hand, the x_t may contain values far from zero, so that Σx_t^2 is fairly large, and the series will contain more information about β, since the likelihood function will be less spread, i.e. β will have a lower "standard error" in the sense defined in Section 1.

It is at this point that the likelihood approach gives a different result from the sampling theory approach. The sampling theory approach cannot distinguish the different amounts of information in different series except as they arise from different initial values, whilst the likelihood notion does. Sampling theory can only specify a sort of average variability of the parameter over runs of the series started at the same value. But this is liable to be very deceptive. For example, a series may have a structure which usually gives a high value of Σx_t^2; x_0 may be far from zero, for example. The sampling variance of β may be supposed quite small. The particular sequence we are examining may have a small Σx_t^2 and so contain little information about β. Thus we would put much more faith in β than the series justifies. The reverse can also

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happen (though that is usually less dangerous). More extreme cases of this type of situation occur in the study of Markov chains, dealt with in the next section. The study of "observational" data is usually the study of one particular length of series, and it is important to know the accuracy implied by that particular length of series.

Of course, if we were designing an experiment with a length of autoregressive series, we would be interested in the sampling distribution of different likelihood functions which we might obtain. Even if we were using our observation for a decision procedure rather than for an inference problem, we might do better to base our decision on the whole likelihood function rather than on one particular aspect of it such as its maximum.

(c) If we study the likelihood function, no distinction need be drawn between samples from stationary series and samples from wandering or explosive series. Such distinctions were only drawn because sampling theory depended on asymptotic results, for which rather different analytic methods were required in different cases (Anderson, 1959).

(d) If σ is not known, we would have to obtain from our sample a joint likelihood surface for (in the simplest case) β and σ. If our attention were concentrated on β, we would take some average of the likelihood curves for β for different σ. If the joint likelihood is $L(\beta, \sigma)$, a natural way of doing this averaging is to find

$$\int L(\beta, \sigma) \frac{d\sigma}{\sigma}.$$

(e) We have assumed all the time that the distribution of the ϵ_t is known to be normal. In fact we often have to guess the form of the distribution. One thing we can do is to calculate the likelihood function on several relatively extreme assumptions about the form of the distribution. The likelihood function can be quite sensitive to such assumptions. For example (and this is quite a possible "extreme" assumption) if we assume that the ϵ_t have a rectangular distribution, we have a situation very much like that in the "playing card" example in Section 2, only with one degree more of complication. Here, too, the likelihood function can vary in width for different samples. The interested reader may like to work out the details himself. In particular cases an "inverse" method is sometimes possible. By examining the scatter diagram one may be able to see for what types of distribution of ϵ_t the more extreme values of β would still have as high likelihood as the middle (least squares) values.

6. Some Inference Problems for Markov Chains

We consider in this section some simple problems in which the data are recorded in discrete form, i.e. they may take one of S values which could be denoted by $0, 1, \ldots, S-1$. When $S = 2$, the sample consists of a binary sequence and a number of problems relating to data of this type have been described by Cox (1958a). In these situations the categories "0" and "1" might represent the absence or presence of some response, or else a discrete (in time) time-series in which a continuous response is being recorded might be quantized and the variable called "1" if it lay above the median, say, and "0" if it lay below. This device of quantization is used frequently in data processing in order to economize in computation and is called "clipping"; in the case where there are just two categories, the procedure is called "infinite clipping". As a typical statistical example of such quantization, we may quote the conversion of a sampling inspection problem for variables into one for attributes by perhaps

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arbitrarily rejecting the item if the response lies above a certain critical value and accepting if it lies below. Significance tests carried out in a mechanical fashion can also be regarded as examples of "clipping".

One of the simplest problems of this type is to examine whether the 0's and 1's could have occurred randomly or whether the probabilities p_{00}, p_{10} of a 0 being preceded by a 0 and a 1 respectively are different. However, it is not in the spirit of this paper to restrict the inference problem to this narrow framework but rather to base all inferences on the likelihood function. Assuming that the model is a first-order Markov chain, for a sample sequence of length n, the sample space S consists of the 2^n possible outcomes of the experiment and the parameter space consists of the unit square $0<p_{00}<1$, $0<p_{10}<1$ as shown in Fig. 3.

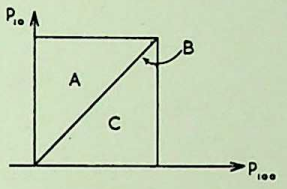

FIG. 3. Parameter space for first-order Markov chain.

Suppose that the transitions (i,j) are represented in the form of a 2×2 table as follows

		j: 0	j: 1	
i	0	n_{00}	n_{01}	$n_{0.}$
↓	1	n_{10}	n_{11}	$n_{1.}$
		$n_{.0}$	$n_{.1}$	$n_{..}=n-1.$

Then the likelihood function, given the first observation, is proportional to

$$L(x;p_{00},p_{01})=p_{00}^{n_{00}}(1-p_{00})^{n_{01}}p_{10}^{n_{10}}(1-p_{10})^{n_{11}}. \tag{6.1}$$

If it were possible to assume that the series had been started up a long time previously, this likelihood could be multiplied by the steady-state probability of obtaining a 1(0) if the first observation in the sequence were a 1(0). In practice we feel that such information is unlikely to be available and so we are forced to restrict ourselves to the likelihood function given by (6.1). Looking at this assumption a little more abstractly, it may be seen that it is equivalent to regarding the sample space S as being made up of two disjoint (mutually exclusive) sample sub-spaces S_0 and S_1. The former of these two spaces consists of all sequences of length n which begin with a 0 and the latter of all such sequences beginning with a 1. In basing our inference on the likelihood (6.1) we are confining ourselves to one or other of these two sample sub-spaces and hence we are violating slightly one of the conditions for the applicability of the likelihood principle, namely, that the sample space should not depend on the parameters being estimated (see Section 1). Since the form of this dependence is not known we are forced to work with the extended form of the likelihood principle (approximate equivalence) referred to at the end of Section 1 (𝔅).

The likelihood (6.1) is exactly the same as that for independent samples drawn from two binomial populations in which the probabilities of success are p_{10} and p_{00}

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respectively. It follows from the likelihood principle that the inference problem is the same in both cases. In particular, the problem of whether the 0's and 1's could have occurred randomly, i.e. $p_{00} = p_{10}$, is the same as the problem of deciding whether two independent binomial samples can be regarded as homogeneous. The usual procedure for investigating the "equality" of two proportions is to perform Fisher's "exact" test of significance or else to apply a χ^2 test. However, Stevens (1938) showed by considering the distribution of the number of groups of 0's and 1's in the ordered sequence that the problem of testing for independence of the type mentioned above could be reduced to an "exact" test using a slightly modified 2×2 table. Consideration of the likelihood functions shows immediately that the two problems are identical.

There is an extensive literature about tests for Markov chains (see, for example, Billingsley, 1961) but the above considerations show that a special inference theory is not required for these problems in the same way as it has already been noted that no new inference theory is required for autoregressive schemes.

Since the problem is two dimensional in this case, it is no hardship to plot the likelihood function in the unit square, or, more simply as far as interpretation is concerned, to plot the contours of constant likelihood in this region. Such a picture would tell us, for example, how much information the sample contains concerning the two parameters p_{00}, p_{01}. Thus, if sections of the surface for fixed p_{10} had a flat maximum and sections for fixed p_{00} had a sharper maximum, it would be possible to conclude that there was much more information in the sample concerning p_{10} than p_{00}. In this case the contours of constant likelihood would be elongated in the direction of the p_{00} axis and narrow in the direction of the p_{10} axis.

If one is now interested in deciding between the hypotheses $p_{00} = p_{10}$ and $p_{00} \neq p_{10}$, then considerations analogous to those discussed in connection with Anscombe's problem are necessary. It should be borne in mind, however, that there would sometimes be very little justification for forcing such a decision on certain samples and that the only inference possible from a scrutiny of a very broad likelihood function might be that more data were required. *Before the experiment was performed* this type of information is what would normally be studied by means of the power function. We do not deny the usefulness of such a study in the planning of an experiment but once it has been performed the likelihood function allows the data to speak for themselves, as it were.

It seems natural in this problem to use the approach referred to in Section 2 above and to approximate the likelihood function by a step function. For this purpose we divide the parameter space into three regions which we have denoted by $A(p_{10} > p_{00})$, $B(p_{10} = p_{00})$ and $C(p_{10} < p_{00})$ in Fig. 3. The values $(\lambda_1, \lambda_2, \lambda_3)$ of the step function in each region are the averages of the likelihood function in that region. It is readily shown in this case that

$$\tfrac{1}{2}(\lambda_1 + \lambda_3) = \frac{n_{00}!\, n_{01}!\, n_{10}!\, n_{11}!}{(n_{0.}+1)!\,(n_{1.}+1)!} \tag{6.2}$$

and

$$\lambda_2 = \frac{n_{.0}!\, n_{.1}!}{\sqrt{2n!}}. \tag{6.3}$$

It does not seem to be possible to express λ_1 and λ_3 separately in terms of known functions, but this is not necessary anyway since the likelihood surface will have been plotted and so a simple numerical integration formula could be used to obtain one of the integrals λ_1, λ_3 and the other obtained by subtracting from (6.2).

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As examples we consider the two sequences

$$M = 0111001010000101$$

and

$$M' = 0001100001111101.$$

The 2×2 tables of transition numbers are then

M

	0	1	
0	4	5	9
1	4	2	6
	8	7	15

M'

	0	1	
0	5	3	8
1	2	5	7
	7	8	15

so that $\hat{p}_{00} = 4/9$, $\hat{p}_{10} = 4/6$ and $\hat{p}'_{00} = 5/8$, $\hat{p}'_{10} = 2/7$. The likelihood odds $\lambda_1 : \lambda_2 : \lambda_3$ and $\lambda'_1 : \lambda'_2 : \lambda'_3$ for the two sequences are in the ratios $1{\cdot}71 : 1{\cdot}00 : 0{\cdot}49$ and $0{\cdot}39 : 1{\cdot}00 : 3{\cdot}59$ respectively, indicating some evidence that $p_{10} > p_{00}$ for M and considerably weightier evidence that $p'_{10} < p'_{00}$ for M'.

We now discuss briefly two problems of a more complicated nature. More detail will be left for later publication.

Problem 1: We are given two sequences which are known to be drawn from processes which may be described by first-order Markov chains and it is required to investigate the question as to whether they could have come from the same process. More precisely, if the two chains are characterized by transition probabilities (p_{00}, p_{10}) and (p'_{00}, p'_{10}) respectively, we need answers to the two idealized questions: "Is $p_{00} = p'_{00}$ and is $p_{10} = p'_{10}$?"

Problem 2: We are given a single sequence of 0's and 1's and it is required to ask the question as to whether they could have been generated by a first-order Markov process with transition probabilities (π_{00}, π_{10}) or alternatively by a second-order Markov process with transition probabilities $(p_{000}, p_{100}, p_{010}, p_{110})$. More precisely, we wish to ask whether $p_{000} = p_{100} = \pi_{00}$ and $p_{010} = p_{110} = \pi_{10}$.

For Problem 1, if we denote the sample sequences by x, y, the likelihood function given the first observation in both sequences will be proportional to

$$L(x, y; p_{ij}, p'_{ij}) = p_{00}^{n_{00}} q_{00}^{n_{01}} p_{10}^{n_{10}} q_{10}^{n_{11}} p'_{00}{}^{n_{00'}} q'_{00}{}^{n_{01'}} p'_{10}{}^{n_{10'}} q'_{10}{}^{n_{11}} \tag{6.4}$$

where $q_{ij} = 1 - p_{ij}$.

For Problem 2, the likelihood function of the single sequence, given the first two observations, is proportional to

$$L(x; p_{ijk}) = p_{000}^{n_{000}} q_{000}^{n_{001}} p_{100}^{n_{100}} q_{100}^{n_{101}} p_{010}^{n_{010}} q_{010}^{n_{011}} p_{110}^{n_{110}} q_{110}^{n_{111}}. \tag{6.5}$$

Since the likelihoods (6.4) and (6.5) are equivalent if we equate p_{00} to p_{000}, etc., it follows that the inference problem is the same and, in particular, the questions asked in connection with Problems 1 and 2 are formally identical. The likelihood surface is now 4-dimensional and hence difficult to visualize. For these questions it is reasonable to approximate this surface by means of step functions over the regions which are of direct interest. In general, these could be represented by numbers μ'_{ii} where $i = 1, 2, 3$ corresponds to the regions $A(p'_{00} > p_{00})$, $B(p'_{00} = p_{00})$ and $C(p'_{00} < p_{00})$ whilst $i' = 1, 2, 3$ denotes the corresponding regions for the pair of parameters (p_{10}, p'_{10}).

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The likelihood odds are thus expressed as a (3×3) matrix from which the statistician should be able to assess the weight of evidence in favour of the various hypotheses.

In these problems, however, there is a further important simplification characteristic of most Markov chain problems since the likelihood function factorizes in the form

$$L(x, y; p_{ij}, p'_{ij}) = L_1(x, y; p_{00}, p'_{00})\, L_2(x, y; p_{10}, p'_{10})$$

for Problem 1. It factorizes in a similar way for Problem 2. This means that the two questions asked above can be answered separately by quoting odds $\mu_1 : \mu_2 : \mu_3$ and $\mu'_1 : \mu'_2 : \mu'_3$. It follows that the odds μ'_{ii} factorize in the form μ_i, μ'_i but in general, of course, the questions asked will "interact" and it will be necessary to quote the matrix $[\mu'_{ii}]$.

If we now compare the sequences M and M', the odds may be calculated from the two tables

	00	01	
M	4	5	9
M'	5	3	8

	00	01	
M	4	2	6
M'	2	5	7

in the same way as the λ_i were calculated on p. 343. These are respectively

$$\mu_1 : \mu_2 : \mu_3 = 1{\cdot}469 : 1{\cdot}000 : 0{\cdot}480,$$

$$\mu'_1 : \mu'_2 : \mu'_3 = 0{\cdot}403 : 1{\cdot}000 : 3{\cdot}449.$$

It may be seen, therefore, that this is equivalent to splitting up the data into two 2×2 tables, one concerned with answering the question, "Is $p_{00} = p'_{00}$?" and the other with "Is $p_{10} = p'_{10}$?" The probabilities calculated from these two tables from separate tests of significance will not be independent but will be very nearly so. Asymptotically, this procedure will be analogous to the partitioning of a χ^2 with two degrees of freedom into two components each with one degree of freedom.

7. Finite Moving Average Processes

We discuss finite moving average processes, not because we think of them of great practical importance—we have yet to see a real, non-artificial time series which could plausibly be thought of in this way—but because discussion of them is traditional, and it serves to illustrate one or two more aspects of likelihood inference.

The simplest possible moving average process is one in which independent, identically distributed quantities ϵ_t are averaged in consecutive pairs:

$$x_t = \epsilon_t - \beta\epsilon_{t-1}, \tag{7.1}$$

where the sequence $x_1, x_2, \ldots, x_n$ is observed, and β is to be estimated. If we take a standard form $\phi(u)$, such as, for example, the standard normal density

$$\{1/\sqrt{(2\pi)}\} \exp -(u^2/2),$$

as specifying the "shape" of the distribution of the ϵ's, so that in any given case the density of the ϵ's is $\phi\{(\epsilon-\mu)/\sigma\}/\sigma$, where μ and σ are, respectively, location and scale parameters, and if we write $\lambda(u)$ for $\log \phi(u)$, then the log-likelihood based on the joint set $\epsilon_0, \epsilon_1, \ldots, \epsilon_n$ is

$$\sum_i \lambda((\epsilon_i - \mu)/\sigma) - (n+1)\log\sigma \tag{7.2}$$

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and this is tne same as the log-likelihood based on the set $\epsilon_0, x_1, x_2, \ldots, x_n$, provided

$$\left.\begin{aligned} \epsilon_0 &= \epsilon_0 \\ \epsilon_1 &= x_1+\beta\epsilon_0 \\ \epsilon_2 &= x_2+\beta(x_1+\beta\epsilon_0) \\ &\cdots\cdots\cdots\cdots\cdots\cdots \\ \epsilon_n &= x_n+\beta(x_{n-1}+\beta(x_{n-2}\ldots\beta(x_1+\beta\epsilon_0)\ldots)) \end{aligned}\right\} \tag{7.3}$$

obtained by inverting (7.1) above. We have merely to substitute in (7.2) from (7.3), to obtain this log-likelihood, because in this case the Jacobian of the transformation (7.3) (which is, in fact, 1) does not involve any of the unknown parameters; had it been otherwise, we should have added the logarithm of the Jacobian as an extra term. Now to obtain the likelihood based merely on $x_1, x_2, \ldots, x_n$, without ϵ_0, we have simply to integrate out ϵ_0, so obtaining

$$L(\beta, \mu, \sigma) = (1/\sigma^{n+1})\int \exp \sum_i \lambda(((x_i+\beta(x_{i-1}+\beta(\ldots+\beta(x_1+\beta\epsilon_0)\ldots))))).d\epsilon_0, \tag{7.4}$$

the integral being taken over the whole range of possible ϵ_0.

The general rule exhibited here is, that if the vector x represents what is directly observed, while the model specifies the distribution of u, by a probability function $f(u, \theta)$, say, and if there is a $1-1$ transformation from u to the pair (x, y):

$$u = T(x, y, \theta)$$

with Jacobian (x, y, θ), then the model specifies the distribution of (x, y) as $f(T(x, y, \theta), \theta)J(x, y, \theta)$, from which the marginal distribution of x is specified as

$$\int f(T(x, y, \theta), \theta)J(x, y, \theta)\,dy$$

and this, for given x, as a function of θ, is the likelihood function. Such an indirect approach to find the likelihood function is often useful.

Returning to our moving average, for any specific values of β, μ and σ, and specified ϕ. the likelihood (7.4) can be computed reasonably easily on an automatic computer. We begin by inserting a table of the standard distribution in logarithmic form, $\lambda(u)$, so that the values required can be obtained by "Table look-up" procedures; we then insert the observed values $x_1, x_2, \ldots, x_n$ in order, and compute the arguments of λ, then the values of λ. The integral may then be evaluated by Cotes's rule. Thus, for any specified form of distribution of the ϵ's, and (say) specified μ, it will not be difficult to make a computer draw contours of constant likelihood in the plane of (β, σ). It will commonly not be necessary to consider values of μ other than 0, since μ will usually be the mean of the ϵ's, and it will be assumed zero. However, if computing time is cheap, relative to observations, it will often be a good thing to check by computing likelihood contours for various values of μ—high values of likelihood for μ away from 0 will suggest that perhaps this assumption about the mean should be looked at again.

In practice we shall not know the form of the distribution of the ϵ_t and it will therefore be useful to have a set of tables giving different shapes of distribution, ready for insertion into the computer, so that the extent to which the behaviour of the

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of the distribution of ϵ away from the normal form, but that skewness in this distribution could upset them. They have all been made on the assumption that $\sigma = 1$. It can be seen that larger values of σ will tend to broaden the limits for β, while at the same time, if the shape remains fixed, the relative influence of the cubic and quartic terms is diminished. For a full analysis, contours of constant likelihood in the (β, σ) plane should be drawn.

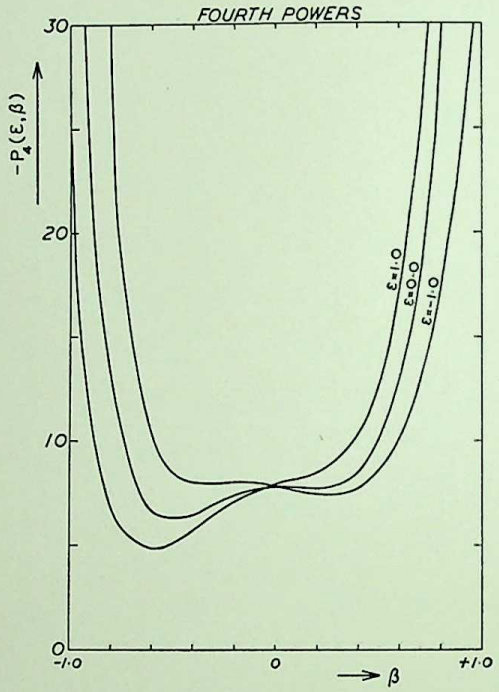

FIG. 6. Quartic terms in log-likelihood for moving average data.

We may note that the likelihood function as given by (7.4) above, even in the normal case, is rather complicated, in that it involves the calculation of *all* the serial correlations of the sample. The amount of computation may be substantially reduced by using the device referred to in Section 3 in a way used by Box and Jenkins (1962).

8. THE PROBLEM OF MANY PARAMETERS

The general theme of this paper has been that, when we are concerned with comparisons between different sets of parameter values, on the basis of experimental data, we should exhibit the likelihood function defined over the parameter values by the data. If there is only one parameter, this can easily be done by means of a graph, while if there are two, contours of constant likelihood can be drawn. But if there are three parameters, sets of contours are called for, or solid models, while if there are more than three parameters, the problem of exhibiting the function in this form becomes unmanageable.

Statistical inference is not, of course, the only branch of applied mathematics in which problems concerning the description of functions of several variables arise, and there are many methods of dealing with them, most of which are applicable to likelihoods. We have already indicated how approximations by step functions can be

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used, and how approximations by normal probability densities can be used. We now wish to mention a few of the devices peculiar to likelihood which may be brought into play.

The first relies on the fact that statisticians are accustomed to the operation of the law of multiplication of probabilities, which means that they can readily grasp the general course of a function of several variables which can be expressed in the form of a product of functions of one or two variables. Thus in the simple analysis of variance between and within groups, with equal numbers n in each group, the group means μ_i have likelihoods which, for each value of σ, the common standard deviation, are independently normally distributed about the respective estimated means m_i. Their likelihood function, for given σ, can therefore be represented by setting out the m_i along a line, and sketching alongside, on the same scale, the density function for the normal distribution with standard deviation $\sigma/\sqrt{n}$ associated with each μ_i. If one imagines a set of normal probability distributions centred at the m_i, with the given standard deviation $\sigma/\sqrt{n}$, it is not difficult to visualize the relative probabilities of the different sets (x_i) obtained by taking samples of one each from each of these distributions. The relative likelihoods for the μ_i will behave like the relative probabilities of the x_i. One can thus assess, for example, the likelihood that the rank order of the μ_i will differ, in various prescribed ways, from the rank order of the m_i, what the chances are against certain sets of the μ_i all having the same value, and so forth. One obtains in this way an approach to the analysis which is, in our opinion, a considerable improvement on the various rules associated with "least significant differences" and the like, all of which seem to us open to the charge of oversimplification, even when the rules themselves are very complicated.

When the likelihood factorizes it is possible to make statements about the relative likelihoods of two values of one parameter, without reference to the value of the other parameters: If

$$L(\alpha, \beta) = M(\alpha)\, N(\beta),$$

$L(\alpha, \beta)/L(\alpha', \beta) = M(\alpha)/M(\alpha')$, which does not depend on β. Conversely, it is strictly only when the likelihood factorizes in this way that the parameters are separable, and separate statements can be made about them. Our tendency to think of parameters one at a time perhaps arises, biologically speaking, from the fact that likelihoods can often be represented, at least approximately, in the product form. But now we must discuss cases where this fortunate circumstance is not present.

If the likelihood does not factorize, it may be that the fact simply has to be faced that no separate consideration of the parameters is possible. In such a case, we should say so and resist any pressure to misrepresent the facts. We statisticians have wasted too much time in the past trying to give reasonable answers to unreasonable questions. But it is worth while briefly to examine what form any statement of likelihood must take, if it is to refer to one parameter only and not to the other. We shall outline the argument here, reserving fuller discussion for another occasion.

We therefore suppose that we have a likelihood function $L(\alpha, \beta)$ involving two parameters, and we wish to obtain what might be called a "pseudo-likelihood" $M(\alpha)$ from it, involving only α, eliminating β. We can write

$$M(\alpha) = \mathscr{E}\{L(\alpha, \beta)\},$$

where $\mathscr{E}\{\,\}$ denotes the operation of eliminating β. It can be seen that for each fixed α, $\mathscr{E}\{\,\}$ is a functional. We shall require our "pseudo-likelihood" to behave, as nearly

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as possible, like a genuine likelihood, so that the following conditions are imposed on $\mathscr{E}\{\}$:

(1) If $L(\alpha,\beta) = M(\alpha)\,N(\beta)$, then $\mathscr{E}\{L(\alpha,\beta)\}$ is equivalent to $M(\alpha)$,

(2) If $L(\alpha,\beta) \leqslant L(\alpha',\beta)$, for all β, then $\mathscr{E}\{L(\alpha,\beta)\} \leqslant \mathscr{E}\{L(\alpha',\beta)\}$.

Further, by considering a mixture of experiments in the proportion $p:q$, or by considering a further parameter γ which takes only two values, we can show that we should have

(3) $\mathscr{E}\{pL(\alpha,\beta)+qL'(\alpha,\beta)\} = p\mathscr{E}\{L(\alpha,\beta)\}+q\mathscr{E}\{L'(\alpha,\beta)\}$.

From conditions (1)–(3) it follows that $\mathscr{E}\{\}$ must be a linear functional, and so, by the Riesz representation theorem, we must have

$$\mathscr{E}\{L(\alpha,\beta)\} = \int L(\alpha,\beta)\,dW(\beta)$$

where $W(\beta)$ is some measure function over the values of β.

Thus the only way of eliminating parameters we do not wish to discuss is to integrate them out. The choice of weight function in the integration will often be more or less arbitrary, but it is worth noting that we can often remove this arbitrariness by appeal to an invariance or homogeneity argument. Thus, for example, if β is a location parameter, and we want our statements about α to be independent of choice of origin for β, the only way to do this is to take

$$dW(\beta) = d\beta,$$

while if β is a scale parameter, and we want our statements about α to be independent of scale, we must take

$$dW(\beta) = d\beta/\beta.$$

More generally, in such cases, we must use the group invariant measure.

An example of this procedure is afforded by the simple between and within groups analysis of variance already mentioned. If we wish to eliminate σ, we integrate the likelihood with respect to $d\sigma/\sigma$, and in this way the density associated with a given μ_i becomes the density function for Student's t, instead of the normal density function. The factorizability of the joint likelihood is also lost, although the dependence between the μ_i is not great unless n is very small.

9. Concluding Remarks

We promised at the beginning to justify restricting our general arguments to cases where the probability distributions involved were discrete. To do so in full would take us too far afield, so we shall confine ourselves to exhibiting an example which shows that unless a continuous distribution is regarded all the time as arising from a *specific* limiting process applied to a sequence of discrete distributions, contradictions with generally accepted principles of inductive inference may result. Specifically, it becomes impossible to identify the conditional probability $\Pr[A\,|\,B]$ with the probability of A when B is known to be true.

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Suppose x and y are independent and each is uniformly distributed between -1 and $+1$, so that their joint density is

$$\phi(x,y) = 1/4, \quad -1 < x,y < +1$$
$$= 0 \text{ otherwise.}$$

Define new variables u, v by the $1-1$ transformation

$$u = x,$$
$$v = y/(1+x^2).$$

Then the Jacobian is $(1+u^2)$ and the joint density of u, v is

$$\xi(u,v) = (1+u^2)\begin{cases} -1 < u < +1 \\ -1/(1+u^2) < v < 1/(1+u^2) \end{cases}$$
$$= 0 \text{ otherwise.}$$

Now consider the conditional distribution of x, given $y = 0$, and the conditional distribution of u, given $v = 0$. If they are to be uniquely defined they must have densities 1 and $K(1+u^2)$ for $-1 < x, u < +1$ which, of course, are different. But u is the same as x, and the condition $y = 0$ is logically equivalent to the condition $v = 0$, so that one would expect the conditional distributions to be the same.

The paradox can be overcome if we suppose that the continuous distribution of x and y is the limit of discrete distributions obtained, for example, by imposing uniform meshes on the x and y axes. Such a uniform mesh for x and y will correspond to a non-uniform mesh for u and v, and the non-uniform conditional probability of u arises from this non-uniform mesh.

We would like to make a final remark on the relationship between likelihood inference and tests of significance. We have already, we hope, made it clear that we do not consider these modes of inference as mutually exclusive, and we wish to stress now that they may be regarded as complementary. We have mentioned above the possibility that the mathematical model assumed for an experiment may be contradicted by the facts of experience, but, like any other defect in the model—non-normality for instance—so long as the defect can be specified, it can be represented as an enlargement of the parameter space by the introduction of parameters to specify lack of independence, departure from normality, and so forth. We should thus regard the actual parameter space over which our likelihood function is considered as perhaps surrounded by a penumbral shadow, corresponding to the assumptions we have chosen to make. It may be that such assumptions are based on knowledge external to the experiment in question; but it can also happen that the data themselves tell us something about a reasonable delimitation of the sample space. Thus we may start with a very large parameter space Ω and then, when the result is to hand, we may delimit ourselves to a subset Ω' of Ω—as, for example, with the moving average considered above. It would be foreign to the spirit of tests of significance to make the boundary of Ω' too precisely defined, but for the sake of mathematical definiteness we might imagine that this boundary was chosen as a contour of constant likelihood. If we did this we would be using a "significance test" of the Wilks ratio type, since we would be saying, in effect, that

$$\Pr\left[f(x,\theta) \Big/ \sup_{\Omega} f(x,\theta) < C \,|\, \theta \right],$$

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where C specifies the likelihood contour, is so small that θ must be rejected. Thus the significance test may be used to delimit the domain of consideration, while, once this is fixed, the likelihood principle is applied.

We hope we have shown that the concept of likelihood is well adapted to use in a great many problems of statistical inference, and that the practice of considering the whole course of the likelihood function has for too long been neglected.

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Discussion on the Paper by Professor Barnard, Dr Jenkins, and Mr Winsten

Professor J. Durbin: A new paper on statistical inference by Professor Barnard, Dr Jenkins and Mr Winsten is a major event and it gives me great pleasure to have the opportunity of welcoming it. Whether or not one agrees with the findings of the paper, one thing we can all agree about is that the sustained interest in and investigation of the fundamentals of statistical inference by Professor Barnard and his colleagues over a long period of years have been a major service to the subject. The findings of these investigations are presented in this paper in a new and challenging form; the authors' claims, if accepted, would have dramatic and far-reaching implications. It is therefore in a spirit of some excitement that one approaches the task of examining the arguments on which these claims are based.

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The central thesis of the paper is the claim that the inference from a sample is determined solely by the likelihood function. More specifically, if x and y are two different samples, possibly arising from different experiments, and the likelihoods calculated from them are in the same ratio for all values of the unknown parameters, then the inference from the two samples must be the same. The implications of this claim are very far-reaching and in complex situations involving a number of parameters, some of them nuisance parameters, I believe that a number of technical difficulties would be encountered in attempting to implement the principle in practice. However, the main points at issue are concerned with foundations and can be adequately discussed in terms of simple examples involving one or two parameters.

I should like to begin, therefore, by going back to one of the simplest real-life problems of statistical inference known to me, that of estimating the mean of a finite population. Suppose you wanted to investigate the mean number of cigarettes smoked per day by individuals over 21 in a certain town. One thing you might do is to interview a sample of 100 individuals drawn at random from the names on the electoral register for that town. The population mean would then be estimated by a quantity, q say, calculated from the observations; q will of course often be the sample mean. In addition to making an estimate we generally require also a measure of the accuracy of the estimate. The way we normally obtain this is to consider the sampling distribution of q over the space of all possible samples of 100 individuals. The Likelihood Principle explicitly rules out a consideration of the sampling distribution as a basis for inference. It is hard for me to see how one could write down the likelihood function for this particular example since there is no well-formulated model for the population. I would be interested to know whether the authors would, in this instance, be prepared to base their inference on the sampling distribution of q.

Suppose now that of the 100 individuals 58 turn out to be men and 42 women and that we are interested in the mean number of cigarettes for the male population only. To estimate this one might use the sample mean $\bar{x}$, say, of the 58 observations. To assess the accuracy of $\bar{x}$ I personally would consider the sampling distribution of $\bar{x}$ conditional on $n_1 = 58$ fixed, even though the number of men n_1 is a random variable which varies from sample to sample. This is, I believe, the method generally taught in elementary courses in sample survey methodology. Once again, I should be interested to know whether the authors would be prepared to use a sampling distribution, in this case a conditional one, as a basis for inference.

This second example enables me to emphasize the point that *even at the most elementary level* the orthodox analysis based on sampling distributions makes a strong distinction between the sampling distribution envisaged before the experiment, i.e. the unconditioned distribution, and that constructed after the examination of the data, i.e. the conditional distribution. n_1 is in fact what Fisher called an ancillary statistic; in Fisher's terminology it tells us nothing about the values of the population mean but it tells us something about the accuracy of an estimate; what one should do is to consider the conditional distribution of the estimate given the ancillary statistic. The recognition and exploitation of suitable ancillary statistics for conditional inference is part of the job of statistical analysis and in my view far too little attention has been given in the literature to the development of an appropriate rationale. Whatever one's view of the likelihood principle or of the Bayesian approach, there is no doubt in my own mind that Professor Barnard has been right in emphasizing so strongly over a period of many years the importance of the distinction between probability calculations before the experiment and inference calculations after it.

In the light of these remarks let us look at the example discussed by the authors on p. 326. The example concerns a discrete rectangular population with unknown lower terminal $\theta + 1$ and known range 12. Given a sample of size 10 we have to make inferences about θ. As the authors point out the smallest value L and the greatest value H are jointly sufficient for θ; however, there is no single sufficient statistic. We see at once that the sample range $H - L$ is distributed independently of θ and therefore tells us nothing

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about θ. On the other hand, it tells us a great deal about the accuracy with which θ can be estimated from this particular sample. In particular, if $H-L = 12$ then θ can only have one value, namely $L-1$; if $H-L = 11$, there are only two possible values $L-1$ and $L-2$ and so on. Clearly $H-L$ is an ancillary statistic in Fisher's sense and in my view the sensible thing to do is to base one's inference on the conditional distribution of L given $H-L$. This turns out to be uniform on the integers $\theta+1$, $\theta+2$, ..., $\theta+13-H+L$ and so resembles very closely the authors' likelihood function. On the basis of this conditional distribution it is an easy matter to construct tests and confidence statements which appear to me quite sensible. The problem seems to me to be very similar to that of making inferences about the mean number of cigarettes smoked per day by men given a sample of men and women which I mentioned earlier. If one is prepared to admit the legitimacy of the sampling distribution as a basis for inference in that case it is hard to see why one should be adamant about it in the bus-ticket example, especially since the sample was chosen by an objective randomization procedure in each case.

Finally I should like to discuss what seems to me to be the most searching example presented in the paper, that of the autoregressive model. I think the authors deserve a special vote of thanks for producing such a brilliantly chosen test case for their ideas, on the ground that anyone who accepts their treatment of this case will, I feel, have little difficulty in accepting their ideas *in toto*. I would like to base my remarks on the simple case of two observations y_1 and y_2 corresponding to the model

$$\begin{aligned} y_1 &= \alpha+\beta x_1+\epsilon_1, \\ y_2 &= \alpha+\beta x_2+\epsilon_2 \end{aligned} \tag{1}$$

where ϵ_1, ϵ_2 are independent $N(0, \sigma^2)$ (known σ^2), and, to begin with, x_1, x_2 are distributed independently of ϵ_1, ϵ_2. The estimator of β is

$$b = \frac{y_2-y_1}{x_2-x_1} = \beta+\frac{\epsilon_2-\epsilon_1}{x_2-x_1}.$$

One recognizes x_2-x_1 as an ancillary statistic, and subject to this fixed, one sees that b has sampling mean β and standard error

$$\frac{\sigma\sqrt{2}}{|x_2-x_1|}.$$

Consider now the corresponding autoregressive model

$$\begin{aligned} y_1 &= \alpha+\beta x_1+\epsilon_1, \\ y_2 &= \alpha+\beta y_1+\epsilon_2 \end{aligned} \tag{2}$$

where everything is as before except that x_2 has been replaced by y_1. The likelihood principle tells us that the inference concerning

$$b^* = \frac{y_2-y_1}{y_1-x_1} = \beta+\frac{\epsilon_2-\epsilon_1}{\beta x_1+\epsilon_1-x_1} \tag{3}$$

should be the same as that concerning b. I must confess I find it hard to accept this. Certainly the sampling distributions are very different whether one considers conditional or unconditional distributions; for example, that of $b^*-\beta$ depends on β whereas that of $b-\beta$ does not. The only ancillary statistic we can recognize in connection with b^* is x_1, and the sampling distribution of b^* given x_1 would be very complicated indeed. The difficulty arises partly because of the presence of β in the denominator of the last term of (3) and partly because ϵ_1 occurs in both numerator and denominator.

I think one can see that the inference must be different for the two cases by considering a comparison between the results of two samples having the same value of x_1, but otherwise different, for each case. Let us indicate the results obtained from the second sample by

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primes. If it turned out that $|x_2'-x_1|$ were very much greater than $|x_2-x_1|$ it would, I think, be reasonable to infer that the estimate b' obtained from the second sample was probably much more accurate than b. If, however, for the second model it turned out that $|y_1'-x_1|$ were very much greater than $|y_1-x_1|$ one would certainly not be justified in inferring that $b^{*\prime}$ was probably much more accurate than b^*, since the high value of $|y_1'-x_1|$ might, for example, have arisen from a high value of ϵ_1' and this would affect the numerator as well as the denominator of $b^{*\prime}-\beta$ as can be seen from (3). For these reasons I am unable to accept the claim that inferences for autoregressive models should be the same as inferences for the corresponding regression models.

I should like to conclude by making a brief remark about the derivation of the likelihood principle from the sufficiency principle given in Section 4 of the paper. The argument here is so appealing that one has to search very hard in order to spot the fallacy. It seems to me that the reason why the argument breaks down arises from the authors' failure to specify the sample spaces over which their statistics vary. I do not believe it is enough to know the values of the sufficient statistics in order to make a satisfactory inference; I believe one must also specify the sample spaces over which these statistics vary. If one accepts this point it will be found that the argument of Section 4 no longer follows. A similar remark applies to Professor Birnbaum's derivation of the likelihood principle in the paper referred to by the authors. Once one is prepared to drop the requirement that the sample space be stated the likelihood principle becomes almost self-evident.

It is obvious from my remarks that I am unable to accept many of the arguments of the paper. This does not, however, prevent me from proposing a very warm vote of thanks to the authors for an important and most stimulating paper.

Professor P. WHITTLE: The reading of this paper has forced me to attempt to clarify my own ideas on these subjects, and for that reason I should like to acknowledge some personal debt of gratitude straightaway.

It is the first part of the paper, dealing with likelihood inference, which is debatable. If one grants the conclusions here, then the applications to time series analysis follow readily enough. So, any discussion must amount to another discussion on the bases of statistical inference; something not to be undertaken lightly.

The authors' likelihood principle is plausible, but I feel that at no point do they really justify it. It must be justified by appeal to something more basic, just as the least-square principle, which is in itself plausible, must be justified by a proof that it yields consistent (or, indeed, optimal) estimates. There are two obvious arguments for the likelihood principle, both of which the authors mention, but both of which they seem to eschew.

The first justification would be the Bayesian one; in this the likelihood is seen as the dominant component of a posterior distribution of parameters. If the situation were such that the idea of a prior distribution in the frequentist sense could be accepted, then the posterior distribution would also have a direct frequentist interpretation, and so could not be questioned.

A second possible justification would be a sampling one; if there are p parameters and certain regularity conditions are fulfilled, then the Wilks statistic

$$-2\log\frac{f(x,\theta)}{\max\limits_{\theta} f(x,\theta)}$$

is approximately distributed as χ^2 with p degrees of freedom. If we accept all θ giving a non-significant χ^2, we have an acceptance region enclosed by a contour of constant likelihood (in θ-space). This is not necessarily a confidence interval, although it will behave as one asymptotically (cf. Professor Bartlett's paper on approximate confidence intervals (Bartlett, 1953)).

However, as I said, the authors kick these crutches aside, and in Section 4 base a derivation of the likelihood principle on the idea of sufficiency. They embody their

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conclusion in the sentence: "Therefore, from the sufficiency principle, it follows that $(1, x)$ and $(2, y)$ must yield the same inference."

Everything depends on what is meant by this last phrase; presumably it implies more than the initial assumption that the two likelihoods are equivalent functions of θ. The authors are also presumably not thinking of sampling properties when they speak of "yielding the same inference", because these obviously vary. Consider the binomial example: for the positive binomial, θ, the probability of success, has a minimum variance unbiased estimator; for the negative binomial this is true of θ^{-1}; for the mixed case, when both n and r are random variables, there is no such estimator at all. But if the authors are not thinking in the sampling sense, what do they mean by the phrase "yields the same inference"?

I think that the point at issue can be exhibited as follows: the authors' conclusion is effectively that in the mixed experiment the likelihood can be factorized as

$$f_1(x; \theta) \quad f_2(x, d; p)$$

where d is the random variable taking values 1 or 2 with probabilities p and q according to whether the experiment is carried out the first way or the second, and x is the outcome of the sample itself (e.g. n and r in the binomial case; the authors' x and y are subsumed in our single x). One sees immediately that x is sufficient for θ, and that d is irrelevant (unless one is to make inferences in a conditioned sample space, when the idea of a mixed experiment would immediately be lost). However, the distribution of x will in general depend on p as well as upon θ, so that one can only say that x yields the same inference, irrespective of the value of d, if p is held constant. The authors are attempting to relate situations in which the value of p varies, which is not valid. This point is, I believe, equivalent to the one made by Professor Durbin towards the end of his remarks.

This rather inexplicit acceptance of the likelihood principle has its dangers. Consider, for instance, the estimation of β in formula (5.9). The authors state that one will obtain valid inferences by plotting the likelihood of the C_t, conditional on the values of the y_t, by which I assume they mean something proportional to

$$\exp -\frac{1}{2\theta^2} \sum_1^n (C_t - \beta y_t)^2$$

(if we omit α, for simplicity), despite the fact that y may be related to C by a feed-back relation of the type

$$y_t = \gamma C_{t-1} + \epsilon_t'.$$

In large samples they are going to find their likelihood function centred on the β value

$$\hat{\beta} = \frac{\Sigma C_t y_t}{\Sigma y_t^2}$$

which will not tend to β if ϵ_t and ϵ_t' are correlated, and $(\beta\gamma)^2 \neq 1$. Even if the authors are not interested in point estimates, this is an inconsistent result under any interpretation.

I have perhaps rather emphasized this one deviation, because in most cases the authors' principle will give results in accord with asymptotic sampling theory. In fact, their idea of examining the slope of the likelihood around its maximum is largely equivalent to the calculation of a maximum likelihood estimate, and to the subsequent calculation of the information matrix directly from observed likelihood derivatives rather than from expected likelihood derivatives computed on parameter estimates. This is a reasonable procedure in large samples, and would, for instance, make the actual calculations for a Markov or a multinomial model identical, even though analogous statistics would in general have different distributions in the two cases. On the other hand, it must be admitted that the authors' graphs of log-likelihoods which are flat-topped rather than parabolic indicate features not envisaged in the classic asymptotic theory.

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Despite the fact that I do not accept the authors' principal argument, I found the paper a most stimulating and interesting one, and am certainly not alone in this. I warmly second the vote of thanks.

The vote of thanks was put to the meeting and carried unanimously.

Professor M. S. BARTLETT: I should like to add my congratulations to those of the proposer and seconder for this very stimulating paper. I think, whether we agree with it or not, it does make us examine the foundations of statistics.

One or two of the remarks I was going to make have been rather anticipated, but perhaps I could make them over again, despite that, in a different way.

It seems to me that the justification for the likelihood principle as the authors give it is steering a somewhat unsteady course on the fence between two standard approaches. You have the statistical or sampling interpretation on the one side which, as an "orthodox statistician", I regard as firm ground, and you have the Bayesian bog on the other (the Bayesian no doubt reversing the types of ground on the two sides); the authors seem to be sitting on the fence in the middle. It is a question of whether they really do justify the likelihood principle more than that. And, as indicated also by Professor Whittle, it seems to me that you cannot really interpret the likelihood function except in these two ways, either in relation to posterior probability, which is the Bayesian approach or, on the statistical approach, you discuss the sampling distribution.

My feeling over the last few years is that one cannot really attempt to tell other people how they are going to make inferences, and I do not think we shall come to any common opinion tonight on this question; for this reason it seems to me that statisticians should at least hope to agree on the large sample case while there will always be differences in the small sample case. If you are a Bayesian, you introduce prior probabilities; if you are not, you leave them out and discuss the frequency interpretation in terms, say, of confidence intervals and so on, but for large samples one really should agree, and I have no reason to suppose we do not.

To come down to detail, I think the authors' claim of simplification for their approach is not altogether justified from the practical angle. Let us take the first example, already commented on by Professor Whittle, where there is either a random number R of successes to a fixed number n of throws or, in the inverse case, a random number N of throws to a fixed number r of successes. While the likelihood function is formally the same, the sampling interpretation gives an optimum estimate R/n for p in the first case, and N/r for $1/p$ in the second.

I did not altogether follow what Professor Whittle was saying about the mixed experiment. As I see it, in the asymptotic case, if you were using small numbers but repeating the experiment several times, then it is true that if you built up the likelihood function completely in either case, you would make a reasonable asymptotic inference. But I think it would be natural for a statistician to want to work from his separate estimates for the individual experiments, and if so it would be important for him to work with the respective estimates I have mentioned. The sampling or "stopping rule" does become important if he wishes to proceed with his inference on this basis rather than keep the likelihood function the whole time.

Another point raised by Professor Whittle was the use of the maximum log-likelihood. You may not want to consider merely the relative likelihood of one value of a parameter to another, but the appropriateness and relevance of the whole function—in other words, a goodness-of-fit test. This at once brings in the sampling properties of the likelihood function, and in particular the sampling properties of maximum likelihood estimates.

Mr D. G. CHAMPERNOWNE: I find myself more in agreement with the authors of this paper than did the three previous speakers—and I hope this will not alarm the authors unduly!—but I do not agree with them altogether. I particularly welcome the clear distinction

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that they draw between the two bases of any inference, namely, the various conventional assumptions which one adopts, and the experimental evidence. Both these bases are needed, and it is a great aid to clear thinking to set out the distinction between them, and also to set out what we assume without evidence, and what is the relevant evidence. Even when probabilities do not come in at all, we depend on both assumptions and evidence for advancing our knowledge.

If you take the possible states of the world in which we are interested as a set of points in a region, the acquisition of further evidence enables us to eliminate some of these previous possibilities as now impossible in the light of the evidence and, as the evidence piles up, we eliminate more and more of the previous possibilities; ideally, we would hope to narrow the possibilities down to a single point by the collection of further and further evidence, in which case we should have come to know all about our subject. But, of course, in general we merely cut down the size of the region; we cut down the set of possibilities by the acquisition of further evidence.

But even in this case of definite knowledge, where probability does not come into it, we depend on all sorts of assumptions and conventions; for example, the convention about what can be accepted as reliable evidence and what cannot. For example, we believe what we see with our own eyes, but we do not believe all we read in the newspapers.

When we seek probability statements, as opposed to a definite "Yes" or "No", we seek not merely to eliminate possibilities but to choose between the likelihoods of the various possibilities that remain open to us. This attempt to attribute different likelihoods to different possibilities may well be attempting the impossible, but, by making heroic assumptions, something can be done. These assumptions and conventions, again, are of many kinds.

We may assume, for instance, that certain things in real life, the results of certain experiments, behave like random variables; or, again, that the past is, in a certain sense, a guide to what will happen in the future; or, to take an example from the paper, we assume the functional form of certain probability distributions. Then a fairly general principle is, that if we compare the probability of one joint effect that both A and B happen with the probability of another joint effect—the probability that both A and C happen, it is fair to assume that the relation between these two probabilities will be unaffected by the actual occurrence of the part common to the two, namely A. It would be rather odd, for example, when the probability of (A and B) exceeded the probability of (A and C), if the actual occurrence of A might reverse the order of these probabilities, so that the probability of (A and B) became less than the probability of (A and C).

Again, we adopt some conventional representation, perhaps, of the residual part of complete ignorance, that which we really know nothing whatever about. For example, Bayes suggested regarding a set of discrete alternatives as equally probable if we knew absolutely nothing whatever about them which might indicate what the relative probabilities were. This is simply a conventional representation of our ignorance and presumably was suggested because nothing better can be suggested, and this is the simplest to state.

Naturally, different practitioners are at liberty to adopt different conventions and assumptions, and it is to be expected that different schools will arise advocating different "cookery books". It is, however, useful to understand just what separates the different schools.

For example, as has already been pointed out, the methods advocated this afternoon faintly resemble methods once advocated by Sir Ronald Fisher. They also resemble methods advocated once by the Reverend Bayes. But I cannot decide which they resemble more. (Perhaps there is a kind of prior distribution of belief between the authors of the paper, which may account for the confusion in my mind.) In order to clear up this point, I should like to end by posing a list of questions, the answers to which I think might help one to choose.

My first question concerns the statement on p. 330, where the authors claim that the uncertainty we feel about a hypothesis is measured by the likelihood. I cannot understand

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how the likelihood of a value of θ (which is, I think, just the probability of the actual result, given that value of θ) can really be supposed to measure the uncertainty we feel; because the uncertainty we feel is something apart from the particular function Prob $\{x = X | \theta\}$. Do the authors, perhaps, use these words in the sense that they recommend that we should use this likelihood to induce a proper degree of uncertainty in our minds, that is to say, are they recommending this as the proper amount of uncertainty for us to have?

Secondly, I am not quite clear whether the likelihoods that are introduced in the paper are additive in the same way that probabilities are. For example, is the likelihood that the value of θ lies in a certain interval A to C the sum of the likelihoods that it lies in the sub-intervals A to B and B to C? If not, do they really draw strength from the Bayesian argument in any case at all?

Professor BARNARD: The answer to your second question is "No". They are not additive.

Mr D. G. CHAMPERNOWNE: That makes it very difficult to see how you can use this concept in order to compare the likelihoods that the value of θ will lie in two different ranges. I should welcome some explanation on that point.

The next point on which I would seek enlightenment concerns the comments on Anscombe's choice of a particular weighting function which was J-shaped. This choice was shot down on the grounds that it was chosen for mathematical convenience. The authors of the paper propose another weighting function which, in effect, extends over 3 per cent. of the range at a height of 97 and over 97 per cent. of the range at a height of 1: I cannot see why this is superior to the other. It seems to me they are both chosen for convenience. But perhaps I have misunderstood. It seems to me, incidentally, that the Beta distribution, of which Anscombe's choice is a simple case, is sufficiently flexible to be able to reflect most of the qualities which the authors might suggest as appropriate for the weighting function.

I should also like a little more enlightenment on when it is or is not legitimate to use the likelihood method. Is it, for example, only to be used when the probability function concerned always gives the same distribution of the difference between the estimator T and the parameter θ, where T is a location parameter also, or can it be used in more general conditions? And, possibly related to that point, how do the authors get over the difficulty that the choice of scale of measurement of θ is arbitrary? That question may perhaps already be answered by the fact that the likelihoods are not additive.

Finally, I should like some explanation of the remark towards the end of the paper "more generally in such cases we must use the group invariant measure".

If the authors will answer a few of these questions it will help me, at any rate, to decide whether their procedure is more nearly situated to what Professor Bartlett described as the Bayesian bog—or to that other.

Professor C. M. STEIN: I should like to add my thanks to the authors for a stimulating paper, and also to make a number of comments, of which the first is merely an attempt to emphasize a distinction that seems important.

A likelihood function must not be confused with a probability density function in the elementary sense. The former is a function determined up to a constant multiple, the latter (together with a basic measure or, loosely speaking, coordinate system) is a convenient way of describing a probability distribution. If you make a transformation of coordinates in the parameter space, a Jacobian enters into the transformed density in the parameter space if you are describing the same probability measure in the two coordinate systems. However, no Jacobian enters into the transformed likelihood. This difference is obscured by the fact that most of the transformations considered in statistical problems have constant Jacobian. From another point of view the likelihood function is proportional

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to a probability density function, the density of the posterior with respect to the prior distribution, but when we change the prior distribution the density remains the same and the posterior distribution changes.

The procedure the authors advocate at the end of Section 2 seems to be essentially a Bayesian approach with the prior distribution or weights chosen after the observations are taken. However, the choice of prior distribution has a strong influence on the Bayesian analysis of a problem at least in most testing problems and in high-dimensional estimation problems. To choose the prior distribution after the observations are taken makes it almost impossibly difficult to avoid wishful thinking and at the same time make efficient use of the prior information you really had. For example, I have indicated in my paper read to the Research Methods Meeting of the Society in February (Stein, 1962) that, for estimating the mean ξ of a highly multivariate random variable X with the identity matrix as covariance, if you are going to use a prior distribution having spherical symmetry about a given point η, you will obtain fairly good results by imagining the squared distance from ξ to η to be nearly uniformly distributed over the positive real line. If you decide to use one of these prior distributions or perhaps a mixture of them but postpone the choice of η, or of the mixture until you have observed X, you can easily obtain nonsensical results.

Even in the simple special case where the logarithm of the likelihood function is a quadratic function of a real parameter ξ, the likelihood principle can lead to strange results.

Professor D. V. Lindley: Likelihood inferences for binomial parameters (Section 2 of the paper) are most easily made by remarking that if the likelihood $\theta^r(1-\theta)^{n-r}$ is treated as a probability distribution then

$$F = \frac{n-r+1}{r+1}\frac{\theta}{1-\theta}$$

has an F distribution on $\{2(r+1), 2(n-r+1)\}$ degrees of freedom. I like to use the prior distribution proportional to $\theta^{-1}(1-\theta)^{-1}$, when the posterior distribution is such that

$$F = \frac{n-r}{r}\frac{\theta}{1-\theta}$$

has an F distribution on $\{2r, 2(n-r)\}$ degrees of freedom. Since $\log F$ is nearly normal we have the useful approximate result that the log-odds, $\log\{\theta/(1-\theta)\}$, is approximately $N[\log\{r/(n-r)\}, r^{-1}+(n-r)^{-1}]$. The log-odds are usually the most convenient quantities to handle, especially as their attractive properties extend to multinomial data.

The weaknesses of arguments using only the likelihood function are twofold: firstly, it is not a probability density and integration with respect to the parameter does not yield probabilities; secondly, statements cannot be made about one parameter in the presence of nuisance parameters without such integration. To see that a likelihood is not a probability density we have only to consider how it changes with the parameter. If we change from θ to ϕ the likelihood remains the same, only replacing θ by $\theta(\phi)$, whereas a probability density incorporates an additional Jacobian $|d\theta/d\phi|$. Thus it seems wrong to me for the authors to speak of likelihood ratios as odds if by odds they mean probability ratios. And if they do not, what do they mean by odds? Of course, a likelihood can easily be turned into a probability density by multiplication by a prior density and if this latter is constant then it has the same mathematical form. Then likelihood ratios are odds but only when the prior odds were unity.

The second difficulty is another aspect of the first. The argument on p. 350 for using linear averages is surely just a part of Savage's argument for proving that any reasonable person must have a prior distribution. The authors' (1)–(3) are reasonable requirements and lead to the result that the nuisance parameters can be eliminated only by use of a prior distribution function $W(\beta)$. And what group requirements have to do with the

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situation eludes me: if the same β had occurred in a different likelihood, why should it be removed by using a different weight function? And what does $W(\beta)$ mean? The Bayesian is clear about this. Indeed this seems to me the only difference between Bayesians and the authors.

The Bayesian introduces his prior distribution openly and clearly. The authors refuse to do this and progress as far as possible without it: but are always coming up against a brick wall which they can only surmount by introducing a prior distribution. And then they refuse to recognize the ladder by which they have ascended.

The authors say that the Bayesian approach is arbitrary and theirs is not. So it is, we say so openly, but so is theirs. Why in the footnote on p. 326 use θ as the natural scale?—this is arbitrary. Why use $W(\beta)$, this too is arbitrary?

But these comments emphasize our differences. We agree on much more and the principles raised in the analysis of time series are common to Bayesians and users of likelihoods. Furthermore they greatly simplify the analysis. I am reminded of the remark of Lord Mansfield quoted by Jeffreys, "Your judgment will certainly be right, but your reasons will certainly be wrong."

Dr M. G. BULMER: I have two reasons why I cannot accept the method of likelihood inference. The first is that it does not seem to me to be a method of inference at all. If I understand the authors correctly, what they are advocating is that the likelihood function should be calculated and reported to the experimenter, and that the job of the statistician is then finished. This would make my life much easier, but I cannot see how the mere presentation of a likelihood function, without any explanations as to what is to be done with it, can be regarded as a statistical inference, and I should therefore like to ask the authors if they can say more explicitly what, exactly, should be done with the likelihood function once it has been calculated.

My second objection to this theory of inference is that, like several previous speakers, I cannot understand the interpretation of likelihood as a measure of the credibility of a hypothesis.

Now, having discovered that likelihoods are not additive in this sense, perhaps my difficulty can be best expressed as a supplementary question to Mr Champernowne's question. Suppose we have the simple case mentioned by the authors in which the likelihood of $\theta = 1$ is 10 times the likelihood of $\theta = 2$, and let us suppose, for simplicity, that these are the only two possible values that θ can take: then the authors state that the credibility or plausibility of $\theta = 1$ is 10 times the credibility of $\theta = 2$.

This statement does not seem to me to have any meaning unless one can actually calculate what the two credibilities in fact are. The simplest way of doing this would be to say that they must add up to one, in which case the credibility of $\theta = 1$ is 10/11 and that of $\theta = 2$ is 1/11. But apparently the authors do not want to do this. Could they state in this very simple case exactly what these two credibilities or plausibilities are?

The following contributions were received in writing after the meeting:

The President, Dr M. G. KENDALL: I must record disagreement with the authors on one fundamental issue, namely the claim that estimation and inference in the autoregression case are no different from those in the regression case. The fallacy lies, I think, in the definition of likelihood. With a frequency function (continuous in x and θ) $f(x, \theta)$ we usually write the likelihood of a sample of n as

$$L = f(x_1, \theta) \ldots f(x_n, \theta). \tag{A}$$

What we ought to write, to be exact, is $L\,dx_1 \ldots dx_n$. We omit the differential elements because they do not depend on θ and are therefore irrelevant to the use we are proposing to make of L.

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Now in the autoregressive case (say, for simplicity, a Markov scheme) we have

$$\begin{aligned} x_1 - \rho x_0 &= \epsilon_1 \\ x_2 - \rho x_1 &= \epsilon_2 \\ &\cdots\cdots\cdots\cdots \\ x_n - \rho x_{n-1} &= \epsilon_n. \end{aligned} \tag{B}$$

The x's are all random variables. What the authors do in their equation (5.8) is, in effect, to transform the likelihood

$$L \propto \exp\left\{-\frac{1}{2\sigma^2}\sum_{i=1}^{n}\epsilon_i^2\right\} \tag{C}$$

into

$$L \propto \exp\left\{-\frac{1}{2\sigma^2}\sum_{i=1}^{n}(x_i - \rho x_{i-1})^2\right\}. \tag{D}$$

But this ignores the fact that we cannot transform a differential element in the n variables ϵ into a differential element in the $n+1$ variables x.

To overcome this difficulty we have either to adjoin to (D) the frequency function and differential of one of the x variables, or to take one of the x's, say x_0, as fixed. In the first case we complicate the likelihood by a factor depending on ρ; and moreover, this is not necessarily negligible in relation to the n other variables because x may have quite a different distribution from the individual ϵ's. In the second case we are making a conditional inference. Neither case is equivalent to an ordinary regression situation.

The contention that for large samples the different methods are equivalent is not in point here (even if it were sound). The problems of inference in time-series in practice are almost always concerned with short series.

Perhaps I can bring this point to a head by asking the authors a direct question. Given a regression situation

$$y = \beta x + \epsilon$$

would they correct the estimate of β,

$$b = \frac{\Sigma xy}{\Sigma x^2},$$

for bias? Given the autoregressive situation

$$x_{t+1} = \rho x_t + \epsilon_{t+1}$$

would they correct the estimate of ρ,

$$r = \frac{\Sigma x_{t+1}\, x_t}{\Sigma x_t^2},$$

for bias? If they would act differently, the two situations are not equivalent. If they would act the same, all my practical experience would suggest that they would get some very poor results in the autoregressive case.

I should also like clarification on the authors' so-called principle of likelihood. Since, as Dr Bulmer pointed out, they give no indication how the likelihood function is to be used in making inferences, it is very far from clear to me what this principle asserts. There must, presumably, be more in it than a statement that reasoning from identical premises should lead to identical conclusions. What more? If the contention is that the likelihood embodies all the relevant premises, I doubt whether the principle is valid. If two different situations gave rise to two likelihoods L and M which were proportional for all values of x and θ, nobody would, I think, object. But the authors apply the principle to situations where L and M are proportional for some particular value of x. In their first illustration (binomial and inverse binomial sampling) the two cases refer (a) to n fixed and r a random variable and (b) to r fixed and n a random variable. The fact

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that, for an observed r, the likelihood is proportional to the likelihood in the second case for that particular r and an observed n equal to the fixed n in the first case does not, in my opinion, mean that the two cases are on all fours.

The root of the difficulty lies, I think, in one's attitude towards conditional inference. To fix a quantity like a sample number beforehand and to make inferences in the sample space so determined is one thing; to make inferences in a space determined *a posteriori* by some random event is another. Apart from the few cases where regions similar to the sample space exist, it seems to me that conditional inference requires validation, and I suspect that the authors are, in effect, introducing it in a disguised form.

Dr E. H. Lloyd: These comments differ in character from those made verbally at the meeting, in that on the whole I accept the likelihood standpoint advocated by the authors.

It might not be out of place to remark on the pedagogical attractiveness of the likelihood approach; students do not find it hard to accept the likelihood function of, say, the binomial parameter θ, given 2 successes out of 10, as being in very satisfactory accord with their intuitive feelings about the relative plausibilities of the various possible values of θ.

I fully agree on the desirability of presenting the entire likelihood function, but feel nevertheless that some condensation is often useful. A convenient way of condensing a unimodal likelihood function $L(\theta)$ is to specify the maximizing θ ($\theta_{\max}$, say) and also the two values ${}_{\pi}\theta^{+}$, ${}_{\pi}\theta^{-}$, for which $L(\theta)$ is a fixed fraction π (say 20 per cent.) of $L(\theta_{\max})$. These two values give an indication of the asymmetry as well as of the precision of the likelihood curve. In the binomial case, for example, with $r/n = 0{\cdot}20$ and $\pi = 20$ per cent., one finds

$\theta_{\max}$	Sample size	θ^{+}	θ^{-}	$\theta^{+} - \theta^{-}$
0·20	10	0·473	0·045	0·428
0·20	40	0·322	0·103	0·219

Surprisingly perhaps, there is one point on which the authors' arguments in favour of the likelihood outlook seem to me to be insufficiently partisan. We are asked to consider the results of two independent sequences each indicating 50 successes out of 100 Bernoulli trials, the 50 successes occurring haphazardly in one sequence, and in the order 010101...01 in the other. We are told that the likelihoods are the same in the two cases, but that the inferences are different. Is this not rather unfair to likelihood? No inference can be better than the model or structure underlying it, and in a case like this a sensible model would incorporate the serial structure of the data in the likelihood function itself. The likelihood inference would not then need extraneous supplementation.

This example does, however, lead to a most important question, namely: "How are we to compare likelihoods based on different models?" The authors consider the triples (S, Ω, f), (T, Ω, g), S and T being two sample spaces, and f and g the two kernels. This framework relates to two conceptually (if not existentially) different experiments, and f and g necessarily involve the same parameter. What is now needed, however, is consideration of the different pair of triples (S, Ω, f) and (S, Σ, g) which arise when a given sample space S can be probabilistically described by two different models, with kernal functions f and g which need not be of similar forms nor involve the same parameters. We shall evidently need two parameter spaces Ω and Σ.

A system of inference must be able to make between-model comparisons, as well as within-model ones. In the (S, Ω, f), (T, Ω, g) case the authors have introduced "likelihood equivalence" as a principle of the irrelevance of the normalizing constant of the corresponding probability distribution. This is fine for within-model comparisons; but for the between-model comparisons entailed by the (S, Ω, f), (S, Σ, g) framework the principle does not seem to be particularly helpful. In comparisons involving different kernels, the normalizing constants, far from being irrelevant, are obtrusively important. It is not obvious to me that these numbers then necessarily represent the correct weighting of the likelihoods.

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I do not know how one would deal with this problem in general, but there is a class of situations where the difficulty can be overcome, namely when the kernels f and g overlap, in the sense that there is a set of parameter values for which $f(\,) \equiv g(\,)$. This intersection then provides a basis for a kind of analytic continuation. Suppose for example that one has, from a sequence of trials, the data $x = 010101001001011$. We might take for f the Bernoulli model of 15 independent trials with $P(1) = \omega$ and $\Omega = (\omega: 0 < \omega < 1)$, and for g the steady-state Markov chain model with transition probability matrix

$$\begin{pmatrix} 1-\sigma_1 & \sigma_2 \\ \sigma_1 & 1-\sigma_2 \end{pmatrix},$$

$P(1) = \sigma_1/(\sigma_1+\sigma_2)$, and $\Sigma = (\sigma_1, \sigma_2: 0 < \sigma_1, \sigma_2 < 1)$. For both models, $S = (0, 1)$.

The likelihoods are then

$$L(f(x, \omega)) = \omega^7(1-\omega)^8, \quad L(g(x, \sigma_1, \sigma_2)) = \sigma_1^6(1-\sigma_1)^2\,\sigma_2^6(1-\sigma_2)/(\sigma_1+\sigma_2).$$

The two models overlap at the point $\sigma_1 = \sigma_2 = \omega = \frac{1}{2}$. The identity of the likelihoods here (common value $= 2^{-15}$) guarantees that the relative scaling is correct.

Professor Allan Birnbaum: Besides adding my congratulations to the authors for developing, in an important new area, applications and interpretations of the likelihood approach which seem to me of great interest and value, I should like to comment briefly on their derivation of the likelihood principle (Section 4).

I am puzzled by the scope which the authors would give to their reservations concerning the general appropriateness of the conditionality principle. Since the latter principle is mathematically equivalent to the likelihood principle (as shown in Birnbaum (1962) and in earlier papers cited therein), presumably these reservations are identical with those the authors describe concerning the likelihood principle. But their stated reservations concerning the scope of the latter principle seem to centre on the possibility of using the fiducial argument in some cases. For those who, like myself, find the conditionality principle (and the equivalent likelihood principle) rather compelling, but regard the fiducial approach as one whose mathematical form and conceptual content have not yet been made clear, this reservation does not seem now to be very forceful.

As the authors rightly emphasize, any mode of interpreting results incompatible with the likelihood principle will require special justification to those inclined to accept that principle rather generally; and such justification must of course apply to the conceptual content as well as to the mathematical form of the proposed mode of interpretation. Since the mathematical form of the fiducial approach has evidently not been given a definite form in the general case as yet, these considerations suggest the question: Are there any considerations of conceptual content of the fiducial approach which would prevent adoption of the likelihood principle within the fiducial approach (granting that this has not been done hitherto)? Detailed suggestions in this direction were offered in my paper (Birnbaum, 1962).

Since the sufficiency principle concerns equivalence of certain outcomes of the same experiment, and does not ascribe any meaning to the question of equivalence of outcomes of different experiments, it is strictly weaker than the conditionality and likelihood principles, and it must be supplemented somehow if the latter principles are to be deduced.

It was Cox's paper (1958b) which made me appreciate the significance of mixture examples and conditionality concepts for the foundations of inference. My formulation of the conditionality principle was the first, I believe, having the form of an equivalence relation, in contrast with the customary "one-way" interpretation in which an unconditional experimental frame of reference is replaced by a conditional one; and from this formulation of the conditionality principle it was possible to deduce the likelihood principle, and conversely.

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Professor E. S. PEARSON: The authors have expressly stated that they are not concerned with the problem of experimental design. Nevertheless, from some comments in their section on "likelihood and power", I think that they are aware that in a rather wider context than that with which they are here concerned, some qualification is needed to Barnard's often-quoted phrase of 1949: "*before* the experiment is carried out probabilities are relevant, *after* the experiment likelihoods are relevant". This must be my justification for the following comment.

I am of course a little uncertain of what is meant by "standard Neyman-Pearson teaching"! But as I see things now—no doubt rather differently and I hope a little more clearly than 30 years ago—Neyman and I were essentially trying to deal in a coordinated way with the two problems of advance planning (the "before") and of action, resulting from the experiment planned (the "after"). We thought that the ideal situation was one in which these two phases of statistical practice could be closely related. In this we were clearly influenced by Fisher.

The term "action" must be given a wide meaning. It may involve the irreversible decision of rejecting a batch of material as defective; or it may consist in something far more provisional, perhaps telling a biological colleague that a new treatment is clearly worth looking into or, again, merely deciding that for the moment there is not enough evidence to justify the claim that some "difference" has been established.

These are all actions in the sense that we have come off the fence and taken some positive step, if only to call for more data. Further it seems clear that the probability that this step is a sound one, i.e. one which we should not regret if more information were available, is intimately connected with the planning, that is to say with the number of observations taken, the experimental design chosen, the form of statistical analysis, etc.

The authors of the paper have perhaps unwittingly left us with the impression that they think the statistician's function is ended when he has provided a likelihood function which he may either store for future reference or hand over to a colleague with the advice: "Here is the answer, draw your inferences." If, however, it is agreed that a good deal of the statistician's work is not just a matter of putting results on record in the most informative way, but is concerned with what I have termed this joint function of planning and action, then it seems to me that it is well worth thinking out the relationship of the before and after in Professor Barnard's 1949 saying.

Directly the question of action is brought in, attention turns to the sample space. It is, I would agree, often an oversimplification to adopt the original Neyman-Pearson specification of a single division of the space into a critical region and the remainder. What we need is perhaps a series of contours, separating off experimental sampling points which will give rise to different actions. We may usually not want to go to the trouble of going through this process of mapping out, because we hope that the results will be so conclusive, one way or another, that we need not face up to the difficult problem of fixing critical boundaries in the middle of penumbra-like belts. But it seems that if we are going to have to take some action anyway, even if only to say "collect more data", then it must in theory be always possible to fix these boundaries in advance, i.e. to prepare the ground so that directly we know the position of the sample point, we can decide what to do.

It seems then to follow that if likelihood has a greater appeal in clinching how we act with the experimental results in front of us, what we need is a philosophy of approach leading to a common method for dividing the sample space, the *same* division having an appeal which before the event is most forcibly expressed in terms of probability integrals and after it, may have a clearer meaning in terms of a comparison of likelihoods. If this is the case, then it may well be more valuable to try to coordinate the before and the after, rather than to repeat continually that they are essentially different. It surely does not make sense to claim that we use one set of rules in planning an experiment so as to secure the greatest chance that desirable action will result and then, after the experiment, that we start afresh in deciding what to do, governed by quite different considerations.

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Professor P. ARMITAGE: The crux of the case for likelihood inference seems to be the derivation of the likelihood principle for two different experiments (Section 4). It is this principle, for instance, which leads to the doctrine that stopping-rules are irrelevant. The preceding proof for different results of the same experiment seems to me incontrovertible, and provides a strong argument against, for instance, certain practices in sequential analysis whereby for a given stopping-rule the inference is made to depend on the order of the observations, and against randomization devices to provide exact significance levels with discrete distributions.

The authors go on to consider a mixture of two experiments (with probabilities p and $1-p$), and for this combined "mixture experiment" the preceding result clearly applies. They then argue that when p is very near unity the mixture is virtually the same as one experiment alone, and when p is very near zero the mixture is virtually the other experiment. But they are here comparing two *different* mixture experiments, with different values of p, and the argument seems to be back where it started.

I am not sure that this circularity of argument can be avoided. If not, one could fall back on an axiomatic acceptance of the principle, as the authors seem to do in the paragraph beginning "To look at this argument . . .". But then the merits of doing so should be balanced against certain difficulties one feels about ignoring stopping-rules, such as that mentioned by Professor Stein.

Mr P. SPRENT: In the authors' notation this remark could well be prefaced ∀.

Pitman (1957) has pointed out that if the parameter space consists only of m values $\theta_1, \theta_2, \ldots, \theta_m$, then the set of likelihood ratios

$$g(x, \theta_1) : g(x, \theta_2) : \ldots : g(x, \theta_m)$$

is a sufficient statistic for the estimation of θ, and further that this implies that in choosing among any finite sub-set of θ values, anything supplied by the samples except the likelihood ratios is irrelevant.

This has a bearing on the use of certain types of step functions as approximations to continuous likelihood functions. For instance, the $\lambda_1, \lambda_2, \lambda_3, \lambda_4$ introduced in Section 2 correspond to the values of λ for four parameter values $\theta_1, \theta_2, \theta_3, \theta_4$ belonging to the intervals (0, 1/4000), (1/4000, 2/4000), (2/4000, 3/4000), (3/4000, 1). Formally, the continuous function λ is here replaced by a step function having four points (at least) in common with it—one in each interval. In this particular case the four chosen values depend to some extent on the observed sample values, and I have not had time to consider whether this has further implications.

However, it is clear that for any well-behaved continuous likelihood function the idea of approximating to it by a step function with a finite number of steps, each step having at least one point in common with the continuous curve, is a useful one. In fact, we subconsciously replace a continuous parameter space by a finite sub-set of that space in many cases "involving" rounding off to a given number of significant figures. Pitman's result then tells us that the likelihood ratios are sufficient.

Formally we may regard the step function procedure as replacing the experiment (S, Ω, f) by an experiment $\{S, (\theta_1, \theta_2, \ldots, \theta_m), g\}$. For the Wald type sequential test of Section 2 this reduces to $\{S, (\theta_1, \theta_2), g\}$, and as such it represents a change in parameter space rather analogous to the change in sample space discussed in connection with power and likelihood in Section 3.

I think this is implicit in the interesting paper we have heard, but I do not know if the authors have looked at this particular byway.

Mr R. POPE: In the introduction, an example is given of a coin-tossing experiment, and θ, the probability of "heads", is estimated from an observed result of 3 heads in 20 throws.

The result is not further specified, and it appears logical to assume that any of the ${}^{20}c_3 = 1140$ possible variations are equally likely.

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If each of these alternatives is recorded as drawn, the third head will occupy position $n_i (= 3, 4, \ldots, 20)$ with probability

$$\frac{(n_i - 1)(n_i - 2)}{2 \times 1140}.$$

A maximum likelihood estimate of θ is made from the result "3 heads in 20 throws", and another from the result "3 heads in 20 throws, the twentieth throw resulting in heads", which is obtained from the rule "stop at the third head". It appears to be a subject of rejoicing that the two results are identical.

However, if the same "stopping rule" were applied to all of the possible variations of the original sample, the maximum likelihood estimate of θ would, in every case except that in which the twentieth observation is a head, be greater.

For instance, for $n_i = 16$

$$g(16, \theta) = \frac{15!}{2!\ 13!}\, \theta^3 (1 - \theta)^{13}$$

$$= 105\ \theta^3 (1 - \theta)^{13}$$

whence $\hat{\theta} = 0{\cdot}1875$ with $\sigma_{\hat{\theta}} = 0{\cdot}0976$.

This can, of course, be generalized to any size of sample, with any observed result. The alternative stopping rule will only give the same result, if the condition is made that the last member of the sample results in a "success".

Dr C. A. B. Smith: What is most remarkable about this paper is that the authors evidently subscribe to a theory of subjective probability. They are careful to deny this in words, and occasionally fire stray shots at some other interpretations of Bayes's theorem. But their code of practice, so far as one can follow it, and as far as it is self-consistent, agrees almost perfectly with that required by the theory of "pignic probability". This is not surprising, since their proof of the likelihood principle is based on the assumption that one can learn nothing about a parameter θ by observing the result of a random process which is independent of θ. The theory of subjective probability is essentially the logical consequence of assuming that one's behaviour should not be affected by observing the result of such a random process (Savage, 1954; Smith, 1961).

Their major innovation is the idea of averaging likelihood over a range of values of the parameter. They offer no explanation of this, and as stated it seems arbitrary. For if the parameter considered is changed from θ to, say, θ^3, the average likelihood will in general be changed. But the procedure seems to make good sense pignically. If a uniform distribution for θ does not run counter to one's initial judgements, i.e. if it is "medial" in the sense of Smith (1961), then it seems reasonable to use it as a weighting function. This is implied by Bayes's theorem, that the final probability is proportional to the product of the initial probability and likelihood. If most people have no very strong initial views about the value of θ, they may all agree to accept such a uniform weighting, thus giving practical unanimity. But if there is good reason to suspect that some values of θ are much more likely to occur than others, a uniform weighting would be inefficient (though not usually seriously so). For example, if the parameter θ under consideration is the genetical recombination fraction, it is well known on both theoretical and experimental grounds that values of θ exceeding $\frac{1}{2}$ by a detectable amount are extremely rare. To take a uniformly weighted average over the range $0 \leqslant \theta \leqslant 1$ would be to give equal importance to values $(\theta < \frac{1}{2})$ of great interest, and those $(\theta > \frac{1}{2})$ of little relevance. I assume that the authors would use an unequally weighted average here, as would be indicated by Bayes's theorem.

Such an averaging principle gives valuable extensions of Fisher's discriminant function, which is effectively the log likelihood ratio (lods) for two simple hypotheses. Suppose, for example, that θ is a probability, and I wish to compare the null hypothesis $\theta = \theta_0$

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with the alternative $0 \leqslant \theta \leqslant 1$. If $\lambda(\theta)$ denoted the likelihood, then

$$\Lambda = \lambda(\theta_0) \Big/ \int_0^1 \lambda(\theta)\, d\theta$$

is a weighted likelihood ratio in favour of the null hypothesis. In practice it is more convenient to use $\log \Lambda$. Thus, if a sample contains x "successes" and y "failures", where θ is the probability of a success, then $\lambda(\theta) = \theta^x(1-\theta)^y$, and hence

$$\log \Lambda = \log x! + \log y! - \log (x+y+1)! - x \log \theta_0 - y \log (1-\theta_0)$$

is the lods or "weighted discriminant function" in favour of the null hypothesis (Good, 1950). This kind of procedure fully obeys the likelihood principle. It seems to be the simplest and most practical way of detecting linkage in human genetics, where the whole course of the likelihood function can be plotted, and I have also suggested its use in the detection of heterogeneity in linkage.

I would protest vigorously at the new use of the word "likely" which has been quietly slipped in. This is sufficiently subtly different from its ordinary use, as a synonym for "probable", to give rise to endless misunderstanding and confusion. Why not say something like "the data offer H_1 4 times as much support as H_2", rather than "H_1 is 4 times as likely as H_2"?

Two further comments. It is untrue that "arbitrariness is a necessary feature of the Bayesian approach". To find initial probabilities, one has to make an honest assessment of one's ideas, to a reasonable approximation, before considering the data. Such an honest assessment is scarcely arbitrary; it should be part of every scientist's equipment. The authors are correct in saying that "every state of knowledge about a parameter" is not "expressible in a probability distribution"; but a distribution may be adequate for practical purposes. To take again the example of linkage, there is a mass of experimental evidence and theoretical discussion of the recombination fraction θ, and an initial probability distribution would seem at first sight a very meagre substitute. But suppose, on taking all this into account, I judge it advantageous to bet initially 10 to 1 on $\theta = \frac{1}{2}$, or 1 to 30 on $\theta \neq \frac{1}{2}$. Then if experimental data give a likelihood ratio 60,000 : 1 on $\theta \neq \frac{1}{2}$, this implies that I now consider it advantageous to bet at 60,000/30 = 2,000 to 1 on $\theta \neq \frac{1}{2}$, which surely would satisfy most people. To say that "I have a complicated set of opinions on this subject, which I cannot put into numerical form, and so cannot express any definite opinion" seems less than helpful.

Additional References

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The authors subsequently replied in writing, as follows:

We must first thank our discussants for the kind reception they have given us, as well as for their comments and criticisms. A few of these represent the hazard in writing an over-long paper: qualifications that have been made get overlooked, while qualifications that ought to have been made were omitted. In particular, Professor Whittle's example (p. 356, line 35) fails to satisfy the condition we did in fact give (Section 2, para. 5) of the independence of the two terms on the right of equation (5.1). On the other hand, in connection with the President's first point, we did not say in this paper (as we did in our note to the Paris meeting of the International Statistical Institute) that in (5.1) we were

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taking the initial x_1 as fixed. Other possibilities are briefly discussed in our Paris note, and are referred to (for the discrete case) immediately after (6.1) above. With this qualification, the difference between the regression (5.3) and the autoregression (5.1) consists in the fact that with (5.3) *all* the x's may be taken as fixed independently of β, while with (5.1) only the first x is so fixed.

The President then asks us whether we would correct for bias in either or both cases. Our attempt in this paper is to persuade statisticians to study the whole likelihood function rather than to take only a single number (an "estimate") from the data. For a whole function the notion of bias is less relevant. But were we concerned with point estimation we would almost never correct for bias. Only in the very special circumstances where we know it is intended to combine several estimates by the special method of taking the arithmetic mean does bias correction appear really appropriate. We propose to devote a separate note to this interesting point.

What we have said concerning autoregression is in line with our own practical experience with economic and industrial time series; and so some explanation of the President's experience needs to be found. This may be the fact that, with autoregression, there are cases where the series concerned are assumed stable. To know that the series (5.1) is stable is to know that $|\beta|<1$; and such knowledge implies that the likelihood function drops to zero outside the interval $(-1,+1)$. If, from such a likelihood function, we happened to want a "point estimate", an argument like that indicated in the first footnote to Section 2, and referred to by G. A. Barnard (*J. R. statist. Soc.* B, **21** (1959), 239–257), would lead to the estimator

$$\tilde{\beta} = \int_{-1}^{+1} \beta L(\beta)\, d\beta \Big/ \int_{-1}^{+1} L(\beta)\, d\beta$$

which turns out to differ from b by an amount

$$\{\phi(h-hb)-\phi(-h-hb)\}/h\{\Phi(h-hb)-\Phi(-h-hb)\},$$

where $h = \Sigma x_t^2/\sigma^2$ and $\phi(x)$ denotes the normal error ordinate and $\Phi(x)$ denotes the normal error integral. Thus we might, in such a case, apply a "correction" to b, but it would be misleading to call this a bias correction. For one thing, with the same additional information, $|\beta|<1$, we should apply exactly the same correction to b in the case of simple regression, although b is already unbiased.

To emphasize this point, we suggest considering the following sampling experiment. Take a set of values of β which span the stable range uniformly and reasonably densely, say $-0{\cdot}99,-0{\cdot}98,\ldots,+0{\cdot}99$. For each of these 199 values of β let us generate a sequence of (say) 10 autoregressive terms, using (5.1), with ϵ's drawn from $N(0, 1)$ and with $x_1 = 1$; and repeat this 1,000 times for each value of β. For each sequence of 10 terms we compute $\tilde{\beta}$ above, and any other estimator we care to think of. We do the same all over again, except that now the equation (5.3) is used instead of (5.1), with x's fixed at, say, $-2,-1, 0, 1, 2$, each value occurring twice. Again we calculate $\tilde{\beta}$, and any other estimator we care to think of. In all cases we evaluate the squared error, $(\tilde{\beta}-\beta)^2$, and the squared error of whatever other estimator we thought of, and we average these squared errors over all the 199,000 autoregressive cases, and average separately over all the 199,000 regressive cases. Barring remotely improbable accidents, of course, squared the mean error of $\tilde{\beta}$ will be smaller than that of its rival both in the 199,000 regressive and in the 199,000 autoregressive cases. The same estimator will win both times. Of course, the problem here is a decision problem, not an inference problem, but it may serve to emphasize the identity of the regression and the autoregression cases, provided the auxiliary data are the same.

We are most grateful to Professor Pearson for his comments and entirely agree with them. We see no conflict between the likelihood principle and Professor Pearson's approach, only a difference of emphasis. Likelihood considerations complement those

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mentioned by Professor Pearson in the following way: if we consider an experiment as a means of choosing between various possible courses of action, we ought somehow to form an idea of the courses from among which the given experiment enables us reasonably to choose. If we take the set of possible actions as given, without querying whether *any* of them could be appropriate, we may be led into absurdity. The railway ticket of our Section 2 is an example, since no assertion of the form "$\theta \leqslant B$ with confidence $1-(6/13)^{10}$" is really appropriate. A consideration of the various forms of the likelihood function makes clear that we must somehow make allowance for the varying precision arising from various types of sample.

Another illustration is provided by estimating a probability θ from r occurrences in n trials. Provided r/n is away from 0 or 1 the likelihood function is not very asymmetrical, and not far from the normal form, so that the usual "estimate" r/n with "standard error" $\sqrt{(rs/n^3)}$ is not too misleading. But when r is 0 or n, the likelihood is so skew that such an "estimate with standard error" form of statement is inappropriate, and many would prefer, for example, a confidence bound, or a set of such bounds. In intermediate cases an asymmetrical standard error, like that suggested by Dr Lloyd, could be used. Thus if we enumerate the possible forms of likelihood function that may arise from a given experiment, they may guide us in framing the set of "actions" from among which we make our "choice" on the basis of the experimental result.

In complicated cases, to enumerate all the possible likelihood functions in advance may be impracticable. We may then recognize that, while in principle the enumeration could be done in advance of the experiment, in practice it is easier to wait till afterwards, because we need then concentrate only on the likelihood function actually obtained. In suggesting that there is a difference of point of view between the planning stage of an experiment and the stage of its analysis we certainly do not wish to imply any drastic break, justifying any downright inconsistency. As Professor Pearson emphasizes, the planning and the analysis are two parts of a whole.

Coming now to Professor Durbin's comments, we tried to make it clear that we were concerned with inference situations describable in terms of a sample space, a parameter space, and a kernel function. It follows immediately that his example concerning the mean number of cigarettes smoked per day does not come within the category of problems we are discussing. We cannot, however, resist the temptation to make a few remarks. The short answer to the last sentence of Professor Durbin's third paragraph is, that we could not base our inference on the sampling distribution of $\bar{x}$ because we do not know what this is; we cannot derive it without knowing something about the distribution of the number of cigarettes smoked. The best way we know of dealing with situations of this sort really involves a parametrization of the problem, although much of the literature on sample surveys ignores this fact. We would here guess for the number smoked some sort of gamma-type distribution, with a lump at the origin, and then try to estimate in the usual way.

Professor Durbin goes on to make some remarks about conditional inference with which we do not seriously disagree. Indeed, one advantage of looking at the likelihood function is that it draws attention to ancillary statistics when these are available.

To come now to Professor Durbin's final question, we recall our statement in the penultimate paragraph of Section 4, that only when the triplet (S, Ω, f) by itself specifies *all* the inferential features of a situation do we claim the likelihood principle to apply. The unknown location parameter α introduced by Professor Durbin makes his regression case invariant under the translation group, while his autoregression is not so invariant. This can be seen because, in his equations (1), changing α to $a+\alpha$ and y_1 and y_2 to $y_1+\alpha$, $y_2+\alpha$ leaves the truth of the equations unaffected. But in his equation (2), adding a to the α and to the y's will falsify the second equation, since $2a$ is added to the right-hand side and only a to the left. Thus we have here a group structure in the regression case which is not present in the autoregression case. Referring to the penultimate paragraph of our Section 8, this means that there is no arbitrariness about disposing of α in the regression case, but

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there is in the autoregression case. With a series long enough to give a very precise estimate of α the arbitrariness becomes negligible and the difference between regression and autoregression fades away. The difference is at its maximum for only two pairs of observations. It should be noted that the likelihood function fails to factorize here, so that all the information about β can be utilized only when we have auxiliary information about α; the distinction noted between the two cases arises only when such auxiliary information is unavailable.

Dr Bulmer's problem about the meaning of a likelihood function may perhaps be answered thus: like all other statistical ideas, likelihood is best understood in its practical application, and we think that if he tries the habit of sketching likelihood functions for a time he will soon come to see how helpful they are.

Both Professor Whittle and Professor Bartlett use the word "optimal" and by it seem to mean "minimum variance unbiased", or MVU. We know of no situation in which the criterion of MVU can itself be justified, unless it happens to coincide with another, more appropriate, criterion—as in the usual least-squares theorem, where a proper criterion is minimum mean square error among estimators of bounded mean square error, which happens to coincide with MVU in this case. We are moved to protest against the use of loaded words like "optimal" and "justified" in these very peculiar senses.

In connection with Professor Bartlett's comment on the combination of estimates, the product of likelihoods from several samples will in all cases give at least as good an estimator, and usually a better one, than will the combination of any point estimates. Both Professor Bartlett and Professor Whittle refer to the maximum log likelihood and we do not disagree with them here; indeed we had mentioned this in our last section.

We are very grateful for Mr Champernowne's sympathetic comments. In reply to his first question, likelihood is to be understood as a measure of *relative* belief. The ratio $L(\theta_1) : L(\theta_2)$ measures the extent to which the evidence favours θ_1 rather than θ_2. A single value of $L(\theta)$, by itself, tells us nothing about the plausibility or otherwise of that value of θ. In answer to his second question, to refer to "the likelihood of (θ_1 or θ_2)" is like referring to "the height of (Peter or Paul)"—it is not defined until we specify which is meant, or with what frequency one or the other is to be chosen. Thus likelihood is certainly not, in general, additive, and it is not generally possible to measure the relative plausibility of θ lying in one interval as against its lying in another. We can, of course, take the lower bound L in one interval and compare it with the upper bound U in another, and say that in comparing any individual value in the first interval with any individual value in the second the evidence points at least as strongly as $L : U$ in favour of the first value; but if we want to go further than this we must find more structure in the problem—for example, group structure, or local group structure—or accept a degree of arbitrariness.

Mr Champernowne's next point concerns our use of a step function approximation to the likelihood function. Here we are not choosing or using a weight function; we are choosing the points where we allow discontinuities in our approximating function, and the metric of our approximation—the choice of points of discontinuity may be compared with the choice of powers of x in a polynomial fit, and the choice of metric exactly corresponds. Like all approximations, our step function approximation should be regarded as one of a sequence, generated when further discontinuities are allowed. With any one member of this sequence we could associate a weight function, but this would change as we went along the sequence, and the association would thus be only accidental. It is true that in discussing Anscombe's example we chose the points of discontinuity in relation to the practical circumstances of the given problem, but such a choice could be compared with the choice of a linear and a quadratic term to represent the response to a fertilizer—it is only a convenient way of separating out broadly different aspects of the data.

In connection with the beta distribution for parameters, introduced by Mr Champernowne, we certainly agree with its usefulness, but it has the defect that any beta distribution either has its mode at the origin, or has zero density there—no intermediate

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cases are possible. This trouble can be overcome by taking a weighted compound of two betas, one with mode at the origin, the other with its mode somewhere else. The posterior distributions are then represented by point-pairs, with weights attached, but most of the geometrical properties are preserved.

Professor Stein has set out his example in a separate note, to which we are replying.†

Finally, we must admit that Professors Armitage, Birnbaum and Durbin are quite right in detecting a gap in our argument in Section 4. The sufficiency principle by itself relates only to one experiment, and further considerations are needed to relate different experiments. We think that the qualified form of the likelihood principle we have put forward can be justified without appeal to the very strong principle of conditionality used by Birnbaum, by reference to the fact that any two experiments can be thought of as parts of a larger experiment obtained by performing the two experiments independently of each other. But this reply is already overlong.

As a result of the ballot taken during the meeting, the candidates named below were elected Fellows of the Society:

ALPINE, Robert L. W., M.A.
BARTER, John Harry, B.Sc.(Econ.)
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† These notes will be published in the next issue. *Editor.*

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Food Supplies and Population Growth

[A One-day Conference of the ROYAL STATISTICAL SOCIETY, held on Friday, April 6th, 1962, the PRESIDENT, Dr M. G. KENDALL, in the Chair during the morning sessions, and Mr A. L. BACHARACH during the afternoon sessions.]

		Page
The Mathematics of Hunger	RITCHIE CALDER	373
World Population Trends	B. BENJAMIN	378
Estimation of Food Requirements	R. PASSMORE	387
Discussion		397
Future Sources of Food Supply: Scientific Problems	N. W. PIRIE	399
Future Sources of Food Supply: Economic Problems	COLIN CLARK	418
Discussion		445
Concluding Remarks	BRINLEY THOMAS	449

The Mathematics of Hunger

By RITCHIE CALDER
University of Edinburgh

I AM here in the guise of a professor but that will not deceive any of you who know that I am a veteran and inveterate journalist. You will forgive me, therefore, if on this occasion in the practice of my trade I vulgarize statistics.

It is difficult for people who are not demographers or statisticians to grasp the meaning of population growth or the mathematics of hunger. This is serious because, next to the risks of the human race vaporizing itself in a nuclear war, this is the most dangerous and urgent problem which confronts the world today—"urgent" because, if we do not take active steps now, the situation, already serious, will deteriorate at an accelerating rate.

If we take as the daily rate of population increase the figure of 140,000, this is about the number of refugees who escaped from Northern Angola into the Congo, creating a famine-relief crisis. If this column were a single file passing a ration point, it would stretch for 30 miles and in the course of a year by daily increment would reach from London to New Zealand. If we think of the extra mouth as a refugee receiving a meagre daily ration of a pound of bread, a half-pint of milk and half a pound of fish, the increased annual demand would require a grain field as big as Lincolnshire, 2¼ million extra cows, and ten times the total catch of the Icelandic fishing fleet.

Or we might, to impress the public, put it another way. It has taken mankind over 200,000 years to reach the present figure of 3,000 millions. At the present rate of increase that figure will double itself in less than 40 years. The curve of population from the days of our primitive ancestors climbed steadily like a plane from take-off,

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soared pretty steeply from 1600 until today and is now rising vertically like a rocket off a launching pad.

This is pretty crude manhandling of statistics but we shall never get down to the problem by arguing about refinements. Nor can we mentally cope without paralysis of despair with figures which demographers use either to confound or to comfort us—like the standing room only predictions for 600 years from now which the U.N. experts extrapolated, or by being told that the present resources of the world could support a population of 40,000 millions. What is important is not the figures but the time factor. Perhaps, without discussing whether it is desirable or not, we could contemplate a world population five or fifteen times as big as it is today. I have no doubt that if we applied the money, the brains and the resources now being devoted to space research—throwing instruments or men into space at $50,000 per lb. weight—to the problems of scientifically feeding the world, we could do it. But biological processes do not react at 18,000 miles per hour. We are dealing with plants, animals and human beings, not computorized ironmongery.

We shall be much better advised to lower our sights and consider what we are going to do about the next 20 years. We can take it that the figure will be inescapably 4,000 millions—it may be more but it will not be less—"inescapably" because that is a figure to which we are already biologically committed. Only a man-made cataclysm like a nuclear war, which would be either total or quite indiscriminating in its genocide, could radically alter that figure. No conventional war could serve as what Sir Arthur Keith called "Nature's pruning hook", nor would any pestilence except possibly a man-made one—biological warfare with invented germs for which there would be no natural immunity. Medical science and epidemiology would arrest anything short of that. Whatever it was would have to assume the proportions of the Black Death, which is supposed to have killed a quarter of the population of Europe. The influenza pandemic of 1918–19 killed more people throughout the world than all the battles of the first world war yet it did not seriously interrupt the march to 3,000 millions. The advances in medical science have been made and it is axiomatic that "knowledge once given cannot be taken away". And knowledge is relative. Even the most ignorant people, in the most remote places, know that diseases are not just the will of the gods and that the wit of man can do something about them. Thus, even if in another form of selective genocide we discriminated against those countries where, in poverty, the numbers are increasing and refused them the assistance of our highly developed medical resources, they know enough to improvise. For instance, in the blockade of Communist China, the United Nations' agencies were forbidden to supply penicillin equipment (on the pretext that the freeze-driers which it would include might be used to freeze-dry toxins, such as botulin, for biological warfare), but the Chinese developed their own penicillin plants and they now mass-produce antibiotics. There is, therefore, no way by which death-control can be effectively withheld.

Nor, realistically and aside from all religious debate or discussion of method, can we foresee that birth-control can effectively modify that figure of 4,000 millions by 1980. It may prevent it from being larger and it could restrain the 6,000 to 7,000 millions foreseen for the year 2000. Nowhere in the world has family planning been more vigorously promoted than in Japan. In 1951, the government decided on a programme for the propagation of contraception. The efforts were redoubled on the recommendation of the Population Problems Investigation Council and recent surveys indicate that about half the married couples, rural as well as urban, practise

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contraception. In the ten years between 1950 and 1960, the birth-rate was halved but, by 1960, the death-rate had dropped from a pre-war level of 17·4 per thousand to 7·4. The expectation of life at birth in Japan is now 65 for men and 70 for women, compared with 50 for both in pre-war years.

With an area about that of Sweden, the population of Japan is the fifth largest in the world, after China, India, the U.S.S.R. and the United States. The population is still increasing at the rate of 900,000 a year, but by 1967 the population of reproductive age will be increasing at the rate of 1,300,000 a year. By 1980 the Japanese population will be 110 millions compared with 94 millions today.

In India, where there is active government support for family planning, the survival rate is increasing fast. The United Nations' experts have had to revise their notional 5 millions per annum increment to 10 millions. Anyone who knows the difficulties of India in its efforts to feed its people and to develop its resources will realize what this means in the frustration of national planning. It is like running up the down-escalator.

Let us accept the fact that in a world in which we inadequately feed the larger proportion of the population we shall have to find rations for another 1,000 millions within the next 20 years. There is a great deal of statistical nonsense talked about food supplies. Until quite recently we were told that for calories the global supplies were keeping pace with the increase in population. *Per capita*, in global terms, everything was satisfactory. But what we are, or should be, concerned about is not *per capita* but *per stomach* and the food has not been getting into hungry bellies. The global figures have been distorted by surpluses which exist but are not, or cannot, be distributed. There was the rapid, and unexpected, recovery of European agriculture after the war and there was the highly industrialized agriculture of the Western Hemisphere. As things are it does not help much if countries like the United States and Canada increase either their acreages or their yields. The effect is only to produce farm surpluses which in the free play of the markets would fetch prices which would outrage the farmers. So the U.S.A. had to introduce "price supports" and the "soil bank", a policy which deliberately took acres out of cultivation. The Government lost $204 million by destroying potatoes or selling them at less than the price of the burlap bags in which they were stored. They fed millions of dollars-worth of California raisins to pigs and they stockpiled food at a cost which rose to over $4,000 million. By 1957, the granaries and warehouses were bulging with 11 million bales of cotton, 900 million bushels of wheat, 1,650 million bushels of maize, 90 million lb. of butter and 100 million lb. of dried milk. The surpluses of coarse grains amounted to the staggering figure of 2,000 million bushels. By 1960, Canada and the U.S.A. between them had most of the surplus stocks of grain—125 million tons. This would have provided for one year the full calorie needs of 450 million people—say the entire population of India—or it would have given an extra 200 calories a day to the Asian people for more than three years.

We saw what happened when it became known that the Milk Marketing Board was going to pour 250,000 gallons of skimmed milk into disused collieries. The Prime Minister had to explain to the House of Commons that it would cost £500,000 to build the plant to convert the skimmed milk into dried milk for the protein-hungry peoples of the world and that the plant would stand idle until the next glut. This is where some of us came in. It was the pouring of milk down the drains which roused Boyd Orr to fury in 1931 and started him on what became the Food, Health and Income Survey which in turn led to the setting up of the Mixed Commission

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of the League of Nations—the "marriage of health and agriculture"—to Hot Springs and eventually to the Food and Agriculture Organization of the United Nations. But the problem of distribution of surpluses is still with us. You have to pay people to take them away.

"Hunger in the midst of plenty" was a battle-cry of the thirties. But the lesson of the past 30 years has been that, in an imperfect world, the surpluses of one part of the world—except in emergencies like emotionally outrageous famines—are no comfort and no solution for other parts. Hunger is, for the impoverished, a local matter. There never has been an "Indian famine". There have been *Bengal* famines, *Bihar* famines and *Orissa* famines—the collapse of district production. And it is in the regions, the countries and the districts themselves that we must look for a solution of the world food problems. There must be ways and means of increasing the yields where the food is needed but where it is needed the ways and means are lacking. It is no use exhorting peasants, in countries with *per capita* incomes of less than $100 a year, to get themselves fertilizers, better tools or better irrigation methods. For example, it is easy to say that if only the Indian peasant could get himself a solar stove which would cook his meals without having to burn the cowpats which should be nourishing his sick soil, he could improve his yields. If that stove costs a sixth of his income he cannot afford it. And anyway his wife does not like cooking out of doors and having her neighbours supervising her recipes.

While it is a local problem which must find its solution on the spot, and for which we must encourage the growth and use of indigenous foods, it is a global problem in the sense that somehow the "misery-go-round" of poverty, the roundabout of wretchedness —sickness, underproduction, malnutrition, sickness—must be checked. This can only be done by outside help and that is where we are falling down and where the increase in population is working against us. In the decade which followed President Truman's call for a bold new policy, some $30,000 million went into the 100 countries and territories we call "underdeveloped": that is, from all sources—multi-lateral aid, bilateral aid and private investment. The overall effect was to raise the *per capita* income from $90 to $100—an average increase of a dollar a year during the decade. The annual income grew at the rate of 3 per cent. but their population grew at the rate of 2 per cent. per year.

It is true to say that science and technology or just better farming practices could, even at this moment, adequately feed the world's population. It is also true, as I am sure Mr Pirie will tell us, that we have been singularly unimaginative in our use of the food sources which exist. I was very much impressed at the Arusha Conference, which was held last September in Tanganyika, by the complete consistency between the preservation of wild life and the improvement of protein supplies through the culling of the game herds. But this again needs capital assistance to provide mobile abattoirs and factories to produce meat-meal and so on. With seven-tenths of our planet under water, it is a reproach to us that we are still at the cave-man stage as far as food from the oceans is concerned. We should have sea-ranches and ocean-pastures, and we should be domesticating the sea-creatures as we domesticated the land animals. But again, who does it? Which nation is going to develop plaice nurseries and plant them out on the Dogger Bank, the Tom Tiddler's Ground for all-comers? Obviously this is the kind of thing—not only in the North Sea but, for instance, in the Indian Ocean where this year the ships of 15 nations are combining in a sort of marine equivalent of the International Geophysical Year—where the World Bank should finance cooperative international sea enterprises.

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We have answers enough. It is not a question of "whether" but "how", and the "how" is not a matter of knowledge but of intention. Let us have all the facts and figures but let us have imagination and purpose as well. It is not enough to give lip-service to the Freedom from Hunger Campaign and the World Development Decade. We are all in it together—3,000 millions of us on a satellite-encircled planet of which only three-tenths is land and, of that land, only one-tenth is cultivated.

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World Population Trends

By B. Benjamin
General Register Office

Summary

The population of the world, estimated at almost 3,000 millions in 1959, is increasing by about 50 millions every year and there are no expectations of an early reduction in the rate of growth. Indeed, for a time, further reduction in mortality without any decline in fertility may even lead to an increase in the rate of growth. Economic policies of individual underdeveloped countries must be accommodated to the prospect of a doubling of their populations by the end of this century. By the year 2000 the population of the world will be between 6,000 and 7,000 millions. Even by 1975, a few years away, it will be nearly 3,900 millions. International interest has been heightened but individual Governments alone can determine policies to deal with the economic and social problems posed by the growing population pressure. Whether these policies approach the problem from the population or the economic end their implementation is often obstructed by illiteracy and other educational difficulties. The very growth of numbers too acts as a brake upon economic development since increases in productivity are largely offset by the growth in the number of mouths to feed. At least the population implications of the present situation are better understood than formerly and economists and agriculturalists, who must bear the major brunt of stresses which are building up, have the advantage of a global appreciation of the underlying demographic developments.

1. Population Growth

The population of the world was estimated by the United Nations Statistical Office (1960) to have reached 2,907 millions at the middle of 1959 and to be increasing at an average rate of 1·72 per cent. a year. In absolute numbers this implies that the annual addition to the world population would be of the order of 50 millions, i.e. rather less than the present population of Great Britain. The birth-rates, death-rates and rates of natural increase in different parts of the world, estimated on the basis of statistics relating to 1950–59, are given in Table 1.

By far the greatest factor accelerating the growth of population in the underdeveloped parts of the world has been the decline in mortality as a result of widespread campaigns waged against malaria and other pestilences, and as a consequence of intervention, medical and economic, largely by the developed countries, through the United Nations and its specialized agencies, as well as efforts made by the underdeveloped countries themselves. On the other hand, those influences tending to reduce fertility which arise as the concomitant of advanced economic development have not penetrated to these areas so that birth-rates have not fallen, indeed in some instances they have risen slightly. It has been estimated (United Nations, 1958) that if these present trends continue to 1975 the birth-rate in Asia will be 43 per thousand while the death-rate will have fallen to 20 per thousand giving a natural increase of 2·3 per

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cent. a year. In Latin America in a similar way the annual rate of increase of population will have risen to 2·8 per cent. In Europe where both rates of birth and death are already low and the room for change most restricted, the rate of increase will be a mere 0·8 per cent.; and in Northern America it will be only 1·2 per cent.

TABLE 1

Population, 1959; and annual average birth-, death- and population growth-rates, by regions, 1950–59†

	Population mid-year 1959 (millions)	*Birth-rates*	*Death-rates*	*Population growth-rates (%)*
World	2,907	36	19	1·7
Africa:				
Northern Africa	78	45	26	1·9
Tropical and Southern Africa	159	47	28	1·9
	237	46	27	1·9
America:				
Northern America	196	‡25	‡ 9	§1·8
Middle America	65	‡45	18	2·7
South America	137	42	19	2·3
	398	34	14	§2·1
Asia:				
South-west Asia	74	46	21	§2·5
South Central Asia	546	44	26	1·8
South-east Asia	208	44	23	2·1
East Asia	794	‡39	‡21	1·8
	1,622	42	23	§1·8
Europe:				
Northern and Western Europe	141	‡18	‡11	0·7
Central Europe	137	‡19	‡10	§0·8
Southern Europe	145	‡21	‡10	§0·9
	423	19	10	§0·8
Oceania:	16	‡25	‡ 9	§2·4
U.S.S.R.:	211	‡25	‡ 8	1·7

† Birth- and death-rates are per 1,000 population and are annual average for 1950–59.
‡ Weighted average of recorded rates.
§ This rate reflects the combined effect of natural increase and migration.

The expected growth of population to 1975 and to 2000 in different regions (based on medium assumptions) is illustrated in Table 2.

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In terms of selected individual countries the short-run increases to 1975 are as shown in Table 3 (medium assumptions).

TABLE 2. *Population growth in different regions*

Continent	*Population (millions)*		
	1959	1975	2000
World . .	2,907	3,828	6,267
Africa . .	237	303	517
N. America . .	196	240	312
Latin America .	202	303	592
Asia . . .	1,622	2,210	3,870
Europe . .	423	476	568
Oceania . .	16	21	29
U.S.S.R. . .	211	275	379

TABLE 3. *Population growth in selected countries*

Country	*Population (millions)*		*Overall increase (%)*
	1959	1975	
India . . .	403†	563	40
Pakistan . .	87‡	128	47
Mainland China .	670	894	33
Japan . . .	93	116	25
Indonesia . .	90	122	36
Argentine . .	21	27	29
Brazil . . .	64	102	59
Mexico . .	33	53	61
United States .	178	218	22

† The 1961 Census gave India a population of 438 millions; prior population estimates were therefore substantially low.

‡ The 1961 Census revealed a population of 94 millions.

2. WORLD POPULATION

The population of the world is thus expected to rise by 1975 by one-third of the existing total to over 3,800 millions. It is the situation in Asia which invites urgent attention; already contributing more than half of the population of the world (56 per cent.), in 1975 Asia is expected to account for a slightly larger fraction (57 per cent.) and to have a population more than three-quarters the present population of the globe. In this monsoon region there are already high concentrations of people§ and

§ With the exception of Europe, Asia and the Far East is the most densely populated region in the world. Its average population density is 60 persons per square kilometre as compared with only 22 for the world as a whole. Japan, South Korea and Taiwan are the most densely populated countries. In China the Kiangsu and Shangtung provinces (population 90 millions) have a density of 380 persons per square kilometre.

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a level of living desperately low by Western standards; and there is no evidence supporting hope of any immediate relief from the pressure of numbers. Beside this central problem, difficulties elsewhere are dwarfed; in any case they are of a different character. In large parts of Latin America and Africa there is ample living space but full exploitation of potential resources is retarded by lack of capital and by social and cultural obstacles to the effective deployment of existing or of much needed immigrant manpower. Elsewhere in Europe and Northern America population pressure is less acute and the need is for the most efficient use of existing resources and for the preservation of favourable terms of trade to sustain the advanced economic organization.

We have been speaking of 1975, which is a comparatively short distance away in time. Even 2,000 is a short 40 years away. During that time the pace of world population growth is expected to increase unless major influences, of much greater proportions than yet exerted, emerge to alter this trend. Such evidence as there is, based upon the experience of countries undergoing industrialization, indicates that the effect of urbanization and education in producing attitudes favourable to smaller families may take a generation to yield significant changes in fertility. The rapid growth of the world's population cannot be slowed instantly.

In the preface to its report on the future growth of world population, the Department of Economic and Social Affairs of the United Nations describes the situation in the following dramatic terms: ". . . while it took 200,000 years for the world's human population to reach 2,500 millions, it will now take a mere thirty years to add another 2,000 millions. With the present rate of increase, it can be calculated that in 600 years the number of human beings on earth will be such that there will be only one square metre for each to live on. It goes without saying that this can never take place. Something will happen to prevent it."

3. International Activity

What will this "something" be? Many influences are already exerting themselves. At the international level there is much activity. A significant part of the resources of the secretariat of the United Nations is devoted to the study of population changes, and the factors underlying them, throughout the world. Their programme of activity is geared to meet the recommendations of the Population Commission, a 15 member advisory body which meets biennially and advises the Economic and Social Council of the United Nations on population matters. The United Kingdom has been a member of the Commission since it was set up in 1948. At its session in 1957 the Commission reported its discussion in the following terms: ". . . The Commission urges that governments concerned should study scientifically the problem of population growth in relation to industrial development. In the course of the discussion the view was expressed that in solving the population problems of those countries [undergoing industrialization] emphasis should not be placed upon limitation of births; an attempt should rather be made to raise the economic and cultural level of the people by means of national programmes of economic development and the elimination of existing impediments to international trade. The Commission considers it essential that intensive efforts should be made by governments to speed economic and cultural development and raise the level of living of the population. In its judgment, it is for each country to decide whether or not it is appropriate to foster the possible influence of industrialization and development in bringing about a voluntary reduction of births." This last sentence represents a meeting point between those countries (mainly

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those with expanding economies and ample land space) which favour unrestricted population growth and those which wish to take urgent action to avoid a situation developing in which world population growth may outstrip the availability of resources. It seems likely that as the world situation becomes more acute the expansionist group will tend to adapt their position and to encourage restriction of population growth. There is general agreement that of all restraints, voluntary reduction of births, especially where this arises as a natural concomitant of economic and cultural development, is preferable to famine and pestilence—the positive checks which Malthus (at the end of the eighteenth century) said were mainly instrumental in adjusting populations to food supplies.

In cooperation with the International Union for the Scientific Study of Population (whose very existence provides further evidence of international interest) the United Nations organized a World Population Conference in Rome in 1954. More than 450 experts from all parts of the world attended. Most of the foremost students of population trends and problems from a large number of countries were present, together with leading experts in economics, sociology, genetics, medicine and the physical sciences relevant to the conservation and development of natural resources. The discussions brought into relief the magnitude of the population increases which were being produced by the progressive reduction of death-rates in underdeveloped countries where birth-rates remained high; and on the positive side consideration was given to the opportunities for economic development under different population conditions and to the possibilities of developing natural resources and meeting the needs for additional capital. Another such conference is to be held in 1965. The Population Branch of the United Nations (1953) has also issued an important technical report, entitled *The Determinants and Consequences of Population Trends*, bringing together a vast body of information to promote more widespread understanding of the inter-relationship of population growth and economic and social factors. This report is at present being revised prior to re-issue at the request of the Population Commission.

Another important aspect of United Nations activity is the work of its Statistical Office in assembling population information. Until quite recently, as history goes, there has been a great dearth of the essential facts needed properly to appraise population growth in just those areas of the world where the growth is evidently most rapid. In many of these areas there is no effective birth and death registration. To fill this gap the U.N. Statistical Office has worked sedulously to foster the institution of systems of vital registration where economic development renders this practicable, and to improve those systems already instituted but not fully efficient. The major contribution has come from census taking on a massive scale. Over the period 1955–64 the U.N. have organized a World Census Program in which some 240 countries will cooperate. Already it is about two-thirds completed. It will supply much needed information about the size, sex and age distribution, marriage incidence, economic activity, family size and growth prospects of national populations. To assist this program the U.N. have provided technical assistance and manuals of instruction; organized training seminars on census procedures and on the utilization of census data in less developed areas; held regional meetings of census officials to agree upon minimum requirements and upon the adoption of common concepts and definitions to facilitate comparability of census statistics.

Another example of international activity is the work of the Food and Agriculture Organization, a specialized agency of the United Nations which is doing much to

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foster efforts to improve the production of food especially in the underdeveloped areas of the world. FAO collects and publishes information from all over the world on land use, crops, livestock, agricultural production and food supplies, and trade in the major food and agricultural commodities; in addition technical assistance and general economic advice is given to aid national campaigns to increase food production. In 1951 the Sixth Session of the FAO Conference urged the necessity for a well-balanced increase in world agricultural production of at least 1 to 2 per cent. per year in excess of the rate of population growth. Reference by the Population Commission to the promotion of international trade as a factor in securing a better balance between population growth and food resources is underlined by the fact that, while in many parts of the Middle and Far East the struggle to raise food production above primitive levels has been intensified, surplus stocks have accumulated elsewhere—mainly in the dollar area. The problem is one of reducing costs and of easing international payments difficulties, without increasing the already extreme instability of prices of agricultural products; and, in the importing countries, of adjusting the priority given to capital goods for economic development (which is a prerequisite of an increase in agricultural labour productivity in these countries).

The general result of this activity has been to raise the level of public consciousness of the need to balance world population and world resources in terms of distribution as well as quantity.

4. Reliability of Estimates

In the demographic field considerable progress has been made. At least the basic facts about population have been largely removed from the realm of controversy which they occupied a decade or so ago. Few serious workers would regard the estimates of population growth with which this paper was introduced as too low. Nevertheless a brief account of the methodology is called for. Several steps were involved. Most countries supply the United Nations with current population estimates, but owing to incompleteness and to variability of criteria it is necessary for the Statistical Office to make auxiliary estimates to permit comparable measurement of population growth among regions of the world. These observations of trend cover only a comparatively short period and do not immediately promote generalizations which have to be made to some extent independently of the mass of statistical material. These generalizations lead to a scheme of population models which, when applied to particular regional populations for given periods of time, yield theoretical rates of growth. These are then reconciled with observed rates of growth to feed back adjustments to the models. The adjusted models then permit estimation of future growth. Within this scheme, estimates for individual countries are finally derived.

As to future changes in mortality and fertility trends, a range of assumptions have been formulated to fit the major demographic situations prevailing in various parts of the world. For example, the following mortality situations have been typified (as an admitted approximation) to accord with different stages of progress:

(a) Constant high mortality (expectation of life near 30 years)
(b) Declining mortality (expectation of life rising continuously, eventually to attain almost 75 years)
(c) Eventually constant mortality (once an expectation of life approaching 75 years has been reached).

The second of these situations will be the most common one though reduction in mortality will be more rapid in some parts of the world than in others.

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Similarly for fertility, account has been taken of levels most relevant to current conditions (gross reproduction rates of 3, 1·5 and 1·25) and of the effect of past trends on age structure which in turn affect future fertility. Alternatives of constant or declining fertility have been incorporated in the models for each situation. Account has also been taken of interaction with mortality. Two main future trends have been formulated:

(a) Where gross reproduction has hitherto been of the order of 3, it may remain constant, or it may decline either in the near or the remote future

(b) Where current gross reproduction rates are of the order of 1·5 or 1·25, future rates, sooner or later, may tend towards 1·5, 1·25 or 1 in each case

and the experience of developed countries has been used to gauge the speed of decline.

The final models are of four main types:

A. Constant high mortality, constant high fertility

B. Declining mortality, constant high fertility

C. Low mortality, fertility moderate

D. Low mortality, fertility low

and sub-types have been distinguished within each main type; for example, B1, mortality very high but declining, hitherto constant fertility (South-east Asia); C1, low mortality, early decline in fertility, fertility now moderate (Northern America, Australia and New Zealand).

The total world population of the more distant future will be determined by the present overweighting populations of high fertility. It has generally been assumed that in these countries mortality will continue to decline at rates which are now normal and that fertility will (a) remain constant until 2000 (high assumption), (b) decline, at rates observed previously in some areas, after 1975 (medium assumption) or (c) decline forthwith and continuously until 2000 at the rates assumed in (b) (low assumption).

This brief summary of the methodology of the U.N. projections does not do justice to the careful historical appraisal of the social and economic factors underlying the observed changes in mortality and fertility or the genuine attempt made to embrace all shades of opinion about the likely out-turn in the future, but it serves to show that in any cooperative venture to balance population and productivity the demographers can provide their fair measure of assistance.

5. National Activity

In one of the most important areas, Mainland China, information of national activity is also most scanty. In a recent careful appraisal of the situation from the outside, Taeuber (1956) finds that while the greater emphasis has not unexpectedly been placed upon industrialization and upon an extension of land cultivation and food production, government policy has been accommodated to the problem of raising the living standards of a population of 670 millions to which 13 millions are added each year. Arguing that a growing population could, in a Communist society, result only in increased production, Communist governments have habitually derided as "Neo-Malthusian" any suggestion that there could be a problem of over-population. Nevertheless they have shown themselves capable of rapidly adapting policy to changed circumstances. If birth limitation had to be rejected when urged on any "Neo-Malthusian" grounds it could still be accepted for personal and family reasons—in order to reduce the burdens of bearing and rearing children. It appears that between 1954 and 1956 there was some reorientation of policy along these lines and

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that in the latter year a campaign to extend knowledge and practice of family limitation was ordered by the Minister of Health—as a health measure. Optimistic statements about the future of agricultural development continue, however, and extend to claims that Mainland China could accommodate another 600 millions; nevertheless it is believed that in future years the campaigns for reducing family size may become firmer.

The picture in India is relatively clear. The population of India at the 1961 Census was 438 millions. If present trends continue the population in 1975 will be approaching 600 millions. The pressure of population presents an urgent problem. The Bengal famine of 1943 still casts an after-shadow. In the first Five-year Plan for India a reduction in the rate of population growth was regarded as essential and this was reiterated in the second Five-year Plan in which large sums of money were provided for family planning programmes. The third Five-year Plan provided £20 million. The Registrar General of India (1953) has spoken of "improvident maternity" for Indian women (childbirth to a mother who has already given birth to three or more children of whom at least one is alive), and he has urged that this should be reduced. There are a considerable number of Family Planning Centres under the auspices of State Governments and voluntary agencies. In addition there has been a large programme of education, field studies, and research into different methods of contraception. The Government objective has been described by the Indian Family Planning Research and Programme Committee as the use of deliberately planned social action as a means of shortening the long historical process which led to a reduction in the birth-rates of other countries, particularly in the West. Whether or not fertility will decline must depend on the extent to which motivation for restraint can be inculcated in the Indian population. Some observers consider the programme relatively ineffective and emphasize that the rural Indian still finds a large family an economic advantage; and that only the minority of urban Indians with high levels of education have changed attitudes in favour of smaller families. Western-orientated education seems to be a more important factor than religion, tradition or social differentials. There seems to be no prospect of a rapid decline in fertility.

In Pakistan family planning is part of Government policy. There is a National Board of Family Planning under the chairmanship of the Director-General of Health. Apart from sums of money voted by the Government (more than £2 million in the current Five-year Plan), money has been provided by private foundations for institutes of family planning, audio-visual appliances, vehicles, consultants and fellowships. Family planning outpatient clinics are invariably a feature in newly opened Rural Health Centres. Surveys have indicated that ignorance, religious orthodoxy and mass illiteracy are the main obstacles to the spread of unbiased knowledge on family planning matters and efforts are being concentrated upon these obstacles. Early in 1961 the number of medical units and clinics exceeded 500; some 3,000 clinics are planned in the current Five-year Plan.

Crowded Japan, its population problem aggravated by the repatriation of five million nationals after World War II, achieved a reduction in the annual rate of population increase from 2 per cent. in 1948–49 to a little over 1 per cent. in 1956, but only by the widespread induction of abortions to an extent that has given rise to official concern about the deleterious effects upon health. An Advisory Council on Population Problems set up by the Japanese Minister of Welfare has urged that efforts should be made to educate the population in methods of contraception and in the principles of family planning.

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6. PROSPECTS

As realization of the sheer arithmetic of world population growth becomes more widespread and as the economic consequences become more acute, attitudes will change in such a way as to retard this growth. Economic development itself will bring retarding influences to bear. But it has to be said that there is little likelihood of any *early* retardation in the rate of growth. Governments are not idle and are likely to become more concerned with population pressure. However, government policy is made in the capital cities; it has to be carried out in the distant villages and it takes time and education to build the requisite chain of understanding and acceptance. This short account of world population trends has to end on a sombre note. The forecasts with which the account was introduced were based on "medium assumptions". A communication just received from the U.N. Population Branch, which is currently reviewing the 1958 projections, states their preliminary view that the "high assumption" seems more appropriate. This would give a world population in 2000 not of 6,280 but 6,900 millions.

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Estimation of Food Requirements

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Summary

For assessing the adequacy of present food supplies and forecasting future needs, three different types of information are required. These are: (1) the numbers to be fed, together with their age and sex—demographic data, (2) the amount of food available or likely to be available in the future—agricultural and trade statistics, and (3) information about the requirements of individuals of varying ages, sex and occupation—physiological data. The physiological information, as set out in the report of the FAO Committee on Calorie Requirements, is probably, for many countries, more accurate than the demographic data and the agricultural statistics. It is certainly sufficiently precise to be used as a guide for present assessments of world hunger and for planning future needs for agricultural developments.

In the prosperous industrial countries of the world, the physical activity of both men and women is being steadily reduced. Obesity is very common. This is an indication that food supplies are present in excess of needs. It is possible that the FAO Report over-estimates requirements in these countries.

1. Introduction

Professor Colin Clark began his comments on Dr Sukhatme's paper on "The world's hunger and future needs in food supplies" (Sukhatme, 1961) with the observation that physiology must be an even more inexact science than economics. Not being a serious student of economics, I am in no position to dispute this judgement. However, his attack was provoked by inconsistent statements about the *qualitative* aspects of our dietary needs. This is indeed a subject about which we as yet know all too little and where there is room for legitimate differences of opinion. Knowledge of the *quantitative* aspects is much more accurate and on this physiologists have spoken with the same voice for at least 60 years and recently with increasing precision.

2. Quantitative Aspects

The first function of food is to provide the body with energy, which it supplies in a chemical form. This chemical energy is liberated in the tissues and converted by the muscles into mechanical work, which enables us to carry out our everyday activities, and into the heat necessary to maintain our body temperature. It is also used for the synthetic processes necessary for building new tissues during growth and for replacing materials broken down in the body's daily "wear and tear". The chemical energy present in the food is converted into mechanical work and heat within the laws of thermodynamics, which are equally applicable for the engineer and biologist. Any unit of energy can be employed by biologists, but custom has led physiologists to use the kilocalorie (kcal.) which is 1,000 times the calorie as defined by physicists.

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The latest important statement on the quantitative requirements of man for food is the report of the FAO Committee on Calorie Requirements (1957a). This Committee was composed of 14 members from many parts of the world, of whom I was one. There was a remarkable general agreement amongst the members: at no time did the question of a minority report arise. The report has been well received and is widely used. Sukhatme (1961) drew heavily on it when preparing his paper for this Society. The report is, in fact, orthodox. It is unlikely that substantial changes in its recommendations will be necessary—at least in this century.

2.1. *The Reference Man*

The basis of the Committee's report is an arbitrarily defined Reference Man. He is 25 years of age, healthy and fit for active work. His weight is 65 kg. and he lives in a temperate zone at a mean annual temperature of 10°C. On each working day he is employed eight hours in an occupation which is not sedentary but does not involve more than occasional periods of hard physical labour. He is assumed to require on an average for the entire year 3,200 kcal./day. This assumption is based on the activities as set out in Table 1. The physiological cost of many human activities has been determined (see Passmore and Durnin, 1955) and the figures taken for each activity in Table 1 are not likely to involve any serious error in the total assessment of energy needs.

TABLE 1

Example of how the energy expenditure of the Reference Man may be distributed throughout the day

(Weight 65 kg. Mean annual temperature 10°C)

			Calories
A.	8 hr	Working activities: mostly standing, over-all rate: 2·5 cal./min.	1,200
B.	8 hr	Non-occupational activities:	
		1 hr washing, dressing, etc. at 3·0 cal./min.	180
		1½ hr walking at about 6 k.p.h. at 5·3 cal./min. . . .	480
		4 hr sitting activities at 1·54 cal./min.	370
		1½ hr active recreation and/or domestic work at 5·2 cal./min. .	470
			— 1,500
C.	8 hr	Rest in bed at basal metabolic rate	500
			TOTAL 3,200

The difficulty is that we do not know, and cannot possibly find out, how the "average male citizen" in any country spends his time. How active is he? For how much of each day is he sedentary? There is no group of men that work regularly week by week as hard as coal miners, although lumbermen and other seasonal workers may expend energy at higher rates over part of the year (Lundgren, 1946). The work of a coal miner has never been found to involve an expenditure of more than 4,000 kcal./day (Moss, 1935; Lehmann *et al.*, 1950; Garry *et al.*, 1955). This figure can be taken as the upper limit of requirement. At the other extreme, sedentary clerks expend about 2,400 kcal./day. Physiologists would not seriously dispute these two figures.

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The FAO Committee had to put their reference man somewhere between these two extremes. In placing him half-way, with an energy requirement of 3,200 kcal./day, they came to an arbitrary decision. In any country there will obviously be many men who are more active than this and also many who are less. Only in a very small community is it possible to take sufficient measurements to enable an accurate assessment to be made of the physical activities of the men.

2.2. *The Reference Woman*

The position with women is similar. The Reference Woman, who is aged 25 and weighs 55 kg., is described as engaged either in general household duties or in light industries. Her daily activities include walking for one hour and one hour of active recreation, such as gardening, playing with children or non-strenuous sport. These activities require 2,300 kcal./day. Details are set out in Table 2. This again is a mid-point between a range of 1,600 and 3,000 kcal. which certainly covers the great majority of women.

TABLE 2

Example of how the energy expenditure of the Reference Woman may be distributed throughout the day

(Weight 55 kg. Mean annual temperature 10°C)

			Calories
A.	8 hr Working activities in the home or in industry: mostly standing, over-all rate: 1·83 cal./min.		880
B.	8 hr Non-occupational activities:		
	1 hr washing, dressing, etc., at 2·5 cal./min. . . .	150	
	1 hr walking at about 5 k.p.h. at 3·6 cal./min. . . .	220	
	5 hr sitting activities at 1·41 cal./min.	420	
	1 hr active recreation and/or domestic work at 3·5 cal./min. .	210	
		—	1,000
C.	8 hr Rest in bed at basal metabolic rate		420
			TOTAL 2,300

The Committee also provided formulae whereby the effects of age, of climate and of body weight upon the requirements of the reference man and woman could be calculated. They also added figures for the requirements of children and adolescents. In drawing up these parts of the report, the Committee was guided in part by the results obtained by measurements of rate of energy expenditure, but had to rely to a large extent on measurements of food consumption of individuals in health. The available data of both kinds were certainly far less than was desirable. However, in my opinion, the error in calculating the food needs of any country, arising out of the use of these parts of the report, is far less than that likely to arise from assessing the activity of the reference man and woman.

Requirements depend on the type of life people lead or want to lead. Physiologists cannot ever be expected to provide scales suitable in all times and at all places. Modifications of the requirements of the reference man and woman may be necessary. Probably in some parts of the world today, requirements have been set too high.

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2.3. *Prosperous Industrial Communities*

In many countries people are becoming less physically active. With mechanization on a large and increasing scale in factories and workshops and also, but to a lesser extent, in our homes, the need for physical activity in our daily occupations is being steadily reduced. Unless this is balanced by a corresponding increase in activity during leisure and recreations, the need for food declines. The fact that supply exceeds requirements for many individuals is proven by the large number of obese people who can be seen in all the prosperous countries of the world.

2.4. *Incidence of Obesity*

Data about the incidence of obesity in those citizens of the United States who apply for life insurance policies have been set out by the Metropolitan Life Insurance Company (1960) and are reproduced in Table 3. There can be no doubt that

TABLE 3

Frequency of overweight and underweight

Percentage of insured lives with weights deviating by specified degree above and below average and "best weight"

	Men				*Women*			
	Percentage deviating from average weight							
Age (years)	*Above average* 10% or more	10–19%	20% or more	*Below average* 10% or more	*Above average* 10% or more	10–19%	20% or more	*Below average* 10% or more
20–29	21	14	7	23	21	11	10	29
30–39	19	13	6	24	25	13	12	35
40–49	21	16	5	22	27	15	12	31
50–59	21	16	5	21	26	15	11	30
60–69	21	15	6	20	27	18	9	31
	Percentage deviating from "best" weight							
Age (years)	*Above "best"* 10% or more	10–19%	20% or more	*Below "best"* 10% or more	*Above "best"* 10% or more	10–19%	20% or more	*Below "best"* 10% or more
20–29	31	19	12	13	23	11	12	22
30–39	53	28	25	6	41	16	25	13
40–49	60	28	32	4	59	19	40	6
50–59	63	29	34	4	67	21	46	4
60–69	57	28	29	5	68	23	45	4

Note: The deviations from average weight are taken from the *Build and Blood Pressure Study*, 1959, Society of Actuaries. The data on deviations from "best" weight, derived primarily from that study, have been prepared by the Metropolitan Life Insurance Company.

many Americans are fat. So also are many Czechs. Ošancová (1961) has reported that in Prague 34 per cent. of the men and 52 per cent. of the women were obese, judging by the fact that they exceeded their ideal weight by more than 10 per cent.

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Similar figures were obtained in other parts of Czechoslovakia. In one rural community during the course of two years, the average weight of a group of men rose 2·6 kg. and of a group of women 7·8 kg. I know of no figures for the incidence of obesity in Britain, but daily observations in the streets of Edinburgh indicate that the Scots are not far behind the Americans and the Czechs.

2.5. *Effects of Obesity*

That obesity is no trivial condition is shown by the data of the Metropolitan Life Insurance Company (1960), from which Table 4 has been drawn up. In men and

TABLE 4

Excess mortality for specified cause amongst men and women approximately 20 per cent. or more above average weight. Ages 15 to 69

	Excess mortality† %	
	Men	*Women*
Coronary artery disease . . .	35	35
Vascular lesions of the nervous system .	53	29
Malignant neoplasms	16	13
Diabetes mellitus	133	83
Pneumonia and influenza . . .	32	27
Diseases of digestive system . . .	68	39

† Compared with all persons insured at standard risks.

women aged 40 to 69 years, mortality amongst those who are 20 per cent. above average weight is 31 and 32 per cent. respectively. There can be no doubt that obesity already affects the demographic picture in many countries. As the disease appears to be increasing, it will become more important in discussions on population problems of the future.

2.6. *Causes of Obesity*

It is a popular opinion that the obese are big eaters. There is no scientific evidence to support this. Whenever the food consumption of obese people has been measured (Swanson *et al.*, 1955; Johnson *et al.*, 1956; Stefanik *et al.*, 1959), it has been found that they do not as a rule eat more than non-obese controls and may indeed eat less. Further, Thomson *et al.* (1961) have shown that in non-obese subjects in this country large people eat no more than small. As there is no doubt that the physiological cost of all activities is closely related to body weight, it must follow that heaviness, in some way, diminishes physical activity. In brief, fat people appear to be not gluttons but idlers or, more politely, leisurely folk. Miscellaneous human activity has been measured by Chirico and Stunkard (1960), who got their obese patients to wear a pedometer for a week and compared the results with controls of the same age, sex and occupation. Table 5 shows a summary of their findings. It will be seen that on the obese patients the pedometers clocked up each day distances much less than those accredited to the reference man and woman. Pedometers are not very precise instruments, but the differences between the obese and the controls are highly significant. Improvements in the design of these instruments may be

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possible. If so, surveys of physical activity involving large samples of a community would be practical and might be worth while.

Enough has been said to indicate that, for prosperous communities, the FAO Report may well overestimate food needs—at least for the kind of life that many

TABLE 5

Summary of data on overweight in relation to miles walked

Group	*Median amount overweight* %	*Mean age years*	*Mean daily distance walked* (±S.D.) *miles*	*P*
Men:				
Obese . .	54	36	3·7 ± 1·8	0·05
Non-obese .		36½	6·0 ± 4·3	
Women:				
Obese . .	52	42	2·0 ± 1·2	0·01
Non-obese .		40	4·9 ± 2·1	

people choose to lead. Of course, if interest in physical recreations changed, then many passive participators, at present spectators and readers of the sporting columns of newspapers, might take an active part in games, sports and hobbies. If this occurred on a large scale, a reassessment of the food needs of such communities would be necessary.

2.7. *The Underdeveloped Countries*

It is much more difficult to judge the accuracy of the present standards when applied to those countries, which are numerous, where hunger is certainly widespread. In many parts of the world human muscles are still the main machines supplying the power necessary for agriculture, for building operations and for all types of industry. The fuel for these machines is food, and without sufficient food the machines cannot work adequately. I know of no evidence which suggests that the FAO Report overestimates requirements either for rural or industrial populations in the tropics. In my opinion the greater part of mankind needs food in quantities which are approximate to those set out in the report; this provides a sensible scale for future agricultural developments in Africa, the Far East, the Near East and Latin America.

2.8. *The Future*

By the end of the twenty-first century that turbulent, bustling species, *homo sapiens*, may have disappeared. He may choose to evolve into *homo sedentarius*, a quiet animal who sits and uses his intelligence to pull knobs and flick switches, which control the output of atomic energy to the machines doing the work necessary to provide his food and comforts. For his leisure, he may be content to rest in front of a television set or talk amiably with his neighbours. This transformation has started: if it becomes complete, then requirements for food will be cut by from one-quarter to one-third.

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3. Food Waste

It may now be instructive to look at some data first presented by Sukhatme (1961) and set out here as Table 6. For Europe, North America and Oceania the difference

Table 6

Current levels of calorie supplies and calorie requirements by regions

(*Per caput* per day at the retail level)

Region	*Calorie supplies*†	*Calorie requirement*	*Calorie supplies as % of calorie requirement*
Far East . .	2,050	2,300	89
Near East . .	2,450	2,400	102
Africa . .	2,350	2,400	98
Latin America .	2,450	2,400	102
Europe . .	3,000	2,600	115
North America .	3,100	2,600	119
Oceania . .	3,250	2,600	125
Group I . .	2,150	2,300	93
Group II . .	3,050	2,600	117
World . .	2,400	2,400	100

† Rounded to the nearest 50.

between calorie supplies and requirements amounts to 600 kcal. or more per head per day. If this excess were all laid down as fat, we should each of us be gaining weight at the rate of about 36 kg. a year (about 4½ stone). This, of course, is absurd. The discrepancy between the two columns is probably due mostly to waste. The table would suggest that every four citizens of the prosperous world waste enough food to supply the needs of one citizen in the underdeveloped countries. If, as I have suggested, our requirement is overestimated in the table—at least for our present manner of life—then the waste is even greater. In practice it has proved extremely difficult to measure household waste. Some of this food is not wasted in the physiological sense, but goes to support our large numbers of household pets. In a few districts it benefits pigs and poultry.

4. Qualitative Aspects

Food has to supply in addition to energy many nutrients, proteins, fats, minerals and vitamins which are essential for the body. It is much more difficult for the physiologists to define with accuracy these qualitative aspects of a diet. Deficiency of any of the above factors may lead to deficiency diseases, most of which are clinically well defined: it is known fairly accurately how much of each of the essential nutrients is required to prevent the appearance of these diseases. This minimal requirement is seldom satisfactory. A margin is needed for safety and also probably for full health. How big should this margin be? Estimates commonly given may vary from several hundred per cent. in the case of vitamin C to about 50 per cent. for protein and less for some other nutrients. These estimates are, in general, based on dietary customs in prosperous communities rather than on scientific knowledge. Tradition and custom determine the type of the diet which we eat. Some are perhaps too eager to get scientific support for their theories that what they like to eat is also good for them.

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4.1. *Minerals and Vitamins*

It would be impractical to discuss at a meeting such as this requirements of the various vitamins and minerals necessary for man. Deficiency diseases due to lack of these nutrients have declined markedly in the last 25 years. They are still common in some areas where they may create an important public health problem. However, for the most part, they are now local problems and so may legitimately be omitted from a discussion concerned with matters of world-wide significance. An important exception to this generalization is anaemia due to iron deficiency, which still affects large numbers of women in all countries.

The decline in deficiency diseases has resulted from a general increasing prosperity rather than from planned application of medical knowledge. The best example of this is pellagra, which before 1939 was very common amongst the negro and poor white communities in many of the southern states of the U.S.A. When the war came and brought full employment and good wages, the disease vanished. It is now very difficult for American doctors, who wish to study scientific aspects of the disease, to find a single patient. If there were a world-wide depression with a marked fall in wages and unemployment, deficiency diseases would recur in many countries where they are now absent, unless specific measures were taken to prevent this.

4.2. *Fat*

The amount of fat present in human diets varies enormously. The quantity is correlated fairly closely with economic prosperity. In many poor communities fats provide less than 10 per cent. of the calories, whereas in the United States they provide just over 40 per cent. In all civilizations, Eastern or Western, old or new, fat has always been an important necessity for the preparation of good meals. Probably no housewife in any country could prepare the meals, which she and her family would like, without materials in which fat supplies 20 per cent. of the calories. The needs for fat are very real, but they are based on social and psychological rather than physiological considerations. It is doubtful if primary fat deficiency has ever arisen in man, except perhaps in a few individuals who have been clinical curiosities. There is an aboriginal tribe in India called the Hos. In a detailed study of their diet, Mitra (1942) found that most of the families made no use of any kind of fat in their cooking. Though they owned cattle, these were used as beasts of burden and no milk or meat was eaten. Intakes of fat were less than 5 g./head/day (about 2 per cent. of the total calorie intake). No evidence of fat deficiency was found. Presumably the small amount of fats present in their vegetarian diets provided sufficient of the essential fatty acids, known to be required by a great variety of laboratory animals. Man's quantitative need for these fatty acids is unknown, but must be small. Very large quantities of fat are often taken by Polar explorers and others without ill-effect. However, diets rich in fat are also often rich in calories and then the fat contributes to obesity.

Recently a large body of medical opinion, strongest in the United States, has come to the view that high intakes of fats of animal origin are largely responsible for the great increase in coronary artery disease that has occurred in the last 25 years. Vegetable oils are supposed to be able to counteract, at least in part, this action. It is impossible here to summarize the evidence, which is necessarily indirect, for this view. There is no doubt that fats of vegetable and animal origin affect the levels of cholesterol and often fatty substances in the blood in very different ways. Whether such changes may lead to coronary artery disease or any other disease is, at least,

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controversial. Generations of Anglo-Saxons have enjoyed butter and cream and have prized fat cattle and bacon. It is hard to believe those who now tell us that such good things are poisons. Yet it is always possible to have too much of a good thing.

As a guide for those responsible for planning agricultural policies, it may be said that if the foods which they produce do not provide 20 per cent. of the calories in the form of fat, then the people may be expected to complain—and rightly so. If the food provides more than 30 per cent. of the calories in the form of fat, then luxuries are being supplied. Luxuries are good, but indulgence is best tempered with discretion.

4.3. *Protein*

It has long been the custom to use the protein content of a diet as a measure of its quality. This practice has been followed by FAO who in several annual reviews of "the State of Food and Agriculture" give the daily consumption of protein per head in selected countries and also the amount of the protein consumed, which is derived from animal sources. For over a generation physiologists have recommended an intake of 70 g. protein/head/day (1 g./kg. of body weight), of which half should be of animal origin. This is a sensible recommendation for adults. It is true that most of us in this room would grumble greatly if we did not get more than this each day, but the excess, which we usually eat, is pleasant and does us no harm. On the other hand, the above recommendation is generous and allows a liberal margin of safety. We could probably eat much less and our health not suffer. Protein deficiency is not an important condition in adults except in those parts of the world where almost all the protein supply comes from a single vegetable source.

In infants and children the position is very different. Lack of sufficient suitable protein leads to a disease known as kwashiorkor which is widespread in many parts of Africa, the Far East, the Near East and Latin America. The mortality rate from kwashiorkor may be high and the disease causes much ill health (for reviews see Brock and Autret, 1952; Waterlow *et al.*, 1960; Scrimshaw and Behar, 1961). Children need protein for growth and their requirements are relatively much greater than those of adults. On a weight basis they are probably 2 to 4 times as great. Further, the quality of the proteins is important. Proteins are composed of amino acids, which are necessary to build up the protein in the new tissues of the growing child and to replace the protein lost by "wear and tear" in the adult. The proteins in foods are not all equal in this respect, but each contains a different mixture of amino acids. The chief protein of milk, casein, is nearly ideal, because it contains amino acids in a proportion well suited to build human tissue protein. All proteins of vegetable origin are relatively lacking in one or more amino acids and so alone are not a suitable source of material for making human tissues. Vegetable proteins, however, differ in respect of these deficiencies and, in a mixture of vegetable foods, two or more proteins may compensate mutually for the deficiencies present in each individual one. The essential problem is that of amino acid requirements.

An FAO Committee on Protein Requirements (1957b) has issued a report, which is a courageous attempt to tackle the problem in terms of individual amino acids. It is no secret that the members of the Committee had great difficulty in agreeing about the report, which itself has been much criticized by nutritionists—in private rather than in public. The report is undoubtedly a challenge to experienced nutritionists to think out the problems of the needs and supply of amino acids more clearly and to collect more information to provide a stronger scientific base for future policies. In

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this respect it has been a great success. As a practical guide to agricultural planners, it falls far short of the ideal.

There are inherent difficulties which may make a simple statement of the protein requirements of a population on a *per caput* basis impossible. There is the fact that requirements for infants and young children are so much greater than for adults. A diet which appears to supply ample protein by the FAO standards may be associated with the presence of protein malnutrition. Nicol (1959) found that, in some of the grain-eating areas in Nigeria, the level of protein consumption provided 1·5 to 2·5 times the safe practical allowance recommended by FAO: yet the incidence of protein malnutrition in children up to 9 years was 3·1 per cent. In other parts of the world, protein malnutrition may be common in children, yet the total supply appears more than adequate by the FAO standards. Difficulty arises because the protein supplies of a family may not be distributed amongst its members according to their physiological requirements. It is often the custom for housewives to withhold the protein-rich foods from the children and reserve them for the adult men, whose needs are relatively much less. Only prolonged and widespread education can change this common practice.

Until these difficulties can be overcome, agricultural planners will have to rely on the older standards. An aim of 70 g. protein/head/day is a sensible target. If the food supplies 2,600 kcal./head/day, then this amount of protein will contribute 10·7 per cent. of the total calories. In many countries to suggest that half of this protein should be of animal origin is a quite impractical ideal. It is, however, practical to plan agriculture so that in any country the supply of protein is derived from at least three different plant or animal sources. Economic and educational programmes will then be needed to ensure that each individual derives some of his daily protein intake from each of these foods. If these aims were achieved, kwashiorkor would become a rare disease and a large number of the children of the world would enjoy better health.

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DISCUSSION ON THE PAPERS BY DR BENJAMIN AND DR PASSMORE

Mr W. H. BECKETT: The word "hunger" is used very commonly now—as in the Freedom from Hunger Campaign—and I should like a definition of the term. Dr Passmore referred to distribution within the family. In some dietary work I did years ago in West Africa I noticed that one of the chief difficulties was the distribution over the season. Although diet was adequate if you simply divided by heads, alongside this quite adequate diet there were definite deficiency diseases which you could see by skin patterns and that sort of thing. This was due to the distribution over the season, particularly with animal protein intake. The people's main intake took place during three months of the year when the snail season was in and during another four months when their cash crop was being marketed and they had money to buy fish. This meant a relationship of much more than 2 to 1 in their total protein intake, between one part of the year and another. Is this hunger or not?

Dr W. KLATT: Body-weight and age are relevant to average consumption levels,† for instance in India, where the average body-weight is considerably lower than in Western Europe. There is also the kind of occupation that the bulk of the nation is engaged in. In India and other parts of Asia one finds that up to three-quarters of the population is engaged in a particularly heavy type of occupation, working behind the plough and planting in the fields, occupations with which we are no longer familiar in Western society.

Mr. A. S. MANI: I cannot agree entirely with Dr Benjamin that in India there seems to be no prospect of a rapid decline in population. In the last census of India, 1961, a reduction in the rate of growth of population has been noticed, particularly in Madras. The reason is that the educated people in India, both in urban and rural areas, do not wish to have large families. For example, my father had eleven children of whom eight are living, and the average number of children of these eight is three. This trend is going on in South India but it is not apparent in the Indo-Gangetic plain. Provided science and technology can assist in furnishing definite family planning methods within 20 years, India should be able to arrest this growth of population.

† (*Added in writing.*) As to FAO's report on food requirements, the Nutrition Panel which prepared the report seems to have underestimated the requirements of men and women engaged in heavy work and overestimated the reductions due to reduced basal metabolism and work at advanced age, thus causing an apparent gap of approximately 250 calories between estimated European requirements at retail level and consumption at retail level. (Ref.: *Economia Internazionale*, Vol. V, No. 2, Geneva, 1952.)

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Dr R. A. M. CASE: Until recently a very large source of animal protein has been wasted because of the obsession of the whaling industry with taking fats, which, in the Antarctic, was done in such a way that the meat was often rotten before the fat was removed from the animal. Drying it by heat, which would sterilize it, unfortunately reduces the value of the protein. There has been a recent development of interest in the smaller whales, the dolphins and the porpoises, which appear to show traits of intelligence that might make them a particularly good form of marine animal to domesticate. They would yield a meat of good quality which would not suffer from the disadvantage of that of the larger whales; that is, the difficulty of quick cooling because of the vast bulk involved. I make the suggestion with considerable sadness because I myself have a very great admiration for both the smaller and the larger whales.

Mr M. S. BROWN: Dr Passmore referred to food waste in the developed countries. What about food waste in the less developed countries, and its relation to the reference man's calorie requirements?

Professor CALDER: In the underdeveloped countries the waste is enormous. Destruction by pests and so on in the fields could, in fact, be overcome to a very large extent. There is also an enormous waste through taboos, through food preferences and through the rotting of foods, for which refrigeration is required. In many South-east Asia villages, for example, solar refrigerators could be developed on an industrial or a cooperative scale, though not on a domestic scale in any economic sense. An oceanographic survey of the Indian Ocean this year may lead to the setting up around its shores of biological stations, and the development of a real interest in food from the sea. This will mean that we have got to think engineeringly. How are you going to get this food into the interior of India or Indonesia, and so on?

Dr BENJAMIN: I am not in any disagreement with my colleague from India. When I said that it would take a generation for a change to show, I meant that something would have to happen in the way that he suggested, namely, that the children who grew up would have smaller families than their parents. Changes of this sort seem to have taken place everywhere apart from Japan, which is a rather special case.

Dr PASSMORE: Hunger, which I have been asked to define, is a psychological term and I am a physiologist. It is a sensation which people experience when they are trying to get food to eat and I think it is an unfortunate word because the sensation of hunger is not always closely associated with the need to eat. Also it has many emotional overtones. It would be a great advantage if we could have better words to describe the undernourished, and the starving. I think the word "starving" is used far too frequently, and I have suggested elsewhere that the word "starvation" should be used only when the person is so seriously undernourished that he is in danger of death. Under that criterion the number of starving people in the world is small. We must distinguish it from underfeeding or undernourishment, which is with us on a large scale. There has been quite a lot of work on the physiological cost of various agricultural processes in Africa and in India, but my impression is that though many agriculturalists work extremely hard on certain days, they have many days of the year doing nothing at all. It would be difficult to give an accurate statement without observations going over the whole year.

About food waste, there is a distinction between waste in the home and waste outside the home. In our communities there is an enormous waste in the home. In the East the waste in the home is probably negligible. Wastage outside the home—storage systems have been mentioned—may be enormous.

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Future Sources of Food Supply: Scientific Problems

By N. W. Pirie
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Summary

The various conventional methods for getting more food—irrigation, increased use of fertilizers, prevention of loss through pests and diseases, etc.—are valuable but they are not likely to satisfy the need if the world's population increases according to current forecasts. Reassessment of the uses to which crops are put is needed. This calls for research on protein-rich crops that people can eat directly, on ways of making some of the oil seed residues more palatable and acceptable, and on the extraction of protein from leaves that cannot be eaten as "spinach". In spite of developments such as these, animal products will retain their appeal, but more imagination is needed on the choice of domestic animals for each region. More use could be made of microorganisms to make residues and wastes useful, and a few vitamins, amino acids and energy sources will probably be made economically by total synthesis.

1. Introduction

More than 30 substances—amino acids, vitamins, carbohydrates, fats, minerals, etc.—are essential in a diet and they are distributed very unevenly in foodstuffs. It is therefore prudent to use as diverse a group of foodstuffs as possible. This policy has the added advantage that it diminishes the catastrophic effect of unusual weather, pests and diseases. It is, therefore, a mistake to compare, in any exclusive way, the different possible ways of getting more food; the world's needs are so large that there is probably a place for every possibility. Not only should all eggs not be put in one basket, they should not all come from one bird.

Clearly, a potential source of food need not be considered seriously if it is inconceivable that its cost, when produced in bulk, would be comparable to that of the equivalent conventional foods. But, in considering research projects, the word *comparable* should be interpreted broadly; many factors make predictions of costs unreliable. The greatest source of uncertainty arises from the arbitrary assignment of importance to the products arising from one operation. Thus fishing produces fish for sale in the normal way; it also produces fish-meal, oil, "stick water", and other by-products. Fish-meal is a very much cheaper source of protein or calories than fish because the latter carries the main costs of the fishing industry. In exceptional circumstances it is economic to fish primarily to make fish-meal but only one-seventh (Nicol, 1962) of the world's total catch of fish is turned into meal. Any project for the more extensive use of fish-meal, or its sophisticated cousin fish-flour, must therefore be coupled with a corresponding increase in the direct use of fish, or costs will become unpredictable. Similarly, the costs of dried skim milk do not reflect the costs of production, for these are mainly carried by the sale of fresh milk and butter, and these foods are subsidized in many countries.

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The elasticity that these considerations introduce into our thinking about potential food sources, though by no means indefinite, is embarrassingly large. To keep this article within the permitted space, attention will be concentrated on sources of protein. Protein-rich foods are at present more expensive than other foods consumed in bulk and every recent survey shows that protein deficiency is even more widespread than calorie deficiency (Aylward, 1962; Waterlow *et al.*, 1960; Sukhatme, 1961). There is disagreement about the precise amount of protein needed by different types of consumer, and about the relative merits of protein from different sources; but there is no disagreement, among those with expert knowledge, about the reality of a world protein deficit. There is no trustworthy evidence that the position is improving as time goes on; the contrary view is maintained only by those who mistake a trend in nutritional opinion, leading to lower estimates of minimum protein requirements, for an increase in protein production.

2. Conventional Approaches

2.1. *Irrigation*

The scientific problems that arise when an attempt is made to solve the problem by producing conventional foods more efficiently, and on an extended acreage, need only be referred to briefly because they are already fully recognized and both national and international organizations are concerned with their solution. There is still some underexploited land, in Brazil and West China for example, that probably only needs fertilizers to be productive; but the main unused areas are either arid or tropical rain forest. We are now familiar with the possibilities, and political complications, of irrigation from large dams when the arid area is near a river such as the Nile; Gorrie (1960) has outlined some problems that arise from building a dam and Ionides (1961) has stressed the greater educational and social advantages that flow from having several small dams rather than one big one. The World Health Organization (1959), while recognizing the immense value of irrigation projects, also recognizes that they increase the incidence of bilharziasis and refers to it as a "man-made sickness". These are themes for social and medical research. The many other problems that can arise in the intensive development of even a small area are well set out in Gaitskell's (1959) book on the Gezira.

In principle, water for irrigation can be made from the sea and other abundant saline supplies. Dodge (1960) and Gillam and McCutchan (1961) estimate the costs of demineralizing by different methods; with more research more economical methods will presumably be developed, but would-be farmers in the arid fifth of the world are likely to go on looking enviously at those who get regular rainfall.

2.2. *Fertilizers*

At first sight, tropical rain forest looks extremely lush and productive and it is puzzling to find (see Linton, 1961) that it supports such a low population density. The usual explanation is that the soils are leached and infertile; Greenland and Kowal (1960) found in Ghana that the top foot of an acre of forest soil contained 11 and 580 pounds of phosphorus and potassium respectively whereas the growth on that acre contained 120 and 800 pounds. Hence the survival of the "slash-and-burn", or essartage, technique of farming. This approach to tropical soils amounts to an

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acceptance of the old idea that a good soil should produce crops indefinitely. Production in all highly productive areas of the world, such as Japan and the Netherlands, now depends on the regular return to the soil of the nutrients removed from it in the crop, although it was at one time possible to get crops there by agricultural mining as it still is in the virgin lands of the U.S.A. and U.S.S.R. Many wet tropical areas are probably not intrinsically worse endowed than a reclaimed polder in Holland; both need farming, draining and the use of fertilizers to make them productive. But scientific research is needed, in scattered areas by different methods and using different crops, to find out whether this expectation is borne out. The difficulties and interest of this work are illustrated by the conflict of opinion shown at a recent symposium on tropical land use (Holdridge, 1959) organized in Costa Rica.

Throughout the world, and especially in developing countries, even a casual observer can see that yields on adjacent farms vary greatly. Sometimes this is because some farmers use improved crop varieties but more often it is a consequence of differences in husbandry. The relevant differences are not always those that the farmer attaches importance to; we are all familiar with enthusiastic gardeners who get remarkable yields in spite of, rather than because of, their belief in composts and the importance of planting at certain phases of the moon. In every locality, therefore, there is need for research to discover the reasons for the success of the successful so that their techniques can be used more widely. Besides knowledge, people need the means to apply it. The two most obvious means are tools and fertilizers. The design of simple tools tends to be neglected because of the no-doubt-justified belief that agriculture throughout the world will in time depend on the large-scale machine operations that are standard in Europe and the U.S.A. But it will be many years before they are adopted universally and in the meantime the hoe will be the basic implement in much of the world. Designs vary fantastically; ergometric research is needed to find whether this variation has any agricultural advantage. Are there conditions in which handles are better long or short, and blades light or heavy? Some of the students in technical colleges in the developing countries might well resist the lure of remote but fashionable studies in nuclear physics and get down to this useful and equally scientific job.

The response of food crops to fertilizers in the temperate zone, and of plantation crops in the tropics, is well documented but more information is needed on the response of food crops in the tropics. Enough is, however, known (e.g. Richardson, 1960; Allen, 1961) to show that the general distribution of fertilizers would be the quickest way to increase food production in the hungry parts of the world. When applied sensibly, a pound of nitrogen will increase the yield of wheat, rice, or maize by 10 to 20 pounds. Except in acute emergencies therefore, it would seem preferable to send fertilizers rather than food to areas of need for they not only give a better return on the costs of transport but also start a useful cycle of technical development and objective thinking about the processes of agriculture.

2.3. *More Even Distribution of Food*

If our problem were simply to increase the total production of food in the world, an obvious first step would be to increase the use of fertilizers and the intensity of farming in the U.S.A. and U.S.S.R. But food is useless unless it is getting into hungry bellies. The food surplus in the U.S.A. is widely publicized and political capital is sometimes made out of the vast cost of storing it. Charity is a dominant human quality

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and feeling is widespread, especially in the U.S.A., that the surplus should be distributed; in 1959–60 646 M dollars† were spent on the distribution of wheat. Commendable as this is it is a stopgap rather than a solution. As the Director General of FAO (1961) puts it, "FAO has always emphasized that the ultimate solution to the problem of hunger and malnutrition in the less developed countries can be found only by raising their own agriculture to a much more productive level." The figures for wheat will serve to keep the possibilities in proper perspective. Aylward (1962) accepts the U.S. Department of Agriculture estimate that the world's annual wheat deficit is 44 M tons; 50 M tons are in stock in North America and this is the annual production there. Distressing as it is to have a surplus in one country and hunger in another, it is clear that the distribution of the existing surplus, which has accumulated over several years, would only allay hunger for a year.

2.4. *Prevention of Loss Through Pests and Diseases*

The control of pests and diseases has been an interesting and important scientific problem from the beginnings of science; the scale of research on these problems has increased dramatically during the last 20 years. This is partly because of general recognition of the amount of food that is being lost and partly because the manufacturers of the substances used to control pests and diseases have done much of the research and publicity themselves. There is no need, therefore, to enlarge on this theme here. It is commonly asserted that a quarter of the crops grown are destroyed by pests and diseases; the figure may well be accurate, to judge by the more specific estimates made by Ordish (1952) and the speakers at a Nutrition Society Symposium (1961). Ordish (1961) compares the cost of treatment with the value of the crops saved by insecticides and gets an average ratio of 1 to 4·7. In 1959 the control of an aphid vector by spraying increased the yield of sugar beet in Britain by 1 M tons, the sugar alone was worth 6·5 M£ and the value of the extra pulp and leaves is also considerable (*British Sugar Beet Review*, 1960). The greatest need at present is for more research on the effects of all these substances on the ultimate consumer of the protected crop. So much mystical nonsense has been written by believers in the merits of "natural" foods that most scientists show understandable impatience at the idea that there may be defects in existing methods of assessing the suitability for general use of the chemicals used in control. Logically, however, it is as impossible to demonstrate that a novel food component is harmless as it is to define the "natural" state. Some forms of sophistication indubitably improve and others damage foods; each case has therefore to be considered on its merits with demonstrated advantages properly compared against potential disadvantages. It may be added that the advantages should take the form of increased amounts of human food. Too often, convenience is the main reason for the addition or treatment; that of the baker or flour miller when nitrogen trichloride was added to flour, or that of the housewife who prefers a fat that does not spit on heating as in the recent troubles with toxic margarine.

† The usual convention in scientific writing is to express a quantity by a number followed by the unit, and there is an internationally agreed set of prefix letters to show the power to which the unit is raised. These invariably precede the unit, and the ones relevant in the present context are: k (kilo), M (mega), G (giga) and T (tera) for 10^3, 10^6, 10^9 and 10^{12} respectively. To avoid indices, which are hard to read, unwieldy numbers of 0's and the ambiguous billion, the convention is adopted here and there is no good reason for referring to quantities of money in a manner different from that used for other quantities.

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3. Reassessment of the Uses to which Land Plants are Put

The conclusion to be drawn from three of the preceding sections is that the amount of food could be increased by more intensive work on many well-recognized scientific problems and that there are also some novel problems awaiting investigation. Developments along all these lines will increase the crop yield; we have now to consider what is done with the crop after it is harvested. There are three main possibilities; it can be eaten as it is or after a little mechanical improvement, for example, by threshing or peeling; it can be subjected to a more elaborate process of extraction; it can be used as food for an animal. There is abundant scope for research on these procedures.

3.1. *Crops for Direct Consumption*

The world's principal foodstuffs—wheat, rice, maize, potato, cassava—are eaten directly and the main scientific problem that arises, apart from those that have already been considered, is breeding more productive varieties, or varieties that are tolerant of more pests and diseases, or of a wider range of climates. All these crops share the defect that their protein content varies from low to negligible. It is possible that varieties will be produced containing a more satisfactory amount of protein. It is more probable that the protein content of a predominantly vegetarian diet will be raised to the 15–20 per cent. that is now thought to be desirable only by more extensive use of leaves, immature flowers, legume seeds or of products derived from them.

Some figures for the production of those legume seeds that are used mainly, though not exclusively, as human foods are set out in Table 1. They are underestimates

Table 1

Production of legume seeds used mainly as human food, 1,000 *tons*

Country	*Dry beans*	*Dry peas*	*Broad beans*	*Chick peas*	*Lentils*
Italy	175	12	516	52	17
Spain	122	23	121	141	28
Mexico	519	—	28	108	3
Brazil	1,476	—	53	—	—
India	1,455	613	—	5,059	178
Japan	311	4	21	—	—
Egypt	—	—	210	6	42
Ruanda-Urundi	295	75	—	—	—
World total (excluding U.S.S.R. and China)	8,400	4,900	4,600	6,400	600
Total for all five	24,900				

Source: FAO Production Yearbook 1959; figures for 1958.

because much of the product of peasant agriculture is eaten by the producer and never weighed. Nevertheless the total, 25 M tons for the world excluding U.S.S.R. and China, is impressive; on the same basis, 28 and 14 M tons of soya beans and groundnuts were produced. These legume seeds vary in composition, but their protein content always exceeds 20 per cent. which means that they supply, or could supply, more than 5 M tons of protein. This is about a third of the weight of protein provided in the same period and area by milk, meat and eggs. A further basis for comparison

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is that 3 G people, each getting 55 g. of protein a day, need 60 M tons; half of this annual requirement is probably now being met by the main energy sources—wheat, rice, maize and potatoes. The present production of legume seeds (including soya and groundnut which have a higher protein content than the seeds listed in Table 1) could therefore supply half or more of the necessary protein supplement. Clearly, therefore, research is needed on all aspects of the husbandry of these seeds—their selection, manuring and protection from pests and diseases—for they are already an important and accepted source of protein-rich food.

There are no trustworthy figures for the production of leafy vegetables and immature flowers, such as cauliflower and broccoli, but it is obvious to any reasonably observant person in tropical areas, other than South-east Asia, that they are much less widely used than in Europe and North America. Although these vegetables are generally eaten as condiments, or for their vitamin and mineral content, or for reasons that are essentially magical, it is important to stress their value as protein sources. The whole group—broccoli, brussels sprouts, cabbage, cauliflower, endive, seakale, spinach and turnip tops—contain 4 to 5 per cent. of nitrogen of the dry matter after cooking. Not all of this nitrogen is present as protein but most of it is, so that 100 g. (wet weight) of one of these vegetables would supply 2 g. or more of protein. It would therefore be easy to get 5 to 10 per cent. of the day's protein requirement in this way, but the associated fibre limits the amount that can be eaten regularly. Nevertheless the encouragement of market gardening and the production of leafy vegetables is probably the quickest way to increase the available protein in many areas.

The varieties of vegetable that have been selected for use in temperate climates do not, in general, grow well in the tropics. Two scientific problems therefore arise that should be tackled simultaneously: the varieties that have been rejected in the temperate zone should be tested in other environments, and the "pot herbs" or "bush spinaches" already in use should be subjected to intensive selection. The analytical figures on some of them are extremely encouraging (Carr, 1956; Oyenuga, 1959; Byers, 1961); with modern technique, improvements comparable to those that our forefathers took several hundred years to bring about in such plants as the brassicas might be made in a few years (Pirie, 1959).

3.2. *Crops that are the Raw Materials for Biochemical Engineering*

The extraction of valuable substances from less valuable raw materials by chemical or intensive mechanical methods is several thousand years old, as the history of the wine and olive press shows. The latter, transmogrified into the screw expeller, is the basis of the modern vegetable oil industry which takes soya, groundnut, coconut, cotton-seed, etc., and presses out the oil carefully but disregards the fact that the world is in much greater need of the protein in the residue than of the oil. Few precautions are therefore taken to avoid damaging the protein; it is sold as cattle food or even as fertilizer. It is reasonable to assume that the protein in any one species of oil seed has a uniform feeding value when tested under the same conditions, but Duckworth and Woodham (1961) found that the gross protein value on a series of samples of soya bean meals varied from 58 to 97 and on a series of groundnut meals from 32 to 63. Even if these materials are being used only as cattle food, it would still be worth while to study more carefully the conditions of handling needed to maintain the higher feeding values.

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Soya bean meal contains highly flavoured substances and a trypsin inhibitor; these raise some problems, but by controlled heating and by adaptations of traditional methods of bacterial and fungal fermentation, acceptable and nutritious products can be made that are well-known constituents of foodstuffs in many parts of the world. Work on groundnut meal is less advanced but it is being vigorously undertaken in India. A recent Symposium on Proteins (1961) included five papers on the technology of processing groundnut meal and incorporating it into various foodstuffs. Factory production has already started on a small scale and there are plans for a unit that will make 2 tons of protein daily. In Central America cotton-seed meal gets more attention. It is an important component of Incaparina, a foodstuff developed by the Instituto de Nutrición de Centro América y Panama in Guatemala especially for young children (Scrimshaw and Bressani, 1961). Coconut protein is in a relatively underexploited state. This is mainly for the historical reason that it was at one time necessary to dry copra so as to preserve it for the long journey from the Pacific to a processing factory in Europe or the U.S.A. Now that processing is done locally there is no reason for this drying which often damages the protein and always dematures it so that its separation from the carbohydrate and fibre of the coconut meat becomes difficult if not impossible. But the drying is traditional, and remains usual, so that there is only 20 per cent. of protein in the residue and this, in a product with a high fibre content, is too low for a useful protein concentrate. Research is now being undertaken on processes in which the fresh coconut meat is ground and an emulsion of oil and protein in solution is separated from the fibre, the emulsion is then separated and the protein recovered by coagulation. The concentrate made in this way is palatable and contains 30–55 per cent. of protein so that it is a useful additive to protein-deficient foods. The merits of various methods of making this separation are discussed by Bhatia (1960) who makes it clear that much research remains to be done.

There is little opposition to the idea of using oil seed residues as the raw material for making human food because three of the seeds are already used extensively in the unfractionated state, and cotton-seed meal is acceptable by analogy. The idea of using leaf protein is less readily accepted. If leaves of any species are already being used as a "spinach" there is no immediate advantage in extracting protein from them; a propaganda campaign in favour of eating the green vegetable itself would have as much effect on the protein intake of a community. If leaves of an inedible species are proposed, prejudice is easily excited by pretending that what is proposed is the eating of grass. Leaf meals have in fact been used in some diets on a small scale because of the contribution they make to the mineral and vitamin intake, but they are quite unsuited to general use because of the risk that too much will be eaten at one time. A spice or condiment can safely be put on the market, although it has a high fibre content, because no one is likely to eat very much, but a main food component cannot. In spite of the difficulty that is to be expected in getting the product accepted, work has gone on at Rothamsted and elsewhere during the last 25 years on the extraction of leaf protein in bulk.

The process of extraction is simply a scaled-up version of processes used in the laboratory from the time of Rouelle (1773). Fresh leaves are passed through a pulper so designed that it cannot choke (Pirie, 1952; Davys and Pirie, 1960), or they are flushed through in a stream of water (Chayen *et al.*, 1961); the juice is then separated from the leaf fibre and the protein in the juice is coagulated and filtered off. With some leaves it is advantageous to add alkali during the extraction (Crook, 1946),

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but we prefer to avoid this whenever possible. The final product is easily incorporated into palatable foodstuffs (Morrison and Pirie, 1960) and its nutritional value, measured in experiments with both pigs (Duckworth *et al.*, 1961) and infants (Waterlow and Cruickshank, 1961), is satisfactory. As would be expected, the protein is not as good as milk protein but it is as good as, or a little better than, fish-meal. A great deal of research remains to be done. The design of the extraction machinery must be adapted to the technical level of the part of the world in which it will be used, the methods of presentation must be adapted to local food preferences and, most important of all, crops that are adapted to the local climate, and that can be processed, must be found.

Ideally leaves that are a by-product, such as those from sugar cane, ramie or jute, would be used and ultimately one can envisage the breeding of special "multipurpose" plants in which nearly every part would give something of value (Pirie, 1962a). The first trials will be less ambitious and will depend on crops normally grown for some other purpose; some that are suitable have already been found in Ghana (Byers, 1961).

In this section and the preceding one emphasis is put on leaves for many reasons; they are at present relatively neglected and so present novel scientific problems; they can give a higher yield of dry matter per acre per year than more conventional crops; they can be harvested at a shorter interval after sowing than other crops. In regions with a fickle rainfall or where devastation by pests is a common occurrence this short interval may enable a crop to be harvested while the water lasts and before the pest arrives; it introduces some flexibility into a system of agriculture. Another important, but one hopes temporary, reason for stressing the quick return given by a leafy crop is well stated by Waddy (1961):

> "One of the main nutritional problems of primitive agriculturalists is the seasonal nature of food supplies, plentiful after the harvest (with luck) but scarce or non-existent at the time of planting, when the hardest physical labour of the year must be undertaken. A vitally important fact is that the actual productive work, breaking up ground and planting, on which the size of the subsequent harvest depends, occupies only five or six weeks after the fall of the season's first rain. At this all-important time the people are most likely to be weak from lack of food. Moreover, the fall of rain results directly or indirectly in the accentuation of several crippling endemic diseases; mosquitoes breed and malaria becomes epidemic; yaws, the rheumatism of the tropics, is exacerbated; guinea-worms come to the skin surface and may immobilize a large proportion of an infected community—all this at a period when a week's illness or disablement means the loss of a fifth of the year's productive work."

Leaves and their products are often disregarded because they are perishable and there is a tendency, especially in governments and the international agencies, to think of nutrition in terms of stable packages of food. This I have stigmatized as "super-market nutrition" (Pirie, 1962b). Packages have their place but so have fresh foods. The preservation of food is an active facet of research now, and it is reasonable to expect improvements in the technique and a wider distribution of existing techniques such as refrigeration. By the time seasonal surpluses are beginning to appear, in what are now areas of need, there is therefore good reason to assume that there will also be the means of preservation.

When a plant is being used as a source of food for direct consumption, its physiological and morphological habit is the main criterion restricting our choice. The ability to give a high yield per acre per year is important, but it is not so important as

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the ability to give something, e.g. a seed or tuber, that is easily made edible. When the plant is to be processed mechanically, this restriction is largely circumvented and our choice of potentially useful plants can be more influenced by their photosynthetic capacities. The study of these capacities is not only useful but it is scientifically very interesting. An important conclusion from recent work is that the maximum rate of photosynthesis, measured by the accumulation of dry matter, is similar in a diverse group of plant species. Under field conditions, with measurements made on areas of significant size and for a full year, the highest recorded crop yield is 30 tons (dry matter) of sugar cane growing in Hawaii (cf. Blackman and Black, 1959; Bunting, 1962). Under laboratory conditions, in experiments conducted during the period of summer growth, it is easy to get higher rates of production, thus sugar beet reaches 45 tons per acre per year (Owen, 1959) and maize 40 (Rothamsted, 1961). These rates are not maintained throughout the year, but they are important for two reasons; they give an indication of the yields that it would be reasonable to expect under market-garden conditions in the wet tropics and they are the yields with which claims for the productivity of algal culture should be compared. Enthusiasm for algal culture as an alternative to agriculture is now declining as people realize (e.g. Wassink, 1959) that the figures quoted at one time were yields in short-term experiments under specially favourable conditions. When higher plants and algae are compared in comparable conditions they have comparable capabilities and the techniques of husbandry are very much simpler with the higher plants. When higher plants are grown in water culture, conditions comparable to those used with algae, the yields are even higher; thus Arnon got a rate of formation of dry matter in the form of potato tubers of 60 tons per year per acre (Allen, 1958); had leaves and stalks been included, the yield would have been about doubled. More general aspects of photosynthetic efficiency are discussed by Ovington and Heitkamp (1960) and by Bray (1961), their conclusion being that even woodland in temperate climate, given only the care usual in a forest, can use up to 7 per cent. of the incident light for photosynthesis; this contrasts greatly with the figure of 1 per cent. that is commonly quoted as the agricultural yield. An important scientific problem, therefore, is the creation of systems of husbandry that can use this large potential photosynthetic capacity to produce human food. An interesting list of trees suitable for use in the wet tropics is given by Santiago (1961), and Pirie (1962a) points out the advantage of growing leafy crops under an open forest canopy.

3.3. *The Conversion of Crops in Animals*

A green plant synthesizes organic matter from carbon dioxide, water, and the simple nitrogen compounds and minerals of the soil; plants vary in the efficiency with which they do this and they synthesize products of very varying value in human nutrition, but they are all making something *de novo*. Animals, on the other hand, though their products are highly esteemed in many parts of the world, simply convert the material the plants have made into another form and waste most of it in the process. Because they waste carbohydrate more quickly than protein they act as protein concentrators. The pejorative word "waste" is sometimes objected to because animals do not destroy the elements but breathe most of the carbon back into the atmosphere as CO_2 and return most of the nitrogen to the soil as urine and faeces; if that argument were valid, a lathe operator who damaged nine-tenths of his stock so much that it had to be returned to the smelter would not be considered wasteful either.

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There is little agreement about the extent of the waste, or the conversion efficiency, because three things get confused with one another. The physiologically interesting question of the ratio, during a period of one or two weeks, of the food eaten to the increase in body substance or production of milk; the ratio, of interest to a producer who buys young animals and keeps them for a limited time, of intake to production during the productive phase of an animal's life; and the ratio of the amount of food needed to maintain an average population of the species under consideration to its total edible product. Only the last is of significance in the present context. So far as I know this relevant ratio has not been measured. I will be surprised if the efficiency of any animal is as high as 15 per cent.—10 per cent. is a more probable maximum. Most of us welcome this waste because we relish animal products and it can be given a more prosaic justification because it introduces elasticity into the nutritional policy of a country. In a period of adversity much of the animals' food can be diverted to people. The magnitude of the waste in animal conversion inspires much scientific work on methods of increasing efficiency by developing improved breeds, using tranquillizers, and avoiding hitherto neglected infections, etc. All these problems are now getting ample attention.

That statement of the problem of animal conversion in terms of efficiency is only valid when people and animals use the same food. With some animals this is the position. Aesthetics apart, the diet on which pigs and chickens will show the high productivity often quoted could have sustained people. Ruminant animals, because of their ability to digest cellulose, are in a different position and can use fodder that is of no direct use as human food. This illustrates an important aspect of the scientific problems arising from animal conversion—the peculiar physiological merits of each animal need careful study to see whether they are being fully exploited. Nitrogen metabolism in the ruminant is one of these peculiarities because the rumen flora can not only digest cellulose but also make use of simple nitrogen compounds. These faculties are still underexploited and insufficiently recognized. High-quality herbage is largely wasted on a ruminant (Chalmers and Synge, 1954); it would be better to use, as food for non-ruminants including man, the protein that can easily be extracted and let the ruminant have the fibrous residue fortified, if necessary, with urea or other simple nitrogen compounds. This is already done but difficulties arise on which much more work is needed; for example, urea has probably sometimes failed as a supplement because the bulk fodder was more deficient in phosphorus and sulphur than in nitrogen.

Simple nitrogen compounds are relatively cheap but they still deserve economical handling. A little attention should therefore be given to physiological methods for their conservation. An important function of the kidney is the removal of urea and other metabolic end-products from the blood. In principle there is no need for this; so long as energy is available these products could have been resynthesized to amino acids so that the cycle of protein synthesis and destruction in the course of metabolism could go on using the same nitrogen in a closed cycle. In most natural environments there would have been no advantage in this because the plants on which animals depend supply sufficient protein. This is not so when straw and other types of withered herbage are the main fodders and it is interesting to note that camels (Schmidt-Nielden *et al.*, 1957) and sheep (Somers, 1961) excrete part of the urea back into the digestive system where it can be reconverted to protein by the rumen flora. This is a useful piece of conservation that deserves more attention than it gets from those who select and breed domestic animals.

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It is generally assumed that the domestic animals of Europe and the U.S.A. should be used throughout the world, but in recent years there has been some reaction against this view. The argument seems eminently logical that, because grazing animals select what they eat, a mixed fauna would make more efficient use of the natural mixed flora of rangeland than a single species can. Worthington (1961) and several speakers at the Arusha Symposium (1961) on the *Conservation of Nature and Natural Resources in Modern African States* quote figures showing that, with care in culling and keeping predators in control, mixed wild animals are more productive than domestic animals on adjacent areas. Some of these figures are set out in Table 2.

TABLE 2

Comparisons of the yield of wild and domestic animals on different types of unimproved land, taken from the papers given at the Arusha Symposium (1961)

Type of land	*Yield expressed in terms of:*	*Land grazed by*	
		Domestic animals (cattle, goats, sheep)	*Wild animals (blesbock, eland, wildebeest, springbock)*
Rangeland	Kg. liveweight gain per animal per day	0·14	0·19–0·24
Acacia savannah	Kg. liveweight maintained per hectare	20–28	157
Bushland	Kg. liveweight maintained per hectare	3·7–13·5	52·5
Black cotton chernozem	Number of animals per sq. mile	34	134
Depleted land	Kg. liveweight per hectare	15·5 and land got more depleted	31 and land recovered

The meat is palatable and sells readily. The case is convincing for so long as land is so abundant or water so scarce, that nothing is done to up-grade the rangeland, or when rangeland is being kept in its "natural" state as an amenity. Ecological research is then needed to devise an optimal mixed fauna for each area. But there is no reason to think that wild animals would be preferable on land that can be carefully farmed and brought into the high-yielding condition to which domestic animals are adapted.

As we become more tolerant of animals other than those generally used now, the possibilities of improved breeds of other species will arise. Settlers from the technically or militarily accomplished countries have taken their animals with them. The results, as the cattle industry of Argentina and the sheep industry of Australia show, are often good, but in parts of the wet tropics it is difficult to get European breeds to thrive. The position is similar to that already touched on when vegetables were being discussed; when there is no traditional domesticated local animal, introduced domestic animals may have an advantage. But the potentialities of domesticated forms of what are at present wild animals have not yet been explored.

4. The More Efficient use of Swamps, Lakes and the Sea

Particularly interesting results are to be expected when thought along these lines is applied to swamps because the conventional approach to them—drainage and farming—is expensive and can only be undertaken slowly. Worthington (1961),

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and Blower and Brooks in their paper to the Arusha conference, give figures for the productivity of hippopotamus farming. But the hippopotamus comes out on to the land to feed and so competes with other land animals. To make full use of vegetation growing in water the potentialities of other mammals need investigation. The manatee (Alsopp, 1960, 1961) is an attractive possibility. It is wholly aquatic and its flesh is readily eaten in Guiana; so readily in fact that the proposal that it should be used to control water weeds such as *Eichhornia* has been criticized on the grounds that it would be slaughtered for food. This criticism seems to miss the point. So long as the weeds are eaten by an animal that is then used as food, it does not matter who does the slaughtering so long as an animal population is maintained that is in accord with the productive capacity of the swamp.

The development of fisheries is now getting widespread and welcome attention and no praise is too high for the work done on fishponds in Israel and Indonesia, and on marine fisheries by Norwegians in South-west India and FAO in Chile. This work covers both methods for improving the yield from confined areas of water by adding fertilizers to increase photosynthesis in it, and for improving the methods of locating and catching fish (Scharfe, 1962). It is a little humiliating that our estimated annual catch of sea fish, 25 M tons wet weight, is only four or five times the catch by sea birds off the coast of Peru.

The potentialities of the sea are enormous; measurements in unfertilized tropical ocean suggest that photosynthesis can produce 35 tons of dry matter per acre per year (Billings, 1957). Fox (1960) estimates that 1·5 T tons of suspended organic matter is available in the ocean as food for filter-feeding animals. These large quantities have encouraged many people to think of the mechanical separation of plankton and organic detritus, but Jackson (1954) estimates that it would cost £10,000–£20,000 to collect a ton (dry matter) and what was collected would be largely silicious spicules. At present, therefore, it seems likely that the collection will be left to marine animals; Fox (1960) estimates that some of them have a 12–17 per cent. efficiency as converters. There is clearly room here for research once we get away from the idea that marine faunas other than fish are either inedible or expensive delicacies. Chance seems to be the main factor controlling the choice of category into which each species, or part of a species, is put. The matter can have important political repercussions as is shown by the difficulties the Food for Peace Agency has run into in its efforts to distribute a product containing material not normally eaten although there is no reason to suspect it of being unwholesome (see *Science*, 1961).

Even if attention is confined to fish, much could be done to shorten the food chains between aquatic plants and the fish that are finally eaten by people. These fish are predominantly carnivorous and many of them feed on other carnivorous fish. The fish we eat is therefore the end-product of two or three stages of conversion. If each is only 10 per cent. efficient, it is easy to see the basis of Lucas's (1951) estimate that less than 1 per cent. of the plant material eaten reappears as human food. Carp and other herbivorous fish are important in fish ponds; it will be more difficult to encourage herbivorous marine forms but it should not prove impossible.

Photosynthesis in the sea is severely limited by the low concentrations of N and P. In controlled areas of water these elements could be added but fertilization is hardly practicable in open water—most of the fertilizer would spread into the dark unproductive depths instead of staying in the surface layers as it does in soil. The N-fixing blue-green algae are generally held to be absent from, or rare in, the sea. Much more effort should be put into the selection or production of strains that can

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live in sea-water; this would be a much more useful aspect of algal research than those now getting intensive support.

A peculiar feature of the flowering plants, on which almost the whole of agriculture depends, is that few species tolerate sea-water. But some, for example mangroves and turtle grass, tolerate it. If more strains could be found or produced with this capacity, sea-water could be used for irrigation and extensive tracts, the West coast of Africa for example, could be opened up to agriculture. There is no obvious physiological reason why this could not be done and the endeavour seems at least as likely to succeed as the de-salting of sea-water on an agricultural scale. At present emphasis is put on the latter processes because it is easier to get money for developments in physics and engineering than in biology.

5. Microorganisms as Converters

Ruminant animals have three merits as producers of food: they can live on rangeland that would be difficult to farm intensively or that we prefer not to farm; they can use fibrous plants, or parts of plants, that are useless in most animals with a single stomach; their need for protein can, in part, be met by the simple nitrogen compounds produced by the chemical industry. The last two merits depend on the activity of the rumen flora and many other microorganisms share them. By the mass culture of these organisms food can be produced in a tank without the losses inseparable from maintaining a ruminant. This is a complement to agriculture and not an alternative to it because, except for the algae and a few photosynthetic bacteria, microorganisms depend on fermentable sugars produced by the higher plants. The physiological peculiarity of microorganisms is that they can use the soluble constituents of what would otherwise be waste products, e.g. sewage, molasses, wood-pulp effluents, etc.; they have indeed often been grown in bulk more as a means of disposing of a noxious waste than as a means of getting more food. When chemical and mechanical methods for extracting protein and other valuable components from plants are used on a significant scale, the culture of microorganisms on the resulting liquors will be an essential part of the process.

Yeasts are the most widely used microorganisms and there are many different species—Cerevisia, Saccharomyces, Torula—adapted to different substrates and for different uses. The methods used for their culture are described by Pyke (1958); those used for other microorganisms are too diverse to list. Microorganisms are now well known as producers of antibiotics, the mycelium is then a by-product that can be used as fodder. The use of *Eremothecium ashbii* to make riboflavine, Streptomyces to make vitamin B12, and various organisms to make lysine (for references see Owen, 1960) are important nutritionally but, pursuing the main theme of this paper, most emphasis will be put on protein production. Anderson and Jackson (1958) collect together figures for the amino acid composition of many microorganisms; they vary widely and this suggests that a thorough search should be made for those that use in their structure those amino acids that are essential and commonly deficient in human diets—methionine and tryptophan come first to mind. Many experiments have been published (e.g. Kaufman *et al.*, 1957; Rastogi *et al.*, 1961) showing that yeast is a useful animal food and it is produced as a human food in many parts of the world, though mainly as a condiment or source of vitamins.

Many questions remain to be answered before microorganisms get general recognition as foods. Efforts to use yeast have not always been successful but it is not clear whether these failures were a consequence of the flavour, or of contamination

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with other organisms, or because of the presence of unhydrolysed metaphosphate, or for some other reason. Work in the laboratory is needed and it would be foolish to embark on further large-scale production of microorganisms as human food until the reasons for earlier failures have been fully explained.

6. The Synthesis of Food

Many people, unfortunately, understand human energy requirements so little that synthetic food conjures up ideas of food degenerating into a few pills eaten at odd moments. This is obvious nonsense because the necessary 2,000–3,000 k calories a day cannot be got from less than 500 g. (dry weight) of food and it is more likely to weigh 800 g. By whatever means food is produced, it will have much the same bulk as at present and there is no reason why it should not have a similar general form. This is a problem for food technologists and it arises as soon as an attempt is made to make a new form of food, e.g. from oil seeds or microorganisms, rather than to increase supplies of a familiar food. The development of this aspect of food technology will be slow because the countries that are best able to undertake it are not those that suffer at present from a food shortage; furthermore, food technologists show a surprising lack of imagination. Considering the scale on which protein-rich low-calorie diets are now being eaten, it is amazing that they come as powders out of a tin, like a cleaning powder or material for repairing holes in plaster, and have not been given a more interesting texture. Perhaps those who eat them wish to mortify the spirit as well as the flesh.

The direct synthesis of food raises this problem in its most acute form and it may seem remote from the problems of improving food supplies in the developing parts of the world. But these parts are now called developing rather than underdeveloped and this emphasizes the fact that technological change is to be expected. There is no likelihood of substances being synthesized in Europe or the U.S.A. and then shipped to the places of need; if anything were being shipped it might as well, considering the scale of synthesis possible during the next 10–20 years, be one of the existing surpluses. But, if local synthesis is contemplated, it is worth while considering what might be synthesized.

The total or partial synthesis of vitamins is already well established and will continue; they will be needed therapeutically and many traditional, and most novel, foods are vitamin deficient. There is little prospect of carbohydrate synthesis because plants make starch and sugar very economically. Fat was synthesized on a 2,000 ton-a-year scale in Germany during the 1939–45 war from petroleum and Fischer-Tropsch hydrocarbons. This may have been wise under the special restrictions of wartime because the oil-bearing plants tend to be tropical, but soya, groundnut and other sources of oil are now grown more widely; this reason for a local shortage seems unlikely to recur. So far proteins have not been synthesized. This does not matter because it is their component amino acids that are needed in food and these can be synthesized. Table 3 gives a list (White, 1961) of the estimated prices of eight out of the ten that are essential for human nutrition if they were being made in bulk by synthesis or fermentation. Lysine and methionine are already being added to human and animal food because many plant proteins are deficient in these amino acids. So long as what is envisaged is the supplementation of a protein with 1 per cent. or less of an amino acid, the prices in Table 3 are reasonable. But if what is envisaged is the total replacement of protein by a mixture of synthetic amino acids it is clear that the mixture would cost 2–3 U.S. dollars per pound; this is the cost of the protein in

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medium-priced cuts of meat. The costs of synthesis will, no doubt, fall; it is, however, worth remembering that up-to-date technology has already been applied to the problems of large-scale synthesis, whereas it is only beginning to be applied to processes of protein extraction and to the conventional methods of producing food.

The amino acid composition of the edible parts of plants depends on the variety and the time of harvest; thus Fowden and Wolfe (1957) found varieties of maize with only 59 per cent. of the isoleucine and 56 per cent. of the lysine that they found in

TABLE 3

Prices of amino acids produced by synthesis or fermentation

	U.S. dollars per pound
DL-Valine	0·85–1·45
DL-Leucine	0·90–1·60
L-Isoleucine: D-alloisoleucine	1·05–1·80
L-Lysine hydrochloride	2·00–3·50
DL-Threonine (75 per cent.)	1·75–3·10
DL-Phenylalanine	1·05–1·80
DL-Tryptophan	4·70–8·00
DL-Methionine	1·43

Source: White, 1961.

the better strains. Much more work is needed on these differences. They have been largely neglected hitherto and varieties have been selected for use with little regard to their nutritional value. There is no basis for the casual assumption that plant proteins have a lower value than animal proteins; the generalization has some foundation if attention is restricted to seed proteins but even with these there are clear possibilities that expensive supplementation could be avoided by suitable selection.

If food synthesis is being undertaken, attention should be concentrated on substances that are easy to synthesize. The most probable members of the group are set out in Table 4; they are chosen because they are small molecules and only one, alanine,

TABLE 4

Some easily synthesized foodstuffs

Acetic acid	$CH_3.COOH$
Alcohol	$CH_3.CH_2OH$
Glycine	$NH_2.CH_2.COOH$
Alanine	CH_3 $NH_2.CH.COOH$
Beta alanine	$NH_2.CH_2.CH_2.COOH$
Glycerol	$CH_2OH.CHOH.CH_2OH$
Succinic acid	$COOH.CH_2.CH_2.COOH$

introduces the complication of stereoisomerism. Although they all appear, either free or combined, in conventional foods, only two, acetic acid and alcohol, are generally relished and then not primarily as foods. But each could supply 2–3 per cent. of the daily calorie requirement and alcohol has been used to supply much more than this (Wilkinson, 1955). Altogether, therefore, the group of substances in Table 4 could supply 10 per cent. of the calories needed by a country and it would be much more

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economical to get energy in this way than by the excursions into food synthesis that have been undertaken or discussed hitherto. All these substances are soluble and so raise few problems in presentation.

7. CONCLUSION

There are so many unexploited or underexploited possibilities for getting more food that the outlook may, at first sight, seem encouraging. There are various disquieting features in it. The existing shortage is great and the annual increase in population is now as great as, if not greater than, the increase in food production. When the full impact of sanitation and medical services is felt in the developing countries it is reasonable to expect a still greater rate of population increase. Thus some Hutterite communities in the U.S.A. only take 16 years to double their numbers (Eaton and Mayer, 1953) and a whimsical extrapolation from existing trends gives 2027 as the date when the population will approach infinity (Foerster *et al.*, 1960). A perfect technique of contraception is not an immediate necessity, but it will become a necessity sometime and the sooner it is devised the less likely become the rhadamanthine population control methods that are sometimes forecast. If no baby were conceived unless positively wanted, that is to say, if conventional contraception were inverted and a couple had to do something to restore an otherwise inhibited fertility (Pirie, 1961), it is at least possible that the problem of over-population would solve itself. Work along these lines is as greatly in need of support as work on increased food production.

Research on the more radical methods for getting more food is mainly restricted by lack of imagination; not the imagination of scientists—that may be inadequate but it is abundant compared with the imagination of those who control the money. This shows itself in many ways. A great deal of effort is put into excellent projects that are well worth the money they cost but that cannot be expected to increase world food production by more than a few per cent. Many of the themes discussed in this paper could be investigated by the diversion of a small part of what is now being spent. This diversion meets with little official sympathy and a group of standardized arguments regularly appear—that it is not certain that the project would work, that the product might not have the expected value, that consumers might be reluctant to accept novelty and so on. Scientists find this set of reactions tedious; had they not already recognized all these possibilities, they would not have claimed that they were trying to do research. Besides some diversion of money already allocated for work on food production, there is need for much more money. One is often told that there is now more money for research than there are scientists with ideas to make use of it. This may well be true with research on space and atoms; but in the middle territory of plants, animals and people, which is much more useful than the two extremes, it is certainly not true. The difference in the esteem in which the biological, rather than the more physical, aspects of research is held is shown in the different salaries offered to directors of research institutes. These were collected together four years ago (Pirie, 1958); since then most of the rates have gone up but the disparity remains.

The main effect of shortage of money is, however, felt at a lower level. It is generally easier to get endowment for a piece of biological research that is purely academic, or at any rate unlikely to have any effect on practice for several years, than to get it for research that has a practical orientation. One reason for this is that practical research tends, by its very nature, to straddle the domains of interest of the different grant-giving bodies. It is easy, therefore, for such a body to postpone

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decision by saying that the proposed work would be more properly financed by someone else or done in a different institute from the one proposed. In this way work can be held up or restricted for many years without anyone incurring the obloquy of actually condemning the idea that it should be done. There is no reason to think that the scientifically productive nations differ in this tendency. It is, for example, wise to remember that the four Regional Laboratories of the U.S. Department of Agriculture, which have an excellent record of useful work on borderline subjects, came into being in an essentially accidental manner.

Sukhatme (1961) concluded his review with the words, "The broad conclusion thus is that should population grow according to the U.N. medium forecast, the world's total food supplies would have to be doubled by 1980 and trebled by the turn of the century in order to provide a reasonably adequate level of nutrition to the peoples of the world." I have tried to outline some useful directions for research and some of the obstacles that keep research from being done. The greatest obstacle is the failure of those who finance research to recognize the scale of the problem.

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Future Sources of Food Supply: Economic Problems

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Summary

A large proportion of the world still lives by primitive forms of agriculture involving labour for only part of the year. However, without fertilizers, etc., they cannot increase productivity on their limited acreages and it is difficult to use the labour elsewhere than on the land. As agricultural productivity increases, rural communities may devote about a third of this increase to improving their diets, urban communities more.

1. Introduction

The scientific study of the economics of food supplies, as of any other economic issue, requires the finding of some patterns and uniformities in the bewildering multiplicity of human actions. Out of this very large field, though some mention will be made of agriculture in the wealthier and more productive countries, I shall direct attention principally to the problems of agriculture and food supplies in the poorer communities (representing, however, a preponderant proportion of the earth's inhabitants).

An economic problem arises, too, in respect of the really poor and backward communities, who live a great distance away from railways, roads, shops, government offices and the like. These people live perforce almost entirely on food which they can produce in their own villages; and will have to continue to do so until methods of transport and distribution are greatly improved. An attempt to feed them on imported food, generously donated by governments at the other end of the world, would raise very serious transport and distribution problems. Lacking other means of transport, loads of food would have to be carried for long distances on men's heads. Paying the lowest possible wages to the porters, it nevertheless doubles the cost of grain to carry it in this way for 75 miles.

2. The Physiological Minimum

Though the time has not yet come, we hope that in due course physiologists will be able to give us precise information on all the elements required for human nutrition, and economists will then be able to apply ingenious methods of working out least-cost diets. Physiology, or at any rate this branch of it, appears to be almost as imprecise and changeable as economics, judging from the extent to which doctrines believed in the 1930's have now been abandoned. It was then said that we should take at least half our protein in the form of animal proteins, preferably from milk. Medical examination of a religious sect of strict vegetarians (so strict that they even refuse eggs and milk), however, has shown that they remain in good health, if they consume a sufficient quantity of their preferred diet. Likewise, the former doctrine that a substantial proportion of all our calories had to be consumed in the form of fat now appears to have been abandoned—many doctors indeed now warn us about the

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dangers of consuming too much fat, particularly the fats from ruminant animals, and most of all the fats in milk.

Regarding the primary element in man's food requirements, namely calories, FAO (1950, 1957a) published what appeared to be excellent summaries of the best obtainable scientific information to date. The report (1957b) commissioned by FAO on protein requirements, on the other hand, was extremely inconclusive.

In placing an economic interpretation upon physiological statements of food requirements, therefore, we shall begin by assuming that people eat most of their food in the form of grain (which is indeed true of poor communities throughout the world). From our knowledge of their chemical composition it appears that, with most grains, if they have obtained their calorie requirements they will have obtained their protein requirements too. Where, however, they obtain a substantial proportion of their calories from root crops (potatoes, sweet potatoes, yams, cassava, etc.) they are in danger of protein shortage. If, on the other hand, some of their diet consists of groundnuts and similar oil-bearing seeds, they will get a better quality protein.

Shortages of certain vitamins and minerals, of which the body requires a great variety, though mostly in very small quantities, may nevertheless have very serious effects. Such shortages, however, are most likely to arise among urban communities, consuming a limited range of foods, which have been transported, processed and stored in ways which may have caused them to lose some of their mineral and vitamin components. Such shortages appear much less likely to arise in a population consuming foodstuffs in their raw form, supplemented by a variety of other natural products, which may include wild salad plants, seaweeds, snails and many others. The meat, dairy produce, fruit, etc., which predominate in our diets, while much more agreeable —none of us would like to live on a diet computed to meet physiological minimum requirements, consisting mainly of porridge—nevertheless should be regarded as amenities rather than physiological necessities.

3. Community Requirements

When we proceed from stating the physiological requirements of an individual in defined circumstances to stating the average requirements of a whole community, we must, as FAO point out, proceed with care. We certainly should not, as is often done, take a single figure as a world-wide calorie standard. Requirements of calories, in the first place, are functionally related (though not directly proportional) to the weight of the body. They depend upon the surrounding temperature—less food is required to maintain body temperature in a warm climate than in a cold one. The requirements of men and women differ; active physical work increases calorie requirements, as also do pregnancy and lactation. A population with a high proportion of children (as is the case with most of the populations of Asia and Africa) will on that account require rather fewer calories per head than a population with a greater percentage of adults.

From the FAO report, therefore, tables were extracted relating to such a population, with both birth-rates and death-rates fairly high, a high proportion of children in the population, and also a high proportion of pregnant and lactating women. Temperature scales were chosen for four areas in which the level of nutrition may be expected to be low—Central Africa, India, and northern and southern China. (Temperatures could only be approximately estimated from examination of isotherms; the estimates were made for the four quarters of the year and averaged.)

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For such a population the FAO requirements are shown in Table 1.

We have unfortunately extremely little information about the average weights of populations, even in the advanced countries. The range of weights shown in the table represents, however, the extremes likely to be encountered—adult women averaging generally 10 kg. less body weight than men. In Northern Europe and North America it is possible that the *average* body weight of adult men rises as high as 70 kg. (154 lb.). A weight as low as 50 kg. may be found in southern India and South-east Asia, but only rarely elsewhere, except in one or two special groups, such as the African Pygmies.

TABLE 1

Average calorie requirements per head of population in varying circumstances

	Average annual temperature degrees centigrade	*Calories per day per head of population when average weight of adult males is*			
		65 *kg.*	60 *kg.*	55 *kg.*	50 *kg.*
Central Africa	26·6	2,212	2,121	2,029	1,934
India	25·5	2,224	2,133	2,040	1,944
Southern China	20·4	2,285	2,191	2,096	1,997
Northern China	9·2	2,414	2,315	2,214	2,110

If people's food falls short of their calorie requirements it does not follow that they die. They may indeed live for years on end on an inadequate diet. Resistance to many kinds of illness, however, is indubitably weakened, and both sickness and mortality are much greater than they would be if the community were better fed. The FAO experts describe what happens in such a community—"The whole manner of life is adapted to an insufficient supply of calories, with results that are socially undesirable: lack of drive and initiative; avoidance of physical and mental effort; excessive rest."

We have some information about the calorie value of food consumption in low income communities, computed both from national averages and from field studies. Outstanding among the latter is the classical study by Buck (1937) who made a comprehensive agricultural survey of 136 Chinese villages in 1929–33, on behalf of the University of Nanking. A student spent an entire year, keeping records of production, consumption, equipment, etc., in each of the villages surveyed. For few European countries, let alone other Asian countries, do we have such comprehensive information.

In the Yearbook of Food and Agricultural Statistics published by FAO a table is published showing the protein and calorie content of the average diets of certain countries. Contrary to the usual trend, the countries for which information is now published constitute a much shorter list than that which was published a few years ago; this is a tacit admission that much of the previous information was highly inaccurate.

In the early 1950s some very low figures were quoted of Indian grain output and calorie supply per head, and many people still think in terms of these figures. During this period, however, when there was a widespread prevalence, or at any rate threat, of compulsory state purchase of grain, there was every inducement for the cultivator to understate his grain output, so as to avoid compulsory purchase, to leave himself some grain for sale on the black market, or possibly even to qualify for a ration himself.

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The official estimates were almost certainly too low. In 1947 I had a most illuminating conversation on this subject with Gandhi, who was very interested in economics. He pointed out to me what was happening, and strongly criticized Mr Nehru's policy. "Drop rationing and price control", he said, "and every Indian would have to work harder, which he could well do." Nobody but Gandhi could have been so frank on this subject. On the other hand, the willingness of the Indian government over this period to devote foreign exchange, which was badly needed for other purposes, to the purchase of grain, shows that it considered the situation dangerous.

Most of the countries for which information is now published by the FAO are economically advanced countries in which there is no question of a lack of calories. Among the low figures entered is one of 2,210 calories for Japanese consumption, which might at first sight appear to be barely adequate. In Japan, however, people willingly devote a substantial proportion of their food expenditure to fish, fruit, vegetables and other products whose calorific return is comparatively low per unit of money spent; they would not do this if they were hungry. The average Japanese body weight is still low (though now found, among school children, to be rising rapidly) and, for a temperature which can be taken as intermediate between that of north China and south China, a diet with this calorific value would appear to be adequate.

Another low figure given for recent years is that of 1,950 calories per head for India. Though Indian agricultural statistics are improving, no one well acquainted with the subject could say that they are yet precise. Good field studies in India are still scarce. Recently an enquiry was started, under international auspices, with a view to making fully detailed records of food production and consumption in certain selected villages. (See reference to Shafi's work in Stamp (1958).) Surveys were made in 12 villages, spread over areas of four different soil types, which showed the following average consumption of calories per head per day:

Well-drained loam with irrigation	2,141
Stiff black clay	2,018
Poorly drained clay	2,005
Poor sandy soil	1,853

Assuming an average body weight of 50 kg. for an adult male, the requirement for India, on FAO standards, is 1,944 calories (209 kg. grain equivalent).

FAO's predecessor, the International Institute of Agriculture, published all the information which could then be found on food consumption in the Far East (International Institute of Agriculture, 1938, 1939). For Indonesia they published a round figure estimate by Scheltema of 2,000 calories per day. Even assuming 50 kg. body weight, this is only just enough, at Indonesian temperatures; and some districts appear to be below it. No account is given, however, of the accuracy of the source. Some urban Chinese diets, not only of labourers and handicraftsmen, but also of teachers and "middle class", show a considerably lower calorie intake than Buck shows for the rural population.

A very interesting study (Swaminathan *et al.*, 1960) of a village near the Mysore coast, in a high rainfall climate, showed the differences which prevail within an Indian community (see Table 2).

For pre-Communist China, Buck uses the unit of "calories per adult male unit"; converted to calories per head of population (from data given by him) the results work out at 2,533 per day, a food supply well above minimum requirements. There are, however, strong regional differences; in the winter wheat/millet area calorie

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consumption was only 2,240 per person per day, which is probably inadequate for a cold climate, at any rate for large-bodied people. In this region 10 out of the 21 villages surveyed were in fact estimated to be undernourished during the year of investigation; in the rest of northern China (wheat region) 17 out of 46; in southern China (rice region) 12 out of 69. The marked differences between villages indicate, amongst other things, the lack of transport, and also the great variability of harvests. One can only hope that it is not the same villages which are hungry year by year.

TABLE 2

Food consumption of different castes in an Indian village

	Average income Rs. per person per month	*Food consumption calories per person per day*	*Average body weights of adults kg.*	
			Male	*Female*
Fishermen	6½	1,580	48	41
Harijans (low caste)	7½	1,940	46	40
Miscellaneous castes	10	1,960	48	41
Agricultural castes	8	2,440	49	42
Brahmins and Vaisyas	18	2,720	51	45

For Communist China, the best available information indicates that, except for the single year 1958, when the harvest was unusually good, food consumption per head has been substantially below the level of the 1930s.

Buck's own standard of 2,800 calories per day per adult male unit, i.e. 2,154 per head of population, corresponds to the entries in the above table for the mean of north and south China, if average body weight is assumed to be 55 kg. for an adult male.

Another study on average calorie intake throughout Asia, Africa and Latin America was recently completed by the United States Department of Agriculture (1960–61). In a number of cases they come out with figures significantly higher than those of FAO. In one calculation they find a large number of countries apparently below the minimum level of calorie requirements: but this was after allowing, not only for milling and storage losses of grain, but also for an assumed further 15 per cent. wastage in consumption. It does not seem reasonable to assume that really hungry people would waste so much food.

The task of estimating the proportion of the world's inhabitants who are inadequately fed was performed, about as thoroughly as the presently available data will allow, by Sukhatme (1961). His conclusions commanded general agreement from the critics, and showed that the proportion was probably somewhere between 10 and 15 per cent.

4. MEASURING AGRICULTURAL PRODUCTION

We now turn to our principal concern, namely measuring and suggesting methods of improving the agricultural production on which these food supplies are based. To impose some order on the extraordinary heterogeneity of the agricultural production

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figures, we make use of the device of "wheat equivalents", all other agricultural products being expressed as wheat equivalents by means of a price factor. These factors are obtained from Klayman's (1960) report for FAO, who need them for the purpose of preparing their own national and world index numbers of agricultural production, but use different weights for different regions of the world. In the studies which follow, the medians of their weights for Asia, Africa and Latin America are applied uniformly in all cases. Some further equivalents, based on these weights, have had to be computed for some dairy products and oils and oil seeds.

TABLE 3

Agricultural output in economic equivalents of wheat kg. per head of population

	1934–8	1951–2	1958
Pakistan	461	437	437
India	357	309	393
Ceylon	583	521	667
Burma	668	439	596
Thailand	408	472	665
Indo-China	541	370	..
Formosa	774	445	..
Philippines	514	452	831
Indonesia	434	364	455
Malaya	1,094	1,234†	962

.. Not available.
† De Vries entered rubber at its current price.

The device of grain equivalents was pioneered by Buck in his study of China. In this case, however, he regarded all grains as equally valuable; and instead of using an international scale of weights, as is used here, he fixed one or two other products, such as potatoes and groundnuts, on their supposed nutritive value, and took other weights from the prices which he found prevailing in local Chinese markets. This system gave to both meat and vegetables *lower* weights than are used here.

This system of measurement (taking milled rice as standard) was also developed in Indonesia by Dr de Vries (now Director of the Institute of Social Studies in the Hague), and used by him for measuring agricultural productivity throughout Asia.

Table 3 presents de Vries's estimates of total agricultural output per head of population (all population and not only agricultural population) converted into equivalents of kg. of wheat. He specifically included fruit and vegetables. The first two columns of Table 3 contain his data; the 1958 figures have been estimated from the reports of the United States Department of Agriculture on food balances in foreign countries.

The reader may be suspicious—with some justification—of these devices for expressing all agricultural production in terms of wheat or of rice. When historians tell us, for instance, that wages at some distant date would buy a greater quantity of wheat than do wages in some countries now, we hesitate to conclude immediately that the general living standards were higher, suspecting that the price of wheat relative to other commodities may have been quite different then.

This device appears, however, to be permissible when we are dealing with the production and consumption of poor agricultural communities, because inevitably,

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in their circumstances, grains form so large a proportion of all their agricultural production, that any probable differences in the weights which we allow for expressing other commodities in terms of wheat are not likely to make very much difference to our final results. The relative prices of other grains as against wheat appear to represent reasonably well their relative palatability, digestibility and general acceptability.

TABLE 4

Economic and calorie value of diets

Country	*Food consumption*†	*Calories*	
		Per head per day (actual)	*Per gramme of economic wheat equivalent consumed*
Tanganyika	328	2,175	2·42
Indonesia	399	2,125	1·95
Nigeria and British Cameroons	435	2,680	2·25
Cameroons	395	2,470	2·28
Algeria	586	2,230	1·39
Egypt	544	2,340	1·57
Bolivia	508	1,880	1·35
Honduras	542	2,190	1·48
Iran	600	2,040	1·25
Nicaragua	715	1,985	1·01
Costa Rica	751	2,555	1·24
Malaya	518	2,290	1·61
Cuba	797	2,870	1·32
South Africa	1,161	2,620	0·83

† Expressed in kg. economic wheat equivalent per person per year.

Source: U.S. Dept. of Agriculture (1960–61).

The whole method, of course, becomes progressively more inaccurate as we ascend the international income scale to communities where grain for human consumption forms only a small fraction of the whole value of agricultural production.

A scientific reader may ask why we do not convert all other products to wheat, or to some other standard, in terms of calorific value. This indeed can be done for certain purposes. It requires us to say, for example, that a ton of meat could only be counted as of approximately the same value as a ton of grain. This clearly does not make sense economically—the meat contains valuable proteins and people would certainly rather have meat than the grain. Furthermore, this calorie basis for comparison gives us no base for including in our totals other agricultural products which have little or no calorie value but which are valued for other purposes—coffee, cotton, jute, tobacco, etc.

It is desirable, however, to segregate the food from the non-food products, and to see how far our "economic units" (compiled on price weights relative to wheat) compare with calorie units. We should expect the discrepancy to become steadily wider as communities become richer, and include in their diet an increased proportion of meat, fruit, etc., whose calorific value is low relative to the economic price. This is

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exactly what we find, in a sample of the poorer countries at differing economic levels chosen by the device of ranging them in ascending order of calorie consumption per head (as computed in the United States Department of Agriculture study) and taking every fifth country.

If we consume wheat just as it stands, crushing it and cooking it to a porridge, we obtain 3·16 calories per gramme. (This includes the bran, which is barely digestible; if we mill off the outer 15 per cent. by weight of the grain—not the 30 per cent. milling which is required to make white flour—we are left with a product giving 3·46 calories per gramme.) We see clearly from Table 4 that as countries ascend the income scale, the calories obtained per gramme of wheat equivalent consumed fall steadily. There are a few unexpected jumps in the series. The low figures for Bolivia and Nicaragua indicate a high proportion of meat in the diet—though the total calorie intake appears inadequate. The comparatively high figures for the Cameroons and Nigeria are related to a high proportion of sweet potatoes, yams and cassava in the diet, indicating a possible protein shortage. In Tanganyika, the poorest country, the effect of a certain amount of meat consumption in the pastoral regions is offset by millet consumption elsewhere. Millet is given a low price weight relative to wheat; but its calorie content per gramme is actually higher.

Buck gave detailed data for consumption in 136 separate Chinese villages in the early 1930s. These were likewise ranged in ascending order of calorie consumption, and each tenth village selected for analysis on the same lines, all food consumed being expressed in wheat equivalents by the same international scale as used above (not on Buck's original scale, based on Chinese prices, etc.).

In this case, however, the results did not indicate a declining ratio as the nutrition of a village improved (the range covered was from 1,000 to 4,000 calories per person per day). The distinction was rather regional. The average for all the villages surveyed was 2·67 calories/gramme wheat equivalent, which was about what was to be expected in relation to the other figures given in Table 4. But for the rice-eating villages the average was only 2·20. There were, however, several villages whose diet consisted predominantly of millet, sorghum and other coarse grains, which appeared to be satisfactory in that it did not include excessively high proportions of root crops, or a low proportion of vegetables. In these villages the calories obtained per gramme of wheat equivalent consumed were about equivalent to the theoretical figure of 3·16—the calories lost in the consumption of meat, vegetables, etc. were fully made up by the greater cheapness of millet in relation to wheat.

These coarse grains, particularly millet, are dry and difficult to eat. Africans mix leafy vegetables with them to make them more palatable. In many parts of Asia it is regarded as degradation to have to consume millet instead of rice. Nevertheless, if we set out to state the physiological minimum requirements of mankind, we must assume that people threatened with an actual shortage of food will be willing to eat millet, etc., and in this way obtain 3·16 calories per gramme wheat equivalents consumed. This means that each calorie consumed per day calls for 0·01157 kg. of wheat equivalent to be consumed per year.

Turning back to our table of calorie requirements, it appears then that we can express minimum requirements in terms of kg. of wheat equivalent per head per year, as 225 for small-bodied people in a hot climate, rising to 268 for fairly large-bodied people (adult males 60 kg.) in a cold climate.

However, we have so far taken no account of the consumption of textile fibres which (for peoples who cannot afford to buy nylon) must also be an agricultural

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product. FAO studies of world fibre consumption show, for the poorest countries, including China, an average of about 1·5 kg. per head per year, the economic equivalent of 14 kg. of grain. Including a minimum requirement of textile fibres, therefore, we can state the minimum agricultural requirements of mankind as ranging from 240 to 280 kg. of wheat equivalent per person per year, according to varying climates and body weights: in round figures, a quarter of a ton.

In the advanced communities, on the other hand, we consume about ten times as much, or 2½ tons of wheat equivalent per head per year. (The amount actually consumed in the form of cereals is, of course, very small: it is the high proportion of meat, dairy products, etc., in our diet which raises the wheat equivalent.) This appears to be about the maximum beyond which consumption cannot go—you cannot consume *less* than ¼ ton wheat equivalent per head, or *more* than 2½ tons, without endangering health—judging by the experience of the United States, where continually rising real income is now accompanied by approximately stationary consumption per head of agricultural produce (Table 5). (The replacement of fibres of agricultural origin by synthetic fibres has played some part here, too.)

TABLE 5

Food consumption per head in U.S.A.
(*U.S. Dept. of Agriculture*)

	Gross income per head $ of 1954 purchasing power	*All agricultural products expressed as tons of wheat equivalent per head*			
		Production	*Exports*	*Imports*	*Consumption*
1909–14	1,169	2·55	0·38	0·32	2·48
1920–24	1,219	2·63	0·44	0·47	2·41
1925–29	1,440	2·39	0·35	0·45	2·49
1930–34	1,142	2·25	0·24	0·37	2·38
1935–39	1,360	2·31	0·20	0·41	2·52
1952–54	2,265	2·77	0·24	0·33	2·86
1955–58	2,364	2·79	0·34	0·31	2·77

5. AVAILABILITY OF CULTIVABLE LAND

Now that we have some ideas of requirements, we consider the capacity of different systems of food production to supply them. We must, however, shun the *simpliste* idea of trying to measure the area required to produce each person's food supply. This area varies enormously, according to changing agricultural techniques. The evidence shows that people often live for a very long time with a less advanced technique, and only advance to a more productive one when compelled to do so by increasing pressure of population.

The simplest system of food production is to live exclusively on game and fish, as did our ancestors for immemorial aeons of time, and as some communities still living in remote parts of the world do to this day. The primitive hunter no doubt enjoyed his protein but he needed his calories, and the calorific value per gramme of meat is only about the same as that of grain. So a primitive hunting community still has to catch and eat about a quarter of a ton of game per head per year (more if they eat fish, which has higher water and bone content).

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There have been some interesting discussions among anthropologists and archaeologists about the average space per person required in primitive hunting communities. It may take 8,000–50,000 hectares of the Arctic to support one Eskimo (Clark [n.d.]), but as little as 150 hectares to support each person in a primitive community in regions where the fishing is good (Childe, 1954). For prairie grasslands the estimate is 2,000–2,500. Mr Pirie's figures show that such an area should carry nearly 150 tons (liveweight) of game, reproducing itself, on the average, every 3 years for large game, in a much shorter period for small game. Does the primitive hunter really require all that land to produce his quarter ton, or little more, of meat per person per year?

It appears that he does. Most of the game, in fact, is eaten by wild predatory animals; man cannot hope to extirpate them or even keep them in check, until his own numbers have substantially increased. When he ceases to be a hunter and becomes a pastoralist, the practical yield of the land, as opposed to its theoretical carrying capacity for game, is much increased—though simple pastoral peoples too also lose a lot of livestock to predators—witness the Old Testament, for example.

Skilful though primitive hunters have been in devising bows and arrows, boomerangs, etc., and in tracking animals, they often have to work hard actually to kill them. Finally, there is the important point that when they do catch a large animal they have no means of preserving it, and the larger part of the meat is bound to be wasted. Holmberg (1950) describes the Siriono tribe hunting game with bows and arrows in the jungles of eastern Bolivia, faced with this problem. It is not uncommon for four people to eat a peccary of 60 lb. at a single sitting. When meat is plentiful, a man may consume as much as 30 lb. within 24 hours. Nevertheless, added over three months, their average consumption of meat worked out at only 0·2 kg. per day, far from sufficient for their requirements. This they supplemented with jungle products collected wild; in addition, a few tubers and seeds were planted during the semi-sedentary rainy season when the waters of the numerous lakes and rivers are still too high to allow extensive migration and meat supplies are reduced, though their plots were infinitesimal in size as they are frequently not at hand to reap the harvest. But this practice may be regarded as contributing the link between the economy of food gathering and hunting peoples, and that most primitive of agricultural systems, shifting cultivation. Even so, Holmberg takes pains to emphasize that, though the supply of food from hunting and gathering is rarely abundant and always insecure, starvation does not occur.

Let us now construct an imaginary picture of our ancestors, just as they were passing from the mesolithic to the neolithic era. The population of England had probably reached 15,000 (Nongier, 1954), and they were complaining about over-population. Everyone knew that you could not support your population on less than 10 sq. km. per head. Eventually, as we know, they had to take up—probably with immense reluctance—the agricultural life, which had been pioneered one or two millennia earlier in the Middle East.

About 500 years ago, the population of what is now the U.S.A. and Canada was approaching one million. They also were finding population pressure too great for a hunting people, and had to adopt agriculture, which had been pioneered by the Aztecs in Mexico. By the late seventeenth century, the Red Indians of the east coast of the United States were obtaining some two-thirds of their calories from cultivated maize (Bennett, 1955). There is a moral to these tales for all the people who complain about the inability of their land to support population now.

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Primitive pastoral peoples still occupy some 50 hectares per head (reference to Rätzely in Taylor (1950)). Even with some agriculture to support their grazing, they still fall far short of being able to exploit the grazing potentialities of the land which Mr Pirie puts at some 50 kg. liveweight gain per hectare per year. Meat yields of about this order of magnitude are obtained on the poorer pastures in this country now (Davies, 1960) or, for that matter, some of the dry and underdeveloped pastures of Africa and Australia; on the poorest rough grazing in this country the yield falls as low as 25; but on the best modern pastures, fertilized and watered, the figure can be raised to 1,000.

TABLE 6

Potential agricultural area of the world expressed in terms of equivalents of standard farm land per m. hectares

	Total	*Of which:*	
		Standard farm land equivalents of two crop tropical areas	*Standard farm land equivalents of cold climate areas*
U.S.A. and Canada	1,006	4	367
Central and South America	1,835	736	29
Europe (excluding U.S.S.R.)	403	—	47
U.S.S.R.	1,109	—	539
Africa	1,555	732	—
China	409	—	88
India and Pakistan	305	43	13
Rest of Asia	791	464	7
Oceania	268	60	—
WORLD	7,680	2,039	1,090

At a rate of consumption of 2½ tons of wheat equivalent per person per year, it requires 0·97 hectares (2·4 acres) to supply one person in this country: but only 0·61 in Denmark and 0·39 in the Netherlands (the latter country, the most densely populated in Europe, is a net exporter of agricultural produce). The Japanese live healthily on 0·56 tons wheat equivalent per person per year which they produce on 0·07 hectares (one-sixth acre). To produce the same amount of food an Indian used 3½ times as much land: in other words, Indian output could be increased 3½-fold if India could attain Japanese standards in the use of fertilizers, plant breeding, and rural education.

We can make an approximate tabulation (Table 6) of the amount of land in the world available for cultivation or grazing (as we have seen, grazing lands really well cared for can produce one ton liveweight per hectare per year of meat, which is as economically productive as any cropland). The tabulation is on a climatic basis. There is great variability in the quality of soils, and cultivators naturally use the best soils first; but by suitable use of fertilizer, any soil can be improved eventually. Steep slopes cannot be cultivated; but experience in New Zealand shows that, if the climate is right, they can yield very productive grazing.

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All land capable of agricultural use is converted by coefficients on various climatic criteria (Clark, 1957) to the equivalent of "standard farm land", i.e. the equivalent of good temperate farm land. The persistently hot and wet tropics are treated as being capable of giving two crops a year (as they can, if fertilized). Varying discounts are made for lack of adequacy of rainfall (Cambridge is entered as "slightly sub-humid" in this connection), down to the semi-arid grazing lands of Africa or Australia, which are counted as only one-thirtieth of a unit. On the other hand, large areas of cold climate country, hitherto regarded by geographers as unsuitable for agriculture, have been shown by recent experience to be cultivable (I have seen a commercially successful dairy farm 200 miles on the wrong side of the Arctic Circle in Finland). This land is entered in the total at discounts of from 10 to 50 per cent. No estimate has been made for any irrigation of arid land, or for the exploitation of the sea; and the climatic judgements have been cautious (e.g. many regions in North Africa have been rated as uncultivable, even though some crops are in fact grown there).

The best experimental techniques in use in agricultural laboratories now yield 38 grammes dry weight per sq. metre per day. With practical application, which will probably not long be delayed, a man's food requirements throughout his lifetime could be obtained by the continuous cultivation, on the best possible methods, of only 20 sq. m. These considerations may be of some importance to our descendants, floating about the universe in space ships—a problem which is already receiving Mr Pirie's attention.

6. Productivity of Labour

Now we consider the productivity of land in relation to the other principal factor of production, namely labour. Now that our agriculture has entered the tractor age, we tend to think of that as having been preceded by a "horse age" of immemorial antiquity. In fact, the horse cultivation of the 19th and early 20th centuries represented an important technical advance on what had gone before. In many parts of Europe ox ploughs are still to be found; they were predominant in this country until the late 18th century, and survived into the 19th century in Scotland. It is true that the technique of ploughing with oxen was much the same for an 18th-century peasant in England, as for an Asian at the present day, as described by Virgil in the *Georgics*, or even as used by Homer's contemporaries. But even the ox plough represents an important technical advance on the most basic form of agriculture, namely cultivating the soil by hand hoes. This primitive and laborious form of agriculture is still the practice of the majority of African cultivators, of a substantial number in Asia, and of many in Latin America.

There is an economic equilibrium even between the ox plough and the hand hoe. De Vries (1955) stated that, under current conditions in Indonesia, the cost of keeping a buffalo only doubtfully balanced the additional output obtained through his labours; and that there were, in fact, signs of the economy slipping back to hand cultivation. Two buffaloes with a plough will cultivate three times as fast as hand cultivation on a large plot, but only $1\frac{1}{2}$ times as fast on a small plot (Van der Koppel). In those regions with a very short wet season (discussed below), however, it may be necessary for the cultivator to use draught animals, whatever the cost, in order to get his cultivation done in time.

A similar regression appears to have been taking place in the community in which my grandfather lived, namely the Scottish Highlands, in the early 19th century (Gray, 1957). Increasing population density, higher rents, and lower prices for their produce

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meant that crofters in some of the poorer districts were unable to afford even ox ploughs, and reverted to hand cultivation. In the months January to April it was estimated that a man could dig 5 acres.

TABLE 7

Number of persons whose needs of agricultural products at maximum consumption standards (2·5 tons of economic wheat equivalent per year) are provided for by working in agriculture

Country	*Pre-war*	1948/9 *to* 1950/1	1952/3 *to* 1954/5	1957/8 *to* 1959/60
New Zealand	25	34	37	46
Australia	18	22	25	30
Canada	9	13	18	23
U.S.A.	9	16	20	24
Denmark	6	8	9	14
United Kingdom	8	11	12	13

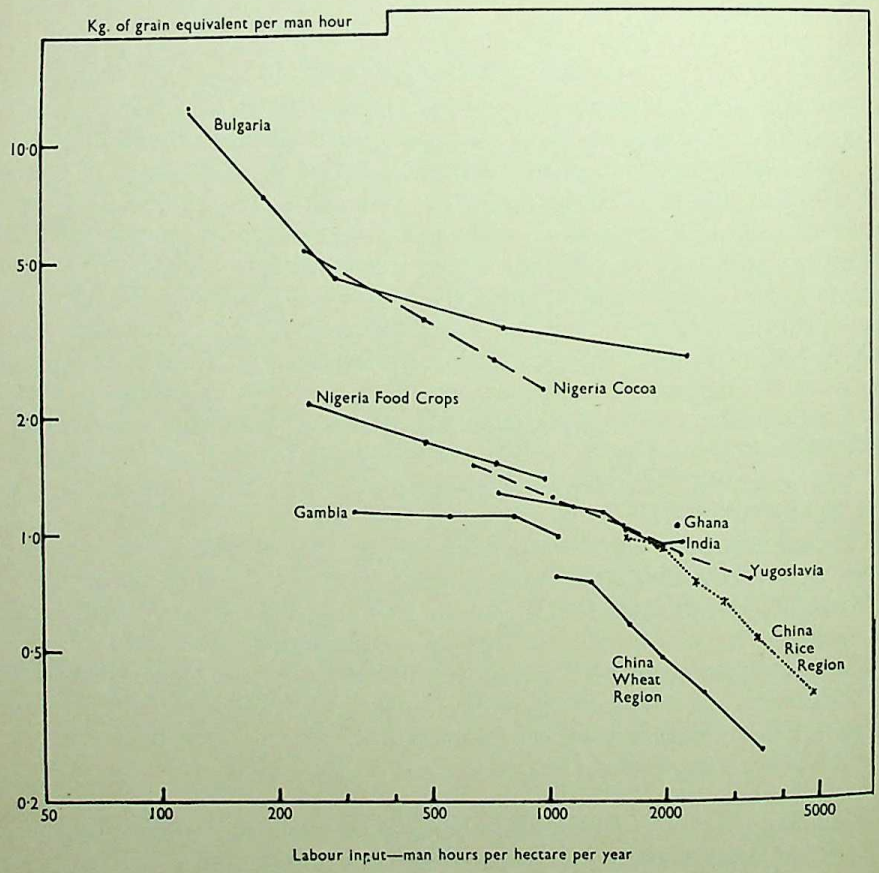

FIG. 1. Average returns of agriculture.

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The question is sometimes raised whether we could not do a good service to these simple African cultivators by redesigning their tools for them. But they have advanced a long way from the simple wooden digging sticks which were used by their remote ancestors (and by ours; and which can still be seen in some of the most isolated regions

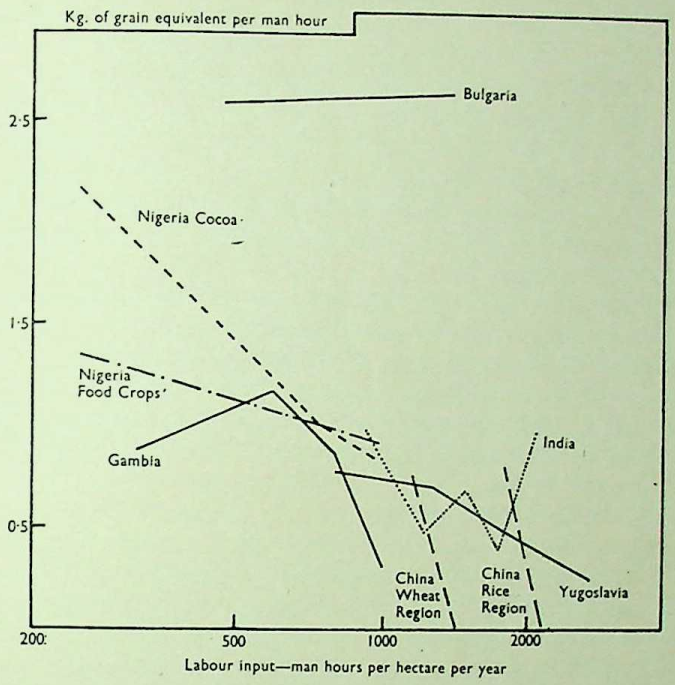

FIG. 2. Marginal returns of agriculture.

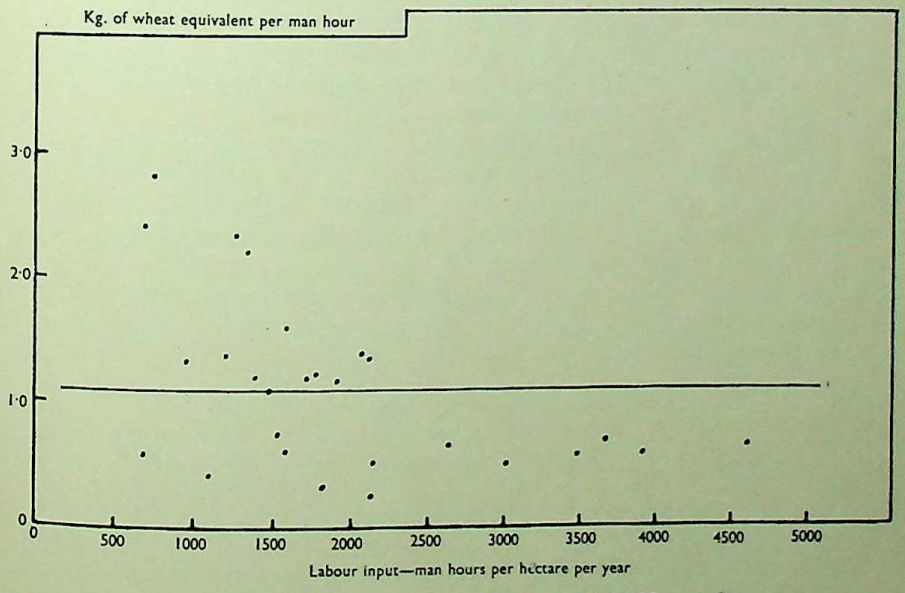

FIG. 3. Average returns : Jamaica 1954-55.

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of New Guinea). Some Africans had discovered how to work iron before their contacts with Europe began in the 15th century. Iron has always been an expensive commodity for primitive peoples, and they use it as sparingly as they can. Nevertheless, they have discovered how to make large-bladed hoes, which can move as much earth as the large iron spade of the European gardener. Separate hoes have been designed for men and for women, carefully adapted to their strength and stature. Those familiar with the problem believe that African empiricism has already discovered as much as any European designer could when faced with this problem.

To set the scale for the productivity of labour, we may first examine how much labour is required to provide us in the advanced countries with our "2½ ton ration" (Table 7). The figures are high, and are rising rapidly. It is clear that very few farmers or farm workers are going to be required in the future. In this process of improvement, of course, each farm worker is being assisted by ever-increasing quantities of equipment, fertilizers, insecticides, etc.

Information from field studies is gradually accumulating to show the productivity of labour in primitive agriculture, measured, as it has to be, in the "natural units" of kg. of grain equivalent per man hour of labour. We can measure both average productivity and marginal, that is to say the product which a primitive agriculturist can expect from doing an additional hour's work, which is generally very low when labour input in man hours per hectare per year is above 1250. This may be called the critical point of overcrowding.

The workings are too lengthy to be shown in full, but the results can be clearly presented in diagram form (Figs. 1–3). There is other confirmation for this estimate of 1,250 man hours per hectare. Japanese official statistics show labour input in the different provinces ranging from 1,400 to 2,500 man hours per hectare, which is clearly too high—there is no significant difference in the yields of rice obtained.

Maruta (1956, 1958) after a careful mathematical analysis of the accounts of 30 farms using no mechanical power in one of the poorest provinces of Japan, with an average labour input of 2,700 man hours per hectare, found a marginal return per man hour of labour of only 0·064 kg. milled rice (0·075 wheat equivalent). Tutiya (1955) also obtains almost insignificant figures. Cotton growing in Soviet Uzbekistan had labour inputs varying from 233 to 348 man days per hectare per year, with no significant difference in yields, i.e. zero marginal return.

A lower figure for India (Indian Ministry of Agriculture, 1954–55) showed, at Ahmednagar, a marginal return of only 3·4 kg. sorghum (2·4 kg. wheat equivalent) per man day. Under much more spacious conditions in Thailand, however, labour inputs were only in the range 20–80 man hours per hectare per year and marginal product was 2·8 kg. rough rice per man hour (2·2 wheat equivalent).

A most interesting new approach to the problem of marginal productivity in rice growing was Sarkar's (1957) study for Ceylon. Comparing different districts in Ceylon, he obtained the equation:

$$\log(\text{yield of rough rice in bushels}) = 0{\cdot}0802 \log(\text{area in acres}) + 0{\cdot}5570 \log(\text{labour input in man days of 8 hours, women counting half men}) + 0{\cdot}2453 \log(\text{all other costs, measured in Rs.}) - 0{\cdot}0141.$$

(Bushel rough rice = 20·5 kg.; wheat equivalent of rough rice = 0·8.)

To calibrate his equation he then assumes (which does not appear to be quite the case) that the marginal product can be equated to the subsistence wage paid in the

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agricultural districts, which at that time was 1·9 Rs. per day (equivalent to 3·9 kg. of rough rice). Taking 268 days as a normal year, he then deduces that 28 per cent. of the agricultural population is surplus. An interesting feature of this result is that the extent of surplus labour appears to be almost unrelated to the size of holding.

Taking Sarkar's equation, for a given area of land, and with miscellaneous expenditure also being given, we can re-write it:

$$\log(\text{number of labour days}) = \text{constant} - \log(\text{marginal productivity of labour measured in bushels per year})/0{\cdot}443.$$

Thus, if we raise the number of labour days by the factor of 1/0·72 (i.e. raising it from Sarkar's computed to its actual level) the left-hand side of the equation is raised by 0·145, so marginal productivity of labour is lowered by the antilog of 0·064, i.e. 16 per cent. In this way we deduce the true marginal productivity of labour at 0·41 kg. rough rice per man hour, as against the wage of 0·49.

While there are many unfortunates in Asia, whose marginal productivity is low or zero, so that they can hardly produce anything more on their present land, however hard they try, nevertheless the average and marginal return to hand hoe labour in many regions, including Africa, appears to be high enough for the adults (women do as much agricultural work as the men) to produce the subsistence requirements for themselves and their children by working only a few hundred hours per year, certainly much less than the 2,000 hours per year which we now regard as customary, or the 3,000 or more which the Victorians used to work. In some of the remoter parts of Africa we hear of men devoting less than 1,000 hours a year to agricultural work. Baldwin (1957) sampled two cocoa farming villages in north-west Nigeria and found that the average numbers of working hours per adult working male per year were 997 and 1,327 respectively. These represented 660 and 800 per hectare, a level of labour input which for marginal productivity is still high.

Among the primitive cultivators in Guatemala, Tax (1953) found the average man working only 87 days a year, i.e. about 950 man hours per hectare per year on an average plot of 0·74 hectares, to produce 2 tons of maize (2·1 kg./man hour). In Jamaica, on the other hand, small farmers are found to work a median of 2,500 hours, even though they appear to have zero marginal productivity (Edwards, 1961). Haswell (1953) found in Gambia in 1949 that adult men and women engaged in agriculture averaged only 855 hours per year (133 days) in agricultural work.

One reason why African farmers do not produce more is that while they would like to buy more industrial goods, and would be willing to produce more crops in exchange for them, they are hampered by the lack of transport, and inadequate marketing organization. This is true of crops even with high value per unit of weight such as cocoa and coffee; and there is a limit to the amount of these crops which the farmer can grow. Good roads are Africa's real priority need: without them crops have lost a large part of their value by the time they are carried to market.

We must not, however, ignore the fact that there is some preference for leisure. Economists have been grumbling about this, and finding it difficult to understand, ever since our illustrious predecessor, Sir William Petty. Petty wrote in 1691, regarding the Irish, "What need have they to work, who can content themselves with *Potatoes*, whereof the Labour of one Man can feed forty?" Petty's figure was approximately correct, and was confirmed by Arthur Young, regarding the ability of one man to produce 50 tons of potatoes per year (to feed 40 people at 8 lb. per day), but he forgets

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that substantial quantities of milk and oatmeal were also included in the Irish diet at that time.

A still more striking result has been obtained by N. A. Mujumdar (see Fig. 4). It is known that most Indian farm families do not have enough land to keep them fully occupied. Mujumdar had the original idea of visiting a large number of these families (in the Kanarese-speaking region of Bombay State) and asking how much land they would take if they were free to buy all that they wanted. He then translated this, plus the land already owned, into terms of man hours per working member of the family, and got the surprising result shown in the diagram.

In Bengal, where the soil and rainfall are very good, some of the land is double cropped, and the labour input per hectare very high. Nevertheless Ghosh (1950) found there, from a detailed study of land registers, that when the land held by any

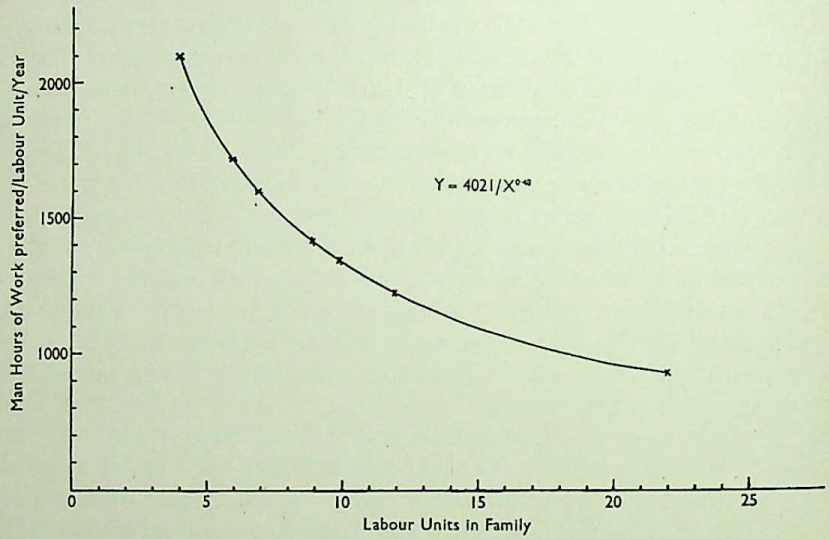

FIG. 4. Preferred amount of agricultural work by Indian farm families as shown by amounts of land they desire to possess.

family exceeded 3 hectares, they tended to let some of it to the neighbours. Unfortunately we do not have data about the size of family, or the average labour input per hectare; but however low we assume the former, and however high we assume the latter, we are bound to obtain the result about which there can be no doubts, namely that the wealthier families have a short working year.

An Indian or an African might be justifiably annoyed at our use of the phrase "preference for leisure". With us, a farmer or farm labourer performs his agricultural work, then has the remainder of his time for rest and recreation. An African or an Indian peasant however, besides his agricultural duties, has to find some time during the year for repairing or rebuilding his house, making his shoes and furniture, and also performing a great many religious and civil duties.

We should have in our minds these preferences (whether for good reasons or for bad) for not devoting too large a proportion of one's working year to agricultural

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duties, when we consider the problem of "disguised unemployment", i.e. the members of farm families who are idle for a substantial part of the year. But we must not go to the other extreme, and say that peasants and rural labourers are getting all the work that they would like to have. We must also remember that it is the poorest families who have the least "leisure preference" and who often, particularly in southern India, suffer very seriously from disguised unemployment. Mujumdar found the preferred amount of agricultural work per year ranged approximately from 1,500 to 2,500 hours. Srinivasan (1957) in a study near Coimbatore in Southern India found that average employment per year ranged from 1,100 to 1,700 hours for different villages, with an average of 1,400. Qayum (1957) made an estimate for the whole of India, in which he assumed that a third of the time was devoted to subsidiary occupations, that men worked 300 days per year, women 120, and boys and girls 150. Even on these assumptions he found that agricultural work only occupied 65 per cent. of the time available to perform it.

Some economists have thought out ingenious schemes for tapping this reservoir of unused labour. But it does not represent "labour for nothing". To use round figures suggested by Professor Raj, an unoccupied Indian countryman may be supported by his family at a cost of $\frac{3}{4}$ Rs. a day. If you ask him to work, even in his own village, he will not do so unless he gets $1\frac{1}{4}$ Rs. or more per day; if he is asked to work away from his own village he will ask 2 Rs. or more. The marginal cost of such labour to an employer is high.

Moreover, any plans for the permanent diversion for this rural labour surplus come up against the fact that it is not available throughout the year (Table 8). It is true that, the better distributed the rainfall, the better distributed also is the demand for labour; but even where rainfall is uniform, there are still peak demands. In some parts of India, where the rainy season is very short, this peaks the demand for labour in a most alarming manner. Instead of being in embarrassing surplus, labour suddenly becomes the critical factor in production. As Mr Pirie has pointed out, if any able-bodied man or woman is incapacitated by illness during this period, the loss of production may be very serious (though the critical period is rarely as short as the 5 weeks which he suggests). Experience in some European countries such as Italy and Greece shows that the cultivation of fruit and the keeping of livestock spread the demand for labour better over the year. These countries too, however, have a problem of labour shortage at the peak period.

The Chinese government has recently discovered these facts, to their cost. Paradoxical though it may seem, they have created a labour shortage in China. The distribution of work throughout the year of Chinese farms was carefully investigated by Buck (see Table 8) who showed that the average amount of time lost was only 15 per cent. (1·8 months) on average; and this is practically confined to the winter months.

But Mao had other sources of information. "Under present conditions of production", he wrote in his book, *Socialist Upsurge*, "there would be a one-third manpower surplus in rural areas." Possibly he was basing his judgement on India, where only two-thirds of the available labour is occupied. If we exclude the districts dependent upon monsoon rain, and confine ourselves to the regular rainfall districts in India, which are climatically comparable to China, it may even be true to say that nearly one-third of the labour force there is surplus, which could be diverted to other employment at any time of the year, without seriously affecting agricultural production. But, in comparison with China, this might be called "technological unemployment":

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India is more "technically advanced" than China, using ox ploughs which require less labour.

Never has a mistake in information on agricultural economics had more serious consequences. It was the belief in the existence of this "one-third surplus" of labour among the rural population which led to the massive diversion of rural labour to all sorts of other projects, many of them quite futile, such as the rural blast furnaces which had to have most of their materials carried by hand, and ended up by yielding an unusable product, during the "great leap forward" which began in 1958. It was this artificially created labour shortage which has been primarily responsible for the food shortages of subsequent years, not floods and droughts.

TABLE 8

Utilization of labour in agriculture as per cent. of available labour supply (figures in excess of 100 *indicate temporary agricultural work done by the non-agricultural workers of the village)*

Month	*China*	*India*		*Ghana*	*Iraq*		*Greece*
		Madhya Pradesh	*Maha-rashtra*	*Cocoa farming*	*Middle Tigris*	*Best area (Diyala)*	
January	47	29	65	57	22	24	50
February	78	29	62	18	23	27	50
March	94	41	66	30	23	26	48
April	97	41	32	28	35	39	59
May	98	52	34	30	70	92	66
June	96	33	42	30	72	94	79
July	94	127	55	47	56	76	77
August	96	127	60	57	60	76	77
September	97	93	47	75	52	43	81
October	92	96	52	100	53	45	69
November	82	82	100	90	54	52	58
December	57	82	94	65	25	35	71

It is not that Mao did not have warning. Ma Yin-Chu, President of the University of Pekin, one of the few independent thinkers in China (he believes that population growth should be restricted), pointed out that there were critical labour shortages at a vital period of the year described by the Chinese proverb "in the morning yellow, in the evening green" (when peasants are harvesting the first rice crop, but having to transplant the seedlings for the second rice crop in the cool of the evening of the same day). Even in 1956, the government had ordered excessive planting in Chekiang Province beyond the capacity of the labour available, with the result that transplantation of the second crop was not performed over a wide area, and over a larger area was left very late, with consequent very low crop yields.

It is these considerations which have led to the paradoxical proposal that some of these Asian countries, although suffering from a rural labour surplus for a great part of the year, would nevertheless find it worth while to employ mechanical cultivation, in order to overcome the limits imposed on production by labour shortage in these peak periods. Wijewardene (1961) suggested that small powered tillers (whose capital cost was no more than that of $1\frac{1}{2}$ pairs of bullocks) would be worth while on

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Asian farms between 2 and 15 hectares in size, where the amount which a farmer can do is limited by the tractive power of his bullocks, and of those he can hire. The figure of 2 hectares as the lower limit is too low; even allowing for 6 or 8 cultivations, such as rice and sugar cane require, a pair of bullocks could easily cover this area in 60 days. Throughout the year also they render valuable service in transport, which power tillers could not give. It is only upon considerably larger farms that tillers are worth while. Even for China, however, Ma Yin-Chu advocated the use of machinery—"The future key to rich increases to rural production", he wrote, "is in mechanical assistance during the excessively busy period."

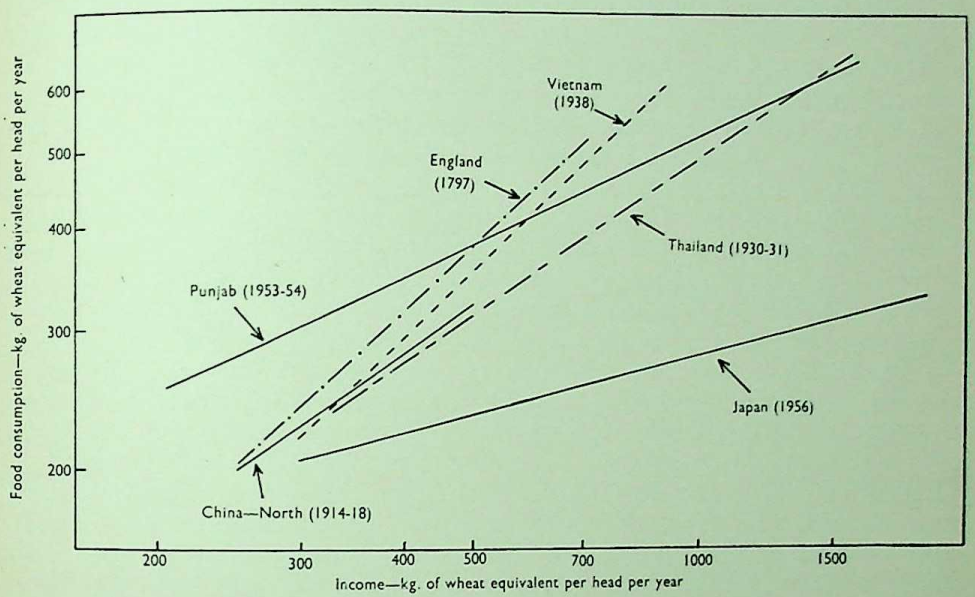

FIG. 5. The income elasticity of demand for food.

The physiologically necessary minimum consumption of agricultural produce, it will be remembered, stands at about ¼ ton of wheat equivalent per person per year. Consumption, of course, rises with increasing income. In the wealthier communities, food consumption per head appears to rise as a semi-logarithmic function of income (Basu and Jones, 1957). In the poorer countries, however, as will be seen, a log-log relationship is found, i.e. constant income elasticities of demand over a considerable range. The value of the income elasticity coefficient is measured by the slope of such a diagram.

The consumption data obtained from field studies in low-income countries are too abundant to tabulate, and are shown in Fig. 5, both income and food consumption being expressed in the standard units of kg. wheat equivalent per person per year.

These data strikingly confirm a bold generalization originally made by de Vries. As production rises from the subsistence minimum to some 300 units, most of the increased product (not unexpectedly) is used to improve the diet. By the time 400 units have been reached, however, the community is trading a substantial part of its agricultural product—this indicates, in effect, the urgency of other needs besides food, for clothing, building materials, medicine and the like. Not until 500 units have been reached is it worth while employing animal rather than human labour. Up to 750

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units, draught and meat animals alike have to live on straw, bran and other by-products, together with a little grazing—and they do not thrive on it. Possibly there is a little grain for the work animals at the busiest season of the year when they most need encouragement. It is only beyond the productivity figure of 750 units that there is enough grain to spare for regular feeding to livestock, particularly to pigs and poultry, who cannot digest cellulose as cattle do, but require concentrated food in the form of grain or roots, as do we.

It is true that there is an important exception which proves the rule. Japan, although a technically advanced country in other directions, is still largely using hand cultivation. India, at a lower level of productivity, uses ox ploughs and apparently has been using them for a very long time. Maybe they descend from an age when productivity was higher, and cattle were easier to feed. (Without them the cultivation of the lands in Central India, with their short wet season, would probably not have been possible.) But in any case, as the slaughter of cattle is forbidden in India, farmers prefer to use at any rate the stronger cattle as draught animals.

It is true that over large areas of Africa the use of both cattle and horses has been prevented by the tsetse fly. This destructive pest is now being extirpated over substantial areas. It does not, however, follow that draught animals will soon be introduced into these areas—the level of productivity is still too low in many cases.

It will be seen from Fig. 5 that there are considerable differences in the slopes. The highest slope, other than for England in 1795 and for Vietnam in 1938, is about 0·77. The line for Japan is striking; it follows a very low slope throughout its length, indicating an income elasticity of 0·25—the wealthier Japanese peasants are content with the same food that they used to eat when they were poorer. Though at a higher level, the same considerations seem also to apply to urban Japanese.

It may be perhaps that in the Indian towns, in Thailand and in 18th-century England there is a so-called "demonstration effect", of abundant spending on food by the wealthier families, which leads the poorer families to spend a substantial proportion of any income increase, if they obtain it, also on food.

A number of figures which have been published for India, based on family budget data, imply even higher income elasticities, of 0·9 or more. Most of these results are vitiated by a rather subtle fallacy. Most of these budget studies state income and expenditure per family. But an Indian who is able to increase his income somewhat above minimum level finds—as do Hollywood stars in our community—that a number of his more distant relatives offer to come and share his home with him. This increasing number of persons in wealthier families is bound to distort the results. Not many data are available on a per-consumption-unit basis. N. A. Khan, of Lucknow University, has been able to do this for a number of civil servants' budgets in different cities, and also for steel workers at Jamshedupur; and in each case found an average income elasticity of 0·7.

We may now follow up these comparisons between families within a country with comparisons between countries. As agricultural productivity increases, this has two effects; first, everyone (agriculturists and non-agriculturists) has more to eat; secondly, an increasing proportion of the labour force can be diverted to non-agricultural activities. At first sight, one might indeed expect the whole of the increase to be used up in these two ways. This, however, ignores the country's needs for imports, which may be urgent. To obtain them, a country in the early stages of economic development must export food, except in so far as it can produce non-food agricultural products (coffee, tobacco, etc.); or oil, tin or other mineral products (in Japan's case also we

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TABLE 9

The economy of low-income agricultural countries (all data not otherwise specified expressed in kg. economic wheat equivalent per person per year)

Country	*Food product*	*Non-food agricultural product*	*Non-agricultural exports (mining)*	*Total product from agriculture and mining*	*Agriculture and mining per cent. of population*	*Product per person in agriculture and mining*	*Food consumption*	*Net exports incl. non-agricultural exports*
Tanganyika	328	92	—	420	80	525	328	92
Indonesia	395	60	55	510	71	718	399	115
Nigeria and British Cameroons	476	31	9	516	74	697	435	81
Cameroons	385	208	5	598	80	747	395	203
Algeria	497	11	91	599	75	799	586	9
Egypt	475	117	43	635	64	992	544	91
Bolivia	413	neg.	270	683	78	876	508	175
Honduras	609	98	6	713	84	849	542	171
Iran	592	30	239	861	75	1,148	600	261
Nicaragua	706	313	9	1,028	77	1,335	715	313
Costa Rica	832	325	29	1,186	63	1,882	751	435
Malaya	421	541	337	1,299	61	2,129	518	781
Cuba	1,530	96	64	1,690	47	3,596	797	893
South Africa	1,292	33	578	1,903	42	4,531	1,161	742

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TABLE 10

The economic development of Japan

	Food production (incl. fish)	*Non-food agricultural production*	*Non-agricultural exports (manufactures less raw material content)*	*Total available for food consumption or for purchase of imports (sum of 3 previous columns)*	*Per cent. of population in agriculture*	*Food consumption kg. of wheat equivalent per year per head of total population*
			Kg. wheat equivalent per year per head of agricultural population			
1885–89	469	45	4	518	67	310
1895–99	630	58	22	710	59	361
1905–09	785	71	54	910	52	387
1910–14	971	85	73	1,129	48	442
1920–24	1,380	127	210	1,717	40	531
1925–29	1,537	180	263	1,980	38	556
1934–38	1,759	212	522	2,493	32	529

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include manufactures for export). These other products thus can "substitute" for food in buying imports, and we therefore add their production to food production for the purpose of preparing Tables 9 and 10 (see Figs. 6–8). The remarkable abstemiousness

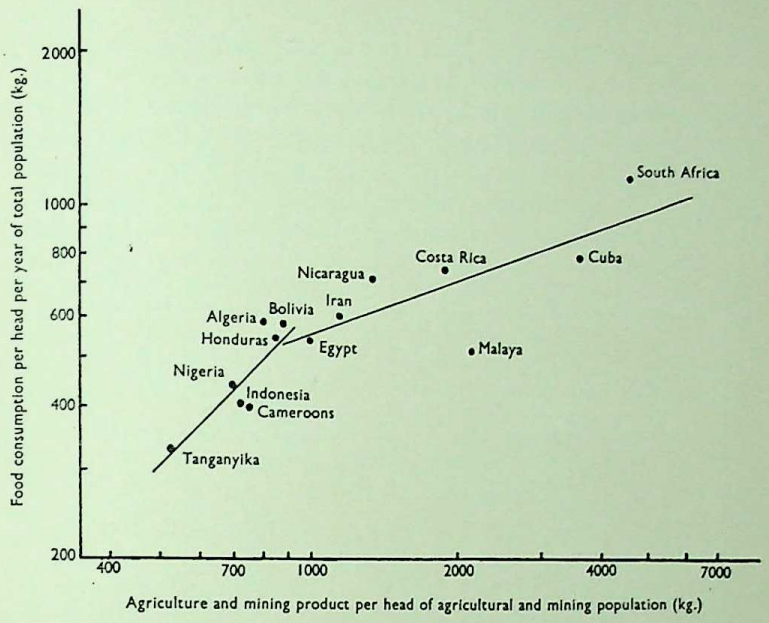

FIG. 6. Real income elasticity of demand for food in low-income countries (in wheat equivalent).

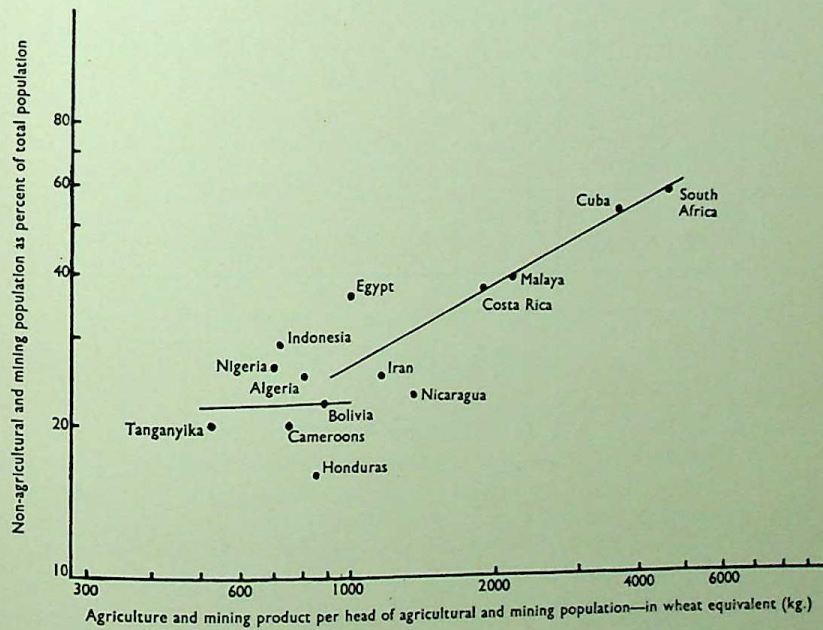

FIG. 7. Demand of non-agricultural products and services in low-income countries.

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of the Japanese appears as much in the international as in the inter-family comparisons: it has probably been of great value to them in accelerating their whole process of economic development.

Mr Pirie, though not anxious about the adequacy of the world's resources to feed an increasing population, expresses the hope that a time will come when men and women undergo some treatment to make themselves sterile for an indefinite period, until they take some deliberate step to reverse the treatment. Quite apart from its effect in provoking (to use the words of Gregory King) "more frequent fornication and adulteries", those who adopted such a proposal would soon find out that not many children are conceived entirely deliberately, and would begin to suffer the economic and political disabilities of underpopulation. But how better can I conclude this discussion than in the words of Malthus himself?—"If it were possible for each

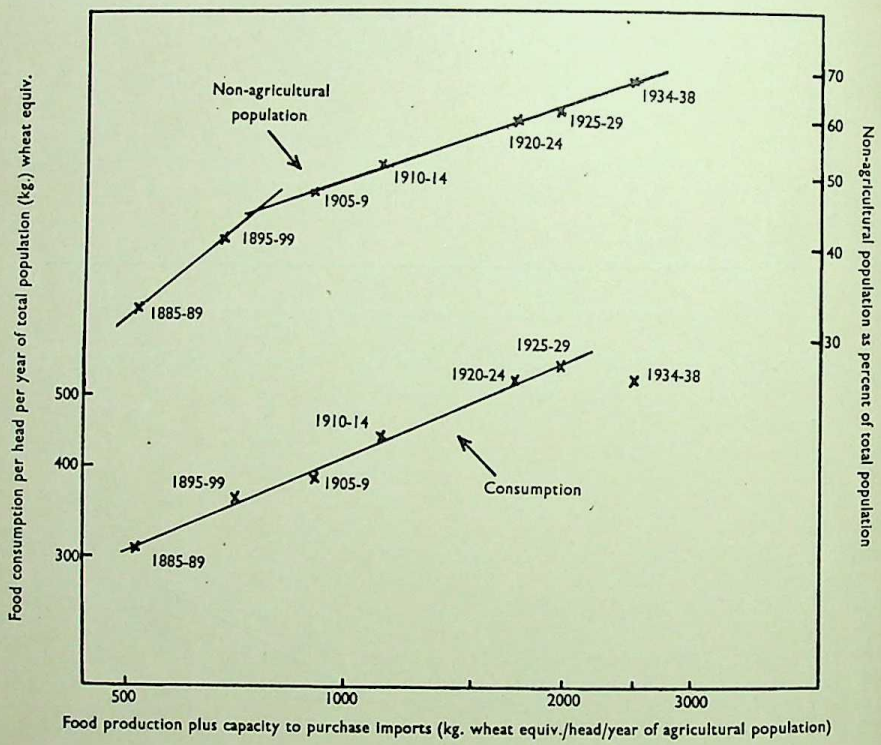

FIG. 8. The economic development of Japan.

married couple to limit by a wish the number of their children, there is certainly reason for fear that the indolence of the human race would be greatly increased; and that neither the population of individual countries, nor of the whole earth, would ever reach its proper and natural extent" (Malthus, 1817).

May I conclude by saying that I could not have written this paper without the help of Miss M. R. Haswell, B.Litt., M.A. of the Agricultural Economics Research Institute, a specialist in the problems of primitive agriculture. I also acknowledge useful help from Mr G. H. Peters, M.Sc., and Mr K. R. Clark.

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APPENDIX

System of Weights

Grains†	
Wheat	1·00
Rice (rough)	0·80
Rye	0·75
Barley	0·65
Oats	0·65
Maize	0·75
Millet	0·68
Sorghum	0·60
Buckwheat	0·65
Other grains (as maize)	0·75
Starchy roots†	
Potatoes	0·65
Sweet potatoes	0·30
Cassava	0·23
Oil crops†	
Castor beans	1·50
Cottonseed	0·87
Copra	1·35
Groundnuts (unshelled)	1·10
Linseed	1·45
Sesame	1·80
Soy beans	1·30
Olive oil	6·83
Palm kernels	1·08
Rapeseed	1·30
Sunflower seed	1·10
Sugar†	
Sugar (raw)	1·03
Pulses†	
All pulses	1·12
Nuts†	
Nuts (unshelled)	2·45
Fruit (total)†	
Citrus	0·92
Other fresh fruit	0·68
Fruit (total)†—*continued*	
Bananas	0·30
Cider fruit	0·10
Raisins	2·50
Dates	1·30
Vegetables†	0·65
Wine†	0·90
Livestock products†	
Cattle and calves	3·00
Pigs	4·30
Sheep and lambs	3·75
Poultry	5·80
Milk	1·30
Eggs	5·80
Fibres†	
Abaca	2·20
Cotton	6·50
Flax	6·08
Hemp	5·43
Henequen	1·90
Jute	2·00
Sisal	2·20
Wool, greasy basis	1·19
Silk, cocoons	1·25
Other crops†	
Cocoa	4·50
Coffee	5·60
Tea	9·00
Tobacco	8·00
Rubber	5·18
ADD:	
Fish	3·50
Vegetable oils	3·00

† Klayman (1960).

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Discussion on the Papers by Mr Pirie and Professor Clark

Mr G. V. Jacks: An important feature of the present situation is that the supply of food exceeds the demand. Economically, demand is a far more potent factor in production than need. Thirty years ago there was ample food supply in this country and a large need for food, but thousands of people went hungry because they had not the means to demand food. The thing to be considered is how to increase the demand for food, which means that people earn enough money to buy it. One way for them to do that would be to produce motor cars, washing machines, etc., and use some of the wealth they create to buy food. The food would then be produced.

Mr A. S. Mani: In India, it will be difficult to change the food habits of the people and make use of cattle as pre-digested food for at least two generations, so it is most important for us to increase the existing food supplies. Rice is one of the staple foods, particularly in the south, as it is in the coastal regions of South-east Asia. This rice, or what we call paddy crop, requires a lot of water, one-fourth of it being submerged in water for nearly two and a half months. I should like to know whether a variety of paddy is being evolved which would take less water so that the regions which have to depend on the vagaries of the monsoon could utilize the minimum quantity of water to produce more rice. In some parts of India where copious quantities of water are available we generally raise three crops a year, but in the majority of the fields there is hardly one crop because there is not adequate water.

Mr N. W. Pirie: British food habits have changed, in my lifetime, in a very remarkable way. It seems to me that the changes have generally been for the worse; but they are still changes. At Rothamsted, people who five years ago ridiculed the idea of making leaf protein and would not touch it now bring along visitors and say, "Have some of this: it's good." I have not changed the protein, they have changed their outlook. Food habits can be changed more easily than you think.

I do not know much about rice cultivation, but I understand that there are varieties of rice adapted to dry management, although they are not as productive as the wet rices.

Professor Calder: I believe most American rice is dry. Perhaps a hybrid rice could be developed. It is very difficult to move a great mass of people on the subject of food habits, but you can readily supplement the diet without changing the food habits. You are not asking them to step over from vegetable eating to meat eating or anything like that.

Professor Clark: The poorer the diet of a people is, the more difficult it is to change their habits. In countries living on highly sophisticated diets such as ours, the introduction of two new foods in my lifetime (grapefruit and breakfast cereals) has met with a resounding success. Whether you could make such changes with similar ease in Central America and West Africa is another point, even if you used the same sales pressure, and even if you assumed that the money were there to pay for it.

Miss D. F. Hollingsworth: Speakers from Indonesia, Fiji and Ethiopia at another symposium all said that food habits were changing in their countries, usually for the worse, with the introduction of highly purified Western-type foods. I think one of the reasons is that women find it easier to prepare these foods. In this country, we have records in the National Food Survey which now go back for 20 years for the United Kingdom and we have noticed that new foods seem to start being used in the south, in London, and spread northwards and outwards. They also start being used among the younger housewives who presumably carry the new fashion onwards as they get older.

Professor Clark: Changed food habits are not necessarily to be identified with changed food patterns. The introduction of cornflakes into the British diet did not involve a change in food pattern, it merely took the place of a certain amount of bread or toast. And it is doubtful whether the introduction of grapefruit did anything much more than reduce some consumption of oranges.

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Mr J. C. McKENZIE: When we talk of people changing their tastes it is usually from the bottom upwards and it may result from an admiration for western civilization or something of the sort. But it is very different, and much more difficult, to work from the top downwards and suggest that people should change their food habits for some nutritional or other reason.

Mr C. B. HOWE: Professor Clark's strictures on the Chinese economic achievement in the last 12–13 years were very sweeping in view of the paucity of information, a paucity partly due to the immense practical and conceptual difficulties in producing figures for national income and production. The diagrams he produced showing the negative or zero marginal productivity in the rice fields would suggest that there may in fact have been a labour surplus. His examples of the use which was made of labour diverted from agricultural purposes, and of the blast furnaces which were useless in the "great leap forward", were rather unfair. In fact, even apart from industrialization, there have been very considerable achievements in irrigation, conservation, rural construction and so forth, particularly the Kwai River and Yellow River projects, achievements which I do not think could be matched previously in Chinese history. I wonder if Professor Clark actually has the figures for the increase in agricultural production in the post-1949 period when this diversion took place, because I think even Luard and Hughes, who certainly did not hold a brief for Chinese Communism, admit that there was a fairly substantial increase in production in this period, far more substantial, I should have thought, than Indian achievements. (I chose 1949 as a base date since this was the first year of Mao's rule, and it was Mao whom Professor Clark specifically singled out for criticism.)

Dr KLATT: In the years 1960–61 and 1961–62 the average food consumption in China was of the order of 1,850 calories or a little above, of which 80 or 85 per cent. were derived from carbohydrates. The last harvest was a little better than the two previous ones and there were in addition over 5 million tons of grain imported from Canada and Australia. Although this calorie intake is nothing to write home about, it represents neither starvation nor hunger on a nation-wide scale. What 1,850 calories as a national average implies is that there are certain periods, particularly before the new harvest, when in certain areas there have been conditions of maldistribution leading to malnutrition. This is an awful situation for a country that has claimed to have solved all agricultural problems and was considering in 1958 skipping one year in seven in agricultural production altogether; but it is a long way off the exaggerations which we have had in some journals, suggesting that the people of China as a whole nation of 650 million or more live on 600 or 800 calories a day throughout the year. I entirely share Professor Clark's view that a major miscalculation led to a major misjudgement in economic policy based on the wrong assumption that there was a great deal of agricultural surplus in China.

An infinitely more important point, because it affects the whole of Asia outside China, is whether there is in fact surplus labour on the scale that is often cited. I can quote the personal experience of seven months' work in 150 villages in Burma. On farms of 5 acres, that is 2 hectares or more, cultivators' families are normally occupied for 11 months in the year, which adds up in my calculation to about 1,500 working hours. This is, I think, ample in tropical paddy-growing societies, if one compares it with the normal working day of a European in these countries. I reckon that two fully working persons in a family would produce a wheat equivalent of about 3 tons, which, if we take the minimum requirement Professor Clark has mentioned, would be sufficient for a family of 12, and there is no such family. What does this mean?

Professor CALDER: They sell some of it.

Dr KLATT: People do not only eat; they also have to sell in order to get non-food items, and there is a wide variety of them, more than is usually recognized. You only have to go into villages to see what is sold by hawkers, to recognize how many people in the

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villages actually live on non-agricultural occupations, and how large a proportion of the village consumption basket falls on non-food items. There are fewer signs of malnutrition than were implied in the first two world food surveys of FAO, which, I suspect, underestimated consumption levels.

It is exceedingly important to get a clearer picture of the potential labour surplus in the villages not at certain non-peak periods of the year but throughout the season. There are unquestionably some societies in Asia with a labour surplus although tools available at present to the working population are primitive. However, where about 5 acres of irrigated land are available per family, there is, on the whole, no genuine surplus labour available throughout the season and it therefore cannot be skimmed off for non-agricultural industrial employment until and unless tools are brought into the villages which create a labour surplus.

Professor CLARK: In Bengal, where the rainfall and soil are very good, Ghosh found that a family who had as much as 3 hectares of land tended to begin leasing it out to their neighbours, which roughly confirms that they are fully occupied. A Chinese diet of 1,850 calories is below, but not so far below, the agreed minimum. I would point out that it is well below the average consumption of the 1930s as reported by Buck. Mr Howe asked about the rate of economic growth in China between 1949 and 1958. This is what I call "growthmanship", namely selection of your base date. The year 1949 was the last of a long period of invasion and civil war and currency chaos. Production was as low as it could be, and any government which could not get an increase of production on 1949 would have been pretty poor. The antidote to growthmanship is to take a long period measurement, which is the only fair thing to do. The year 1958 was a year of very good harvest. Since then food supplies have been definitely worse than in the 1930s. If I exclude the years after 1958, and try to make a comparison between productivity in 1958 and in the 1930s, I get the long period rate of growth at only about 1 per cent. per person per year, which is a very poor rate of growth.

Is there surplus labour in Chinese agriculture? It is quite true there are indications of zero marginal productivity, but at the same time, in view of the seasonality of the demand for labour, it is possible to have a labour shortage in one season, but zero marginal productivity measured over the whole year.

Miss M. HILLS: I have eaten Mr Pirie's leaf protein on a number of occasions without harmful results. I believe it has been fed with success to protein-deficient children in Jamaica. I was impressed by the report of the Nutrition Society's symposium on "Science and Technology in the Campaign against Malnutrition" (*Proc. Nutr. Soc.*, **20**, ii, 1961) in which workers from India, Turkey, Iraq, Pakistan and Ghana referred to the gruels made from the local cereal and whatever milk is available which are fed to weaned children. Two of these speakers said that if symptoms of protein deficiency, e.g. diarrhoea, occurred it was usual to reduce the quantity of milk. Others spoke (as Dr Passmore did) of the breadwinner receiving the largest share of the protein in some countries, as the result of all the members of the family eating from one dish. It seems that much might be done by helping mothers to understand the protein needs of their growing children and thus altering such customs.

Mr N. W. PIRIE: The word "kwashiorkor", I understand, means "the rejected one". It is the name for the baby pushed off the breast when the new baby is born. Kwashiorkor is a widespread problem in the wet tropics where there is insufficient milk and babies are weaned on predominantly starchy foods. Leaf protein has been given, on a small scale, to children in Jamaica. The result was similar to feeding trials with pigs. This is a good protein and well worth using, but it is not as good as milk; that is precisely what we expected.

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Dr PASSMORE: There are an enormous number of adult Indians who do not weigh more than 50 kg. and are therefore very much underweight. I am absolutely certain that it is not for genetic reasons but because they did not get enough protein when they were children. I am not so naïve as to say that a 50-kg. Indian adult would in any way be inferior to a 65-kg. Indian. It is probable that a considerable part of the Indian mortality and wastage of children is associated with inadequate protein intake.

Mr E. H. M. BADGER: I was interested in Section 5 of Mr Pirie's paper in which he talks about getting foods from trade effluents. As a result of recent legislation about clean rivers, more and more industries are using biological processes for purifying their effluents. If you do that, about 40 per cent. of the carbon content of your liquid goes into bacterial cells which comes out as a sludge, usually regarded as a nuisance. You either dump it or recycle it back to your tanks, to do a rather unnecessary secondary oxidation. In the gas industry and the coke oven industry we have been working on biological purification of effluents for some years. I admit it seems rather improbable that any waste product from a gas works could be a useful food, but it is no more improbable than the fact that you can breed bacteria to feed on carbolic acid. We read reports from eastern Germany that these sludges are being used as additions to cattle food and we thought we would like to get some of our own assessed. This has now been done and I should be delighted to send some to Mr Pirie.

Dr D. S. MADGE: I wonder if Professor Calder could give us a résumé of future trends and developments in world-wide food distribution? He mentioned, earlier, an important international meeting of experts to be held in the near future. Could he give us some idea of the scope of this meeting (and any others) and also outline the large-scale economic problems to be considered?

Professor CALDER: Three things are happening in the United Nations at the moment. One is the Freedom from Hunger campaign which is very appropriate to what we are discussing, another is the World Development Decade, which is supposed to be under way, and the third is the big conference being held in Geneva in February, which is bringing together the scientists, the economists, the management and so on. It is not just a lot of people reading papers for the edification of their fellow scientists. It is an attempt to integrate the disciplines in the social sciences into this problem and it will, I think, be extraordinarily useful. But Mr Pirie's point is right, there is just no imagination in the people who are supposed to be dealing with this kind of problem at the statesmen's level. We have tried over the last 12 years to deal with it and I think we have dealt with it quite inadequately. For instance, the original idea of development, agricultural and otherwise, was that it should be by conventional banking through the World International Bank. That proved completely futile because they expected to show returns, when most of this problem is really putting a bottom into a bog of poverty throughout the world. Then there was the Technical Aid Programme. The Technical Aid Programme became window shopping. It simply showed people on a small scale what they could have had if they could have afforded it. Now we have the Paul Hoffman Special Fund. Again, this is not adequate. What is being done is following up on a pilot plan level the kind of things which technical aid showed you ought to have. What is needed is a tremendous imaginative break-through on the part of the economist, to set up the infrastructure which enables the kind of things we have been talking about to be done. I challenge anyone who says that there is only 10 per cent. malnourishment in the world. I spend a great deal of my time travelling about, I know what the face of poverty looks like.

Mr T. W. WYATT: I regret that the financial aspect of this problem has not been dealt with. It is useless to discuss a problem of this kind unless the financial implications are understood.

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CONCLUDING REMARKS

BY BRINLEY THOMAS

I am sure none of you envy me at this point. I certainly cannot sum up the whole of the day's proceedings, so what I am going to do is give you a few minutes of highly subjective impressions. The first impression I have is the extraordinary contrast between the opening address, the pointed sense of urgency we got in Professor Calder's remarks, and then the rather calm sense of unflappability that we had in the other papers, though this was perhaps all to the good. There has been hardly any reference to planned limitations of births, a lot of reference to the need for girth control, but hardly any reference to birth control. In the morning we were being given a prospect of the demand side, i.e. the probable rate of increase of the number of people, of mouths, in the world, and the probable rate of what you might call the physiological requirements of those people. In the afternoon we had the possible rate of increase of the food that you can get from existing land and also by blocking present and future leaks from the store or annual output of the world's food. On the whole I leave the conference as a pessimist. I think Professor Clark is decidedly a long-run pessimist. On the facts of population, there is no question from Dr Benjamin's analysis that up to 1975 the population of the world is going to increase by a third, and if the United Nations is right in its latest assessment, by the end of the century, only 40 years ahead, the population of the world will have gone up from 2,900 million to 6,900 million. I suppose a word of caution is needed there. You know how often demographers have been wrong. Since the seventeenth century there has been a cycle of prophecies. Our last Royal Commission was set up because we were afraid of under-population in this country. It took six years to do its work, and by the time its report was ready the cycle had changed. Of the underdeveloped countries we are told that it is possible that they will be able to miss out a few of the stages that the developed countries went through, so that they can come to the fourth stage of Dr Benjamin's model quickly without taking the long time that we took. This is rather depressing to me because if they do that they will get to the fifth stage which I think you ought to add to the model, i.e. the United States case. The United States, the richest country in the world, has a rate of growth of population equal to that of a large part of Asia. I am surprised that in the United States so few demographers seem to be interested in this important problem. We cannot bank on the stages argument, because if underdeveloped countries go to the fourth stage quickly they will arrive at the fifth stage all the more quickly and there will be a late population explosion.

Mr Pirie showed that there are many promising ways of securing a great increase in the amount of edible food, and pointed out that the main obstacle was the lack of imagination not of scientists but of those who control the money. I must say I did not realize until today what an appalling gap there is here. Then Dr Passmore drew our attention to the changes which have taken place in the diets of people in the richer countries and the further changes which can be expected as the human species evolves into *homo sedentarius*. We and the Americans choose to have an animal diet, and this means that out of every 100 calories fed to the animal about 90 are used up by it and only 10 are left as food for human beings. The rich peoples of the world feel they are entitled to have these palatable diets as part of the good life. The ultimate logic of Professor Calder's argument is that such countries should not be free to use the fruits of their technical progress in any way they please. This raises profound questions. For example, it is sometimes held that mass alms-giving by the rich countries to the poor would gravely dislocate the price mechanism, and that the damage thus caused would be far greater than the evil exemplified in the occasional destruction of surplus foods. Perhaps the answer depends on the scale on which these things are done. In a world where so many millions of people are hungry it is morally repugnant that any edible food should be deliberately destroyed. Surely it is possible to organize the transfer of surplus foods to those who desperately need it, without thereby doing any serious damage to the operation of the price system.

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The record of Japan demonstrates what can be done through intensive agriculture. The cultivated acre per capita in that country is 0·17, and calories produced per cultivated acre per day 13,200 compared with 4,500 in the United States and 2,500 in India. In considering the relevance of Japanese methods to other parts of the world, it must be remembered that this tremendous productivity per acre entails a relatively large utilization of labour.

This conference brought forcibly to our attention the stern challenge posed by the coming increase in world population. Mr Pirie concluded that ". . . if no baby were conceived unless positively wanted, that is to say, if conventional contraception were inverted and a couple had to do something to restore an otherwise inhibited fertility, it is at least possible that the problem of over-population would solve itself. Work along these lines is as greatly in need of support as work on increased food production." Professor Clark, whose paper gave striking examples of the difficulties of increasing food production in poor countries, opposed Mr Pirie's view. On the facts before us, however, it is difficult to avoid the conclusion that, both in logic and as a matter of mercy, knowledge of birth control must be made available to all the peoples of the world, particularly the low-income countries.

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The Reverend Thomas Bayes, F.R.S. (1702–61)

By J. D. Holland
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On November 10th, 1763, Dr Richard Price (1723–91) (see Thomas, 1924; Thorncroft, 1958) of the Unitarian Church, Stoke Newington, wrote a letter to Sir John Canton, F.R.S., which begins: "I now send you an essay which I have found among the papers of our deceased friend Mr. Bayes, and which, in my opinion, has great merit and well deserves to be preserved. Experimental philosophy, you will find, is nearly interested in the subject of it; and on this account there seems to be particular reason for thinking that a communication of it to the Royal Society cannot be improper."

It was in this manner that Thomas Bayes's paper, "An Essay Towards Solving a Problem in the Doctrine of Chances" (1763), was first published, and it ranks as one of the most famous, least understood, and controversial contributions in the history of science. Recently the Essay has been edited with modern notation and idiom by Barnard (1958), who says: "Bayes' mathematical work, although small in quantity, is of the very highest quality, and contains certain thoughts that did not receive as clear expression again until almost a century had elapsed."

Very little is known about the personal history of Thomas Bayes, yet he expressed ideas in science and in the humanities which will endure as long as man retains his freedom of thought and the power to express it. He was not prepared to accept established opinion on theology or on scientific subjects without the most searching scrutiny and for these reasons anything he has to say on the questions he raised is of human as well as scientific interest. Above all was his integrity in which lay the readiness to search for objective truth and the courage and ability to state it clearly.

Thomas Bayes was born in 1702 and the evidence suggests that his birthplace was in Hertfordshire. Unfortunately, the bishop's transcripts of the parish registers of Bovingdon, Hemel Hempstead, Herts., covering the period 1700–06 are missing. Urwick's (1884) comment on this is acute: "The historian's time and labour would be marvellously served if Parochial Registers, like Non-parochial, were accessible at some central office. The old register of St. Albans Abbey Church was found in a hay-loft and that of another Herts. parish was cut up by a tailor for a measure."

Thomas was the eldest son of the Rev. Joshua Bayes (1671–1746) and Ann Bayes (1676–1733). He had three brothers, John (1705–43), Nathaniel (1722–64) and Samuel (1712–89), and three sisters, Mary (1704–80), Ann (1706–58) and Rebecca (?). Joshua was ordained in 1694 (with Edward Calamy and others) at Little St. Helens Meeting House and was the minister at Box Lane, Bovingdon, Herts., until 1706, four years after the birth of his first son Thomas. (See James, 1867; Wilson, 1814; Toulmin, 1814; Jones, 1849.) He was assistant at the Presbyterian Meeting House, St. Thomas's Church, Southwark, until 1723 and finally became the Pastor at Leather Lane in Hatton Garden, London, until his death.

He was well known as an able lecturer and he helped to raise money for the assistance of county nonconformist ministers in a series of lectures, principally at

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Salters Hall. Many of these ministers were in need and suffered hardship but they had a common bond and were steadfast in times of persecution.

Joshua was the nephew of Samuel Bayes of Trinity College, Cambridge, ejected minister of Grendon in Northamptonshire, and was educated under the Rev. Richard Frankland, M.A., of Christ's College, Cambridge. (Frankland was the founder of the Academy from which Manchester College, Oxford, traces its origin.) Joshua was also a man of wealth, his portrait is in Dr Williams's Library and he lived in Milk Street in the Parish of Saint Mary Magdalene. (He was not a member of the Royal Society as stated in some references.)

No records have been traced of Thomas's boyhood days but we do know that he received a "liberal education for the ministry" and it is reasonable to suppose he received his education at a dissenting academy. These academies are to be distinguished from the dissenting schools of the time. (See McLachlan, 1931; Trevelyan, 1946.) The latter were charity foundations but the academies made good to some extent the shortcomings of the two universities. They provided a sound background of literary and scientific subjects and for those destined for the ministry, instruction was given in all branches of divinity including the oriental tongues. Their success was due to the personal exchange of ideas at close quarters. In the opinion of Dr W. A. Shaw, "The abiding claims of the dissenting academies to historical recognition rest on the high standard of academic education which they maintained during a century in which the English universities were nearly as palsy-stricken as the church."

The only academy in the London area during the period 1716 to 1730 was Coward's Academy (later known as the Hoxton Academy) in Tenter Alley, Moorfields. (See McLachlan (1931) and Minute Book of Coward Trust.) It was founded in 1701 by a Mr Coward of Walthamstow (d. 1738) and was later supported by the Congregational Fund Board. Unfortunately, the names and record of students for the period 1704 to 1738 have not been traced and, while we cannot furnish direct evidence of Thomas's attendance at this Academy, the following facts emerge.

Firstly, it was within easy reach of the Bayes's home in Milk Street, and secondly, John Eames, F.R.S. (d. June 29th, 1744), was one of Thomas's sponsors for election to the Royal Society (in Crane Court, Fleet Street) on November 4th, 1742. His other sponsors were Martin Folkes, President of the Royal Society, an antiquary; James Burrow, an antiquary; Cromwell Mortimer, physician; and the Earl of Stanhope. (For references to Eames see McLachlan (1931) and Smith (1954).) Eames was the principal tutor in languages, mathematics and natural philosophy at Coward's and would certainly be eligible to sponsor a former pupil. He was a great scholar and, although lacking in oratorical gifts, he attracted to his lectures some of the most distinguished pupils who ever entered a nonconformist academy. He was a friend of Sir Isaac Newton (1642–1727). Thomas's friend, Richard Price, attended this academy from 1740 to 1744 although he was not, of course, a contemporary of Thomas.

As far as we can ascertain, the theory of probability was not in the curriculum at Coward's, although under Eames's tuition the students learnt mechanics, statics, hydrostatics, optics, spherical geometry, and the use of applied mathematics in navigation, geography and astronomy—a comprehensive course. This fact, perhaps, strengthens Professor Barnard's theory that Thomas may have learnt the mathematics of probability from "poor De Moivre" who earned a living in London teaching mathematics at Slaughter's Coffee House in St Martin's Lane (Clarke, 1929).

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Alternatively we may believe that Thomas received his education further afield, but no records have been found to substantiate this. It was not unusual for students to move between academies. Quite a number went to Scotland, especially to Glasgow, and to the Continent, mainly at Leyden. If the lists of students at Coward's Academy are traced and contain no reference to Thomas, it is by no means conclusive evidence that he was not a student at Coward's. According to the Rev. C. E. Surman, known lists of academical students are far from complete.

In 1723 Joshua Bayes became Pastor to the congregation at Leather Lane and Thomas assisted his father until about 1729. We next find Thomas's name on Dr John Evans's (1767–1827) list of "Approved Ministers of the Presbyterian Denomination" in 1727, and in 1731 he became the Presbyterian minister, succeeding the Rev. John Archer at the Meeting House, Mount Sion, in Tunbridge Wells (Strange, 1949). This chapel was opened in 1720 and was situated, at the time, in the fashionable quarter of the town. According to Benge Burr (1766), it was a "capacious, handsome, presbyterian meeting house".

We do not know the reason Thomas went to this town, but we do know that it was customary (and still is) among Nonconformist congregations to invite approved ministers to settle over them in the pastoral office. A minister has complete freedom of choice where he settles or how long he remains. Thomas's estimation of Tunbridge Wells could well have been founded on the following (James, 1867): "There were Arminians and Socinians, Calvanists and Antinomians and Ranters and Libertines; there are many of them whimsical, giddy and unstable; yet it must at the same time be acknowledged there are many Christians among them." An excellent field for a young minister to begin work in.

It is of interest to know that Thomas's period as a minister at Tunbridge Wells coincided with the period when this town was at the height of fame as a centre of fashion and elegance and a favourite resort for London citizens in search of a healthy holiday. (See, for example, Church (1948), Knife (1916), Melville (1912), Pearce (1912), and Betsy Sheridan's Journal (1960).)

Tunbridge Wells was, in the words of Defoe, "agreeably covered by good houses, fine gardens, and fruit trees" and, indeed, the countryside has a particular charm and an ethereal beauty in apple-blossom time. The town was famed for its chalybeate waters whose efficacy was advertised to a degree that was only matched by the incredible credulity of the public. Art flourished and was a part of normal life and trade. The famous Tunbridge ware (originated in 1685 at Tonbridge), of design based on the natural colour of wood to form mosaics and patterns of great beauty, sold in quantity.

Most of the social activities of the town came under the direction of a Master of Ceremonies, firstly Belle Causey from 1725 to 1735, and then by Beau Nash until his death in 1761. Nash persuaded the formidable Sarah Porter to come from Bath and organize "touting" and to enlist subscribers for the assembly rooms ("touters" were named after the people of Tooting). Nash was Master of Ceremonies in the grand manner, on easy terms with Royalty and men like Dr Johnson, Pitt, Garrick, Handel, and many other distinguished patrons of the town. He mixed with commoner and noble alike and managed to maintain an aloofness from his fellows as befitted his eminence; yet he caught the popular imagination and commanded their respect. He was a majestic figure who maintained his usefulness until his death.

Defoe says, "Tunbridge Wells wants nothing that can add to the Felicities of life, or that can make a Man or Woman completely happy, always provided they have

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money." This comment is in line with other material, and expresses in some degree the solace that was sought by the inhabitants from ominous events of national concern. For example, in 1745, Prince Charles had become ruler of most of Scotland, had established an invasion base at Gravelines and had marched as far as Derby.

Reference to letters and other writings of people who were likely to have had some personal contact with Thomas reveal very little information about him. A capable reporter of the times was Mrs Elizabeth Montagu (1720–1800), wife of the grandson of the Earl of Sandwich. She held parties where "cards would not be thought of and where ladies might participate in conversation with literary and ingenious men". She was the first of the "Blue Stockings", so named because once a "gentleman appeared at one of her assemblies in blue worsted instead of black silk stockings". The list of eminent scientists who visited the town includes Dr Robert Smith, Master of Trinity College, a noted mathematician, and Mrs Montagu cannot understand why he was so fond of Tunbridge Wells "for it seems not very agreeable to the nature of a philosopher". Again from her letters, she names a person "most taken of" Philip, the second Earl of Stanhope (one of Thomas's sponsors to the Royal Society). She says, "He is always making mathematical scratches in his pocket book."

Mrs Montagu's comments on intellectuals is worth recording. Of one gentleman she writes, "He has not found that the wisest man in the Company is not always the most welcome, and that people are not at all times disposed to be informed."

William Whiston (see Whiston, 1753), who succeeded Sir Isaac Newton as Lucasian Professor of Mathematics at Cambridge, met Thomas in 1746 and they breakfasted together. Whiston records "respectful admiration" for his host's mathematical ability. Samuel Richardson, the novelist, records in 1748: "An extraordinary old man we have had here . . . the noted Mr. Whiston, showing eclipses' and explaining other phenomena of the stars."

The Pantiles was the main promenade for important people who visited the town, and the scene of pageantry and glitter at the height of the season can have been rarely equalled except perhaps at Bath. The Beaux strolled along in pigtail wig, long-decorated coat, lace ruffle and shirt front, velvet breeches, gay stockings, and high-heeled shoes. Swords were forbidden by Beau Nash and he banned smoking in public places. Feminine dress was equally colourful and very large hats were worn of almost every shape known to geometry. Dairymaids and fruit girls wore hooped-out dresses and sold their goods in the Lower Walk.

Thomas himself was no doubt suitably attired. There are plenty of engravings and portraits at Dr Williams's Library which show ministers of the period dressed in full wig, Geneva-type preaching gown, cravat and "bands". Away from the pulpit, the normal attire was a wig (often a tricorne hat), dress coat, knee breeches, stockings, silver-buckled shoes, and a cravat, often of a distinctive ministerial type of folding. Mount Sion Chapel is only some four hundred yards from the Pantiles and Thomas's comments on the scene and life in general in this unique town would have been of interest.

In 1731, presumably after his arrival in Tunbridge Wells, Thomas Bayes wrote a tract, which attracted notice. It was in answer to Balguy and Grove who maintained that moral uprightness (rectitude) and wisdom were the "first spring of action in the Deity". The following quotations are taken from this tract:

> (Page 22.) "I don't find (I am sorry to say it) any necessary connection between mere intelligence, though ever so great, and the love or approbation of kind and beneficent actions."

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(Page 27.) "If kindness or a good disposition be not the spring no matter what the nature or consequence of the action be, however beneficial it may be to us, we like the being that produced it never the better; we don't think ourselves obliged to gratitude, or imagine him any ways the more perfect, as to his moral character on account of it."

(Page 46.) "He says, the essentials of beauty are order and proportion; but if I am mistaken not, the essence of deformity may as truly be asserted to be also order and proportion. In the same sense that one kind of order and proportion constitute beauty another kind of order and proportion seems to constitute deformity. Objects are not therefore ugly because they want order and proportion."

(Page 72.) "If the universe was to consist of uniform sort of beings however happy they might be 'tis evident that they could not in some respects enjoy so great a happiness as they might by variety; there would then be one uniform object of contemplation, one uniform relation among all the creatures of the universe; which could never afford so great a happiness as variety of these to intelligent and rational creatures, whose happiness is in great measure derived from objects of contemplation, and the relations they stand in one to another."

About this period Sir Isaac Newton's work on fluxions was subjected to fierce attack. Some of these attacks contained sly innuendoes and veiled insults and were highly abusive.

One of Sir Isaac's antagonists was Bishop Berkeley, a contemporary of Bishops Butler, Bentley and William Law. The bishops, almost without exception, were either relatives of noblemen or wealthy men and yet none had been raised to the Episcopate for services rendered to the Church (Trevelyan, 1946). The custom was, however, not without some benefit. Bishop Berkeley was a noted philosopher and metaphysician. He attacked Sir Isaac's work under the pseudonym of the "Analyst" and also, we believe, on another occasion as the author of *The Minute Philosopher*. The *Analyst* was printed in 1734 and resulted in several controversies becoming the starting point for all philosophical discussion of the new mathematics in England during the eighteenth century (Clarke, 1929).

Sir Isaac's work was defended by a number of intellectuals, including Benjamin Robins, F.R.S., and J. Walton, but the most scathing reply, in Sir Isaac's defence, came from Thomas's pen in a tract published in 1736. (This tract was anonymous for reasons that are well understood but is ascribed to Bayes by his contemporaries, as may be seen in copies at the British Museum and Dr Williams's Library.) The following are quotations:

(Preface.) "But the invidious light in which he has put debate by representing it as a consequence to the interests of religion is I think truly unjustifiable, as well as highly impudent."

(Page 47.) "To suspect Sir Isaac Newton of the mean design of seeking reputation among the ignorant by venting unintelligible notions, and defending them by artful cunning and cunning sophistry, is what I think no man is capable of doing."

(Page 48.) "He [i.e. the Analyst—Bishop Berkeley] represents the disputes and controversies among mathematicians as disparaging the evidence of their methods; and, Query 51, he represents Logics and Metaphysics as proper to open their eyes, and extricate them from their difficulties. Now were ever two things thus put together? If the disputes of the professors of any science disparage

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the science itself, Logics and Metaphysics are much more to be disparaged than Mathematics; why, therefore, if I am half blind, must I take for my guide one that can't see at all?"

In 1746 Thomas lost his father and he inherited about £2,000 and his father's library. He and his brother Samuel were appointed executors to a trust fund amounting to £1,400 on behalf of his sister Rebecca in order to prevent Thomas Cotton, Rebecca's husband, from paying his debts with his wife's money. Rebecca received "the sum of £40 for mourning" and the rest of the family shared a sum of nearly £10,000 between them. (A considerable sum in those days. A craftsman's wages in 1740 were approximately £50 per year, and a labourer's wages were about £15 less than this figure (Cole and Postgate, 1946).)

Thomas's inheritance seems to have been untouched because he left most of it to be divided between his family and friends including £200 to be divided between "John Hoyle late preacher at Newington and now I suppose at Norwich and Richard Price now I suppose preacher at Newington Green".

There is little to add to the scant documentation that exists with respect to Thomas's life in Tunbridge Wells. A reference by Timpson (1859) describes him as "a gentleman of fortune but not a popular preacher", an attribute he shared with Richard Price. Popularity can be of an ephemeral nature and, in relation to a minister, the term may not include the gift of guidance or the ability to give lasting comfort to those most in need; or even the ability to strengthen belief already held.

The Rev. Onelly, a clergyman of nearby Speldhurst, described Thomas as the "best Greek scholar he had ever met" (Timpson, 1859) and we also find that he was a friend of a local family named Jeffrey. Thomas left "£500 and my watch made by Elliot and all my linen and wearing apparel and household stuff to Sarah Jeffrey daughter of John Jeffrey (a holloware turner) living with her father at the corner of Jourdains Lane".

Thomas was unmarried and it is possible that Sarah looked after his household or assisted in some useful manner at Mount Sion chapel. She was married to Robert Jeffrey, a wheelwright. No further information has been found about Sarah or her family. Her date of birth is uncertain. There were five "Sarah Jeffreys" buried in the churchyard between 1777 and 1815 and the name is common, according to the Rector of Speldhurst who checked the Registers.

In 1749 Thomas wished to retire from his ministry and he allowed the Independent ministers from London to use the Chapel and take services each week (Timpson, 1859). It is recorded: "All that summer of 1749 we had supplies from London, Sabbath after Sabbath; 'twas indeed a summer to be remembered." This arrangement was terminated on Easter Sunday in 1750 when Thomas resumed his pulpit because of dislike of the Independents' doctrine and despite protests they had to find other accommodation (Strange, 1949). (In the early part of the eighteenth century the division between various forms of religious belief, even among Nonconformists, was greater than it is today. According to Thorncroft (1958), the "Independents stood for orthodoxy and soundness of doctrine, while the Presbyterians placed greater emphasis on integrity and free enquiry". Furthermore, the sombre philosophy and narrow religious viewpoint of John Calvin still exercised considerable influence on Independent thought.)

A valuable relic has been preserved in Thomas's notebook. This book is in the muniment room of the Equitable Life Assurance Society, the "Old Equitable" founded in 1761, and came into their possession through Richard Price and his

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nephew, William Morgan, who gave advice to the Society on actuarial matters. The notes are undated but the pages are numbered and may be representative of a number of such books Thomas entered and kept for reference purposes. The writing is distinctive and contains the same characterizations noted in Thomas's Essay and other material lodged at the Royal Society. The material includes a method of "finding the time and place" of the conjunction of two planets, some notes on weights and measures, on a method of differentiation and a note on logarithms.

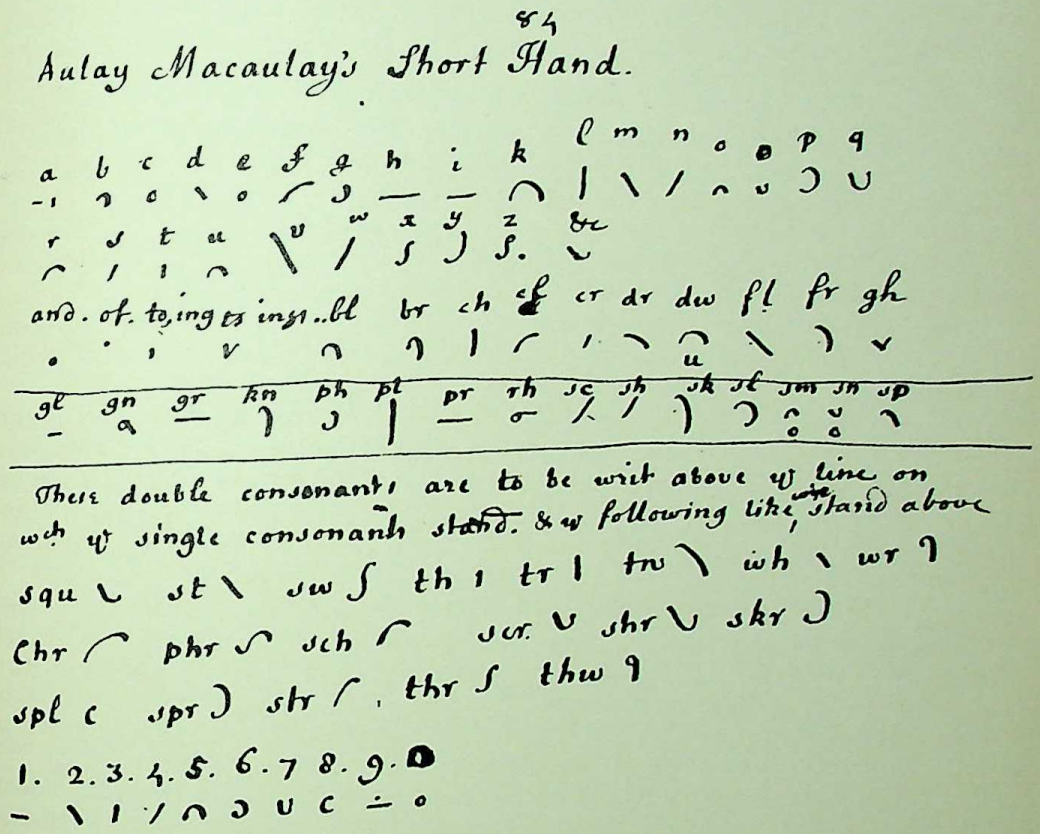

FIG. 1.

Of particular interest, written in the concise style of an engineering report, is a discourse on an electrifying machine. Rather strangely, the first page of this report is headed "Paris July 4th 1746" and the material concludes with some astonishing results which make good reading. Interposed in the notebook is material which appears to be subject matter for sermons. These notes are partially in shorthand, and on page 84 is the complete key to a system of shorthand (Fig. 1) which was later found to bear no relation to the material in the notes or to that taken from the Sir John Canton papers (MM.1.17, Royal Society) and shown in Fig. 2.

Thomas's interest in electricity in relation to electrically charged bodies is evident and it would have been of interest to know what he had to say on a problem that was not solved until nearly a century had elapsed. The shorthand has been examined by a number of authorities (Mr W. J. Carlton, of Andover; Mr F. McD. Turner,

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Pepysian Library, Magdalene College, Cambridge; and Professor W. Matthews, Department of English, University of California, Los Angeles).

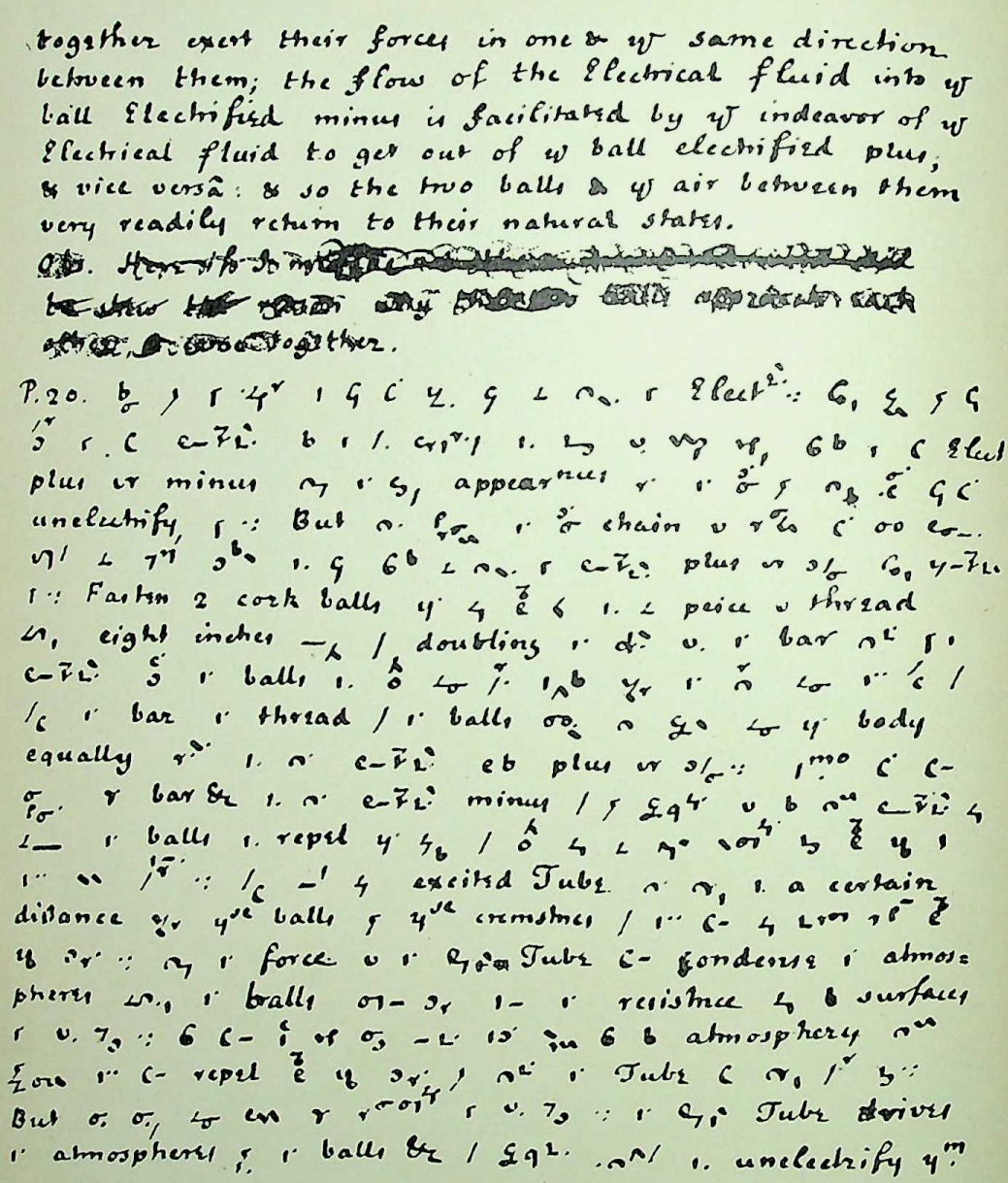

FIG. 2.

The system shown in Fig. 1 was, it seems, extracted by Thomas from Aulay Macaulay's *Polygraphy or Short-Hand*, Manchester, published in 1747 (3rd edition in 1756). The shorthand shown in Fig. 2 is not, as at first thought, that of Thomas Shelton which Pepys used in his diary. Professor Matthews suggests that the symbols should be examined, on a frequency basis, compared with the frequencies of letters

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of the alphabet, and then the analysis collated with Julius Ensign Rockwell's book on early shorthands. If the system is discovered, the British Museum or Pitman's may have the original text-book from which the system can be learnt and the notes transcribed. Shorthand was freely used in the eighteenth century, particularly by ministers, and one system that was "easily acquired" was invented by Jeremiah Rice in the seventeenth century, but the key to this system has not been traced (McLachlan, 1931). (A certain John Orton had made himself "so perfect a master of it that he could take down the whole of most of the sermons which he heard".)

One fact that emerges from this study is Thomas's reticence and dislike of publicity. He could well afford a funeral oration (a common practice in those days) but he directed that his funeral expenses "may be as frugal as possible". It seems doubtful that Thomas took any personal steps to gain admittance to the Royal Society. His 1736 tract in defence of the mathematical art was published at an opportune time and was of such merit that it is likely that his election was unanimously agreed to by members of this distinguished body. (We note that the President, Martin Folkes, was a sponsor.) We do not believe he obtained election in the manner of some members who contributed nothing of merit but were wealthy enough to pay the high admittance fees and yearly dues. More stringent measures taken about 1743 led to a resolution "not to receive any person as a member who had not first distinguished himself by something curious" (Clarke, 1929).

He left his papers and books to the Rev. William Johnston, his successor at Mount Sion in 1752 (see Strange, 1949), and it was fortunate that Richard Price found Thomas's Essay. The greatest credit belongs to Richard for forwarding the Essay to Sir John Canton.

The friendship of Thomas with Richard Price remains an enigma. It would seem that a feeling existed between them that amounted to more than respect for each other's intellectual ability and in a number of ways both men had similar characteristics and shared a common interest. Both were interested in the new mathematics, both were indifferent preachers but were able to express themselves in writing of lasting quality on religious and philosophic subjects. Furthermore, both held the same shade of religious belief; Richard was an avowed Arian (the forerunner of modern Unitarianism) and Thomas, according to Dr S. W. Carruthers, was also an Arian. It is likely that Thomas discussed the theory of probability with Richard, a point we infer from a study of Bayes (1763)—but Richard's omission of Thomas's introduction to his Essay is unfortunate. We might then have understood the reason that prompted Thomas to solve a problem not attempted by mathematicians of the calibre of De Moivre, Jean Bernoulli, Von Leibniz, and others.

We have included the names of scientists and mathematicians who visited Tunbridge Wells, but it is doubtful if Thomas discussed the fundamental problem he solved because he makes no reference to the work of others in his Essay; a point that would not have escaped him. He did, however, point out the fallacy in Mr Simpson's hypothesis relating to a method of taking the mean of a number of observations in relation to astronomical work (in a letter in the Sir John Canton Papers). The mathematicians in this country were a brilliant set and included men like Brook Taylor, De Moivre, Ditton, Whiston, Robins, Maclaurin, Simpson and Robert Smith. Mathematical societies flourished and an old-established gathering was in Spital Market where, it is reported, "each man had his pipe, his pot, and his problem" (Clarke, 1929).

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Richard Price's close friends for 30 years were Sir John Canton, Benjamin Franklin and Joseph Priestley. Thomas may have been acquainted with them yet we cannot find evidence on this; further study on these lines is continuing.

It may be thought that Thomas's parochial duties confined him to Tunbridge Wells; furthermore travelling, even to London, was not without hazard. The turnpike road was almost impassable in winter, and in summer a light white sand blew everywhere. Not until 1762 was the road improved. Tunbridge Wells to the Golden Cross Inn at Charing Cross was a journey of 7 hours, representing an average speed of about 5 m.p.h. Stops not listed were frequent owing to Sarah Porter's "touters" and the occasional peril from highwaymen was real. The coaches had no springs but despite the discomforts the service was much used and was fiercely competitive. Arrival at the destination on time, despite these hazards, was a matter of prestige to the coachmen who maintained a bawdy outlook to anything that hindered their progress.

This quiet man, of earnest thought and abiding faith and of immense intellectual stature, died on April 17th, 1761, and rests (within a few yards of Richard Price) with most of his family in Bunhill Fields Burial Ground by Moorgate.

The family memorial (156 in Section 12) is approximately midway between Bunhill Row and City Road and is in great disrepair. According to the City Surveyor, the Corporation of London have recently obtained Parliamentary power to effect improvements to this Ground, but as far as can be seen this memorial will not be disturbed.

Acknowledgements

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Estimated Expenditure on Road Transport in Great Britain, 1960

By R. F. F. Dawson

Road Research Laboratory, Department of Scientific and Industrial Research

Summary

This paper estimates the expenditure by road users, other than the Service Departments, on road transport in 1960. The expenditure, including depreciation, is given separately for goods vehicles, public service vehicles, cars and cycles; it is calculated largely from figures of operating costs for different classes of vehicles, supplemented by information from a large number of other sources. The total estimated expenditure of £3,000 million includes £640 million taxation, and a sum of probably about £50 million due to duplication; the total excludes just over £400 million of net investment in new vehicles.

1. Introduction

The first comprehensive estimates of the expenditure on road transport in Great Britain were made for the years 1949 and 1950 by Rudd (1952). Extrapolations of Rudd's estimates have been made for the other years since 1950, some of which have been published (Glanville and Smeed, 1958). This paper gives an estimate of the expenditure in 1960, at market prices, and uses much the same sources and methods as were used for the earlier estimates.

Expenditure on road transport can be divided into that on private transport, i.e. private motoring and carrying of goods under a C licence, and expenditure on public transport, i.e. the carrying of passengers by public service vehicles and taxis or the carrying of goods under an A or B licence. This is the type of division which is adopted in the national income and expenditure blue books (Central Statistical Office, 1961), where road transport is generally included in a wider heading and expenditure on transport under C licences is not given at all.

This paper estimates the expenditure for each class of vehicle and does not differentiate between private and business uses. All purchases of new cars are considered to be investments, and depreciation is counted as an annual cost. This is in contrast to the procedure adopted in the national income accounts, which attempt to divide the purchase of and expenditure on private cars between business and private uses and to treat the purchase of a car in the private sector as current expenditure.

A group of vehicles which does not fall into any convenient category and for which no figures are available, not even the number licensed, are those belonging to the Service Departments. The only statistics that relate to these vehicles are those of petrol consumed by all government vehicles; from these it can be estimated that the expenditure on these vehicles is possibly about £50 million. As this is an extremely unreliable figure, and as nothing is known of how much use these vehicles make of public roads, they were excluded from this estimate.

The method adopted in this paper is to give the expenditure on public service vehicles, then to discuss the various items of expenditure on commercial vehicles; a

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similar procedure is used for taxis, for private cars and motorcycles, and for bicycles. Finally the estimates for the expenditure on all forms of road transport are brought together.

2. Public Service Vehicles

The annual expenditure on public service vehicles, in 1960, is given by the Ministry of Transport (1962); this is shown in Table 1 analysed according to the type of traction.

TABLE 1

Expenditure on public service vehicles in 1960

	£ million
Bus and coach—passengers	296·3
Bus and coach—parcels, mail, luggage, dogs, etc.	0·5
Tram	2·6
Trolleybus	15·6
Total	315·0

3. Commercial Vehicles

The total expenditure on vehicles of any given class depends on the fixed costs per vehicle, the running costs per mile, the number of vehicles and the number of vehicle-miles. The procedure adopted is to determine or to estimate the number of vehicles and vehicle-miles and then apply to them the fixed costs and the running costs which are given in *The Commercial Motor* (1961) for vehicles of different sizes. Where possible cross checks are made.

The Ministry of Transport (1961a) gives the number of vehicles licensed by type of carriers' licence for different unladen weights on December 31st of each year. The number of vehicles in 1960 is assumed to be the mean of that at the end of 1959 and at the end of 1960. The number of vehicles of all classes with licences current at any time during the third quarter of the year is given by the Ministry of Transport (1961b). There is some discrepancy between the numbers of commercial vehicles derived from these two sets of statistics, partly because of the difference in dates; in particular there are many more vehicles under 12 cwt. in the licensing statistics than there are goods vehicles under 12 cwt. in the annual census of vehicles. This discrepancy is presumably accounted for by vehicles that are licensed as cars and which have a C licence for carrying goods. Many of these will have a private as well as a commercial use and for the purpose of this estimate they have been counted as private cars.

From *The Transport of Goods by Road* (Ministry of Transport, 1959), which gives the results of a sample survey carried out by the Ministry of Transport in April 1958, the annual mileages travelled by vehicles with each class of licence and of different weights were taken and multiplied by the number of vehicles in these classes in 1960. The estimated mileage by goods vehicles, in 1960, including agricultural vehicles (which were assumed to travel the same number of miles as C-class vehicles of the same weight) and government vehicles (excluding cars and all vehicles belonging to the Service Departments), was $18{\cdot}46 \times 10^9$ vehicle-miles. This is remarkably close to the estimate of $18{\cdot}66 \times 10^9$ vehicle-miles by light vans and other goods vehicles made by Tanner *et al.* (1962) from traffic counts at the 1,000 points which were used in the sample survey of the roads of Great Britain.

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The analysis of numbers of vehicles and of vehicle mileage in the official statistics and Ministry of Transport (1959) is related to the unladen weight of the vehicle, but the costs given in *The Commercial Motor* (1961) relate to carrying capacity. The average carrying capacity according to unladen weight and type of carriers' licence is given in *The Transport of Goods by Road* and these were used to find the costs appropriate to unladen weights.

For each class of vehicle—classified according to carrying capacity and type of fuel used—the costs were taken from *The Commercial Motor* (1961). The fixed costs for each class of vehicle (licences, wages, rent and rates, insurance and interest) were

TABLE 2

Estimated expenditure on commercial vehicles

£ million

Item	*Expenditure based on tables of operating costs*	*Adjusted expenditure*
Licences	36·9	40
Wages	696·7	643
Insurance	37·9	38
Garages (rent and rates)	33·2	33
Interest	39·1	39
Fuel	211·3	218
Lubricants	16·3	16
Tyres	70·6	60
Maintenance	127·2	127
Depreciation	113·7	92
Profits and other expenses	98·8	99
Total expenditure	1,481·7	1,405

multiplied by the number of vehicles in that class and the running costs (fuel, lubricants, tyres, maintenance and depreciation) were multiplied by the estimated number of vehicle-miles run by that class. Allowance must also be made, in some cases, for overhead charges and profits; these can be derived from the operating-cost tables as the difference between the total operating cost and the recommended minimum charge. Overhead charges and profits were calculated at this rate for A-licence vehicles and at two-thirds of this rate for B-licence vehicles (this being the proportion of tonnage carried by B-licence vehicles for hire according to the Ministry of Transport (1959)).

The results of these calculations on the expenditure on commercial vehicles are given in Table 2.

Independent checks can be made for some of these items and some adjustments were made on the basis of these checks; these are now discussed.

3.1. *Licences*

The Ministry of Transport (1961b) gives the total expenditure on motor-vehicle and trade licences for goods vehicles and tractors as £39·1 million to which has to be added £0·6 million for driving licences, making a total of approximately £40 million.

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3.2. *Wages*

Rudd (1952) deducted half of the wages relating to C-class vehicles of less than $2\frac{1}{2}$ tons unladen weight because "most of the C-licenced vehicles weighing under $2\frac{1}{2}$ tons (unladen) are used for retail delivery and other local work. The drivers of these vehicles frequently work part-time as drivers and part-time in some other capacity." There is undoubtedly some truth in this argument but a deduction of 50 per cent. seems rather high: the percentage distribution of C-licence vehicles of less than 3 tons (unladen), according to the business of the owner, given by Morton-Williams *et al.* (1962), was retail 25, wholesale 15, manufacturing 25, builders and contractors 12, services 21 and general carriers 2. It is impossible to determine what deduction should be made from the wages; in this estimate it has been taken quite arbitrarily as 10 per cent. The total estimate would have been £215 million lower if Rudd's figure of 50 per cent. had been deducted.

3.3. *Interest*

This factor should be calculated by applying the current rate of interest to the depreciated value of the vehicle. In *The Commercial Motor* (1961) interest is calculated at 3 per cent. of the initial cost of the vehicle; as this gives nearly the same result as the first method of calculation no amendments were made.

However, two other amendments were made: an addition for interest paid on hire purchase agreements and a deduction for interest on vehicles which are fully depreciated. From the figures published by the Board of Trade (1961) it was estimated that, on average, throughout 1960 about £90 million were owing on hire purchase agreements for commercial vehicles. The additional interest on this would be approximately £6 million. The original amount calculated for interest was reduced by one-fifth to allow for vehicles which are fully depreciated. (The reason for using one-fifth is given when discussing depreciation.)

3.4. *Fuel*

The Petroleum Information Bureau (1961) published statistics of the quantity of petrol and of diesel oil which is used by different classes of vehicle, from which the expenditure on fuel for goods vehicles, in 1960, can be calculated as £204 million; the estimated expenditures of £8 million on agricultural vans and £6 million on government vehicles need to be added to give a total of £218 million. The figure of £211 million, calculated from the table of operating costs, assumed that all fuel for commercial vehicles is bought at bulk rates.

3.5. *Tyres*

According to the Secretariat of the International Rubber Study Group (1961) the replacement sales, in 1960 in the United Kingdom, of tyres for commercial vehicles were 1,076,000 new covers, 835,000 tubes and 650,000 remoulded tyres; these figures include some for public service vehicles and some sold in Northern Ireland.

On the other hand, some small commercial vehicles will have tyres which are indistinguishable from those sold for cars. The cost of tyres for commercial purposes varies enormously but from the 1958 census of production ((Part 121—Rubber), Board of Trade, 1960), the average wholesale price of tyres of different categories was calculated for 1958 and these prices were corrected to 1960 values by the wholesale price index for tyres and tubes. The wholesale prices were converted to retail prices by assuming that the relation between wholesale and retail prices was the same for all

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classes of tyre—private and commercial. This gives average prices for commercial tyres, in 1960, of £36 for tyres, £2 for tubes and £15 for remoulded tyres. This indicates a total expenditure of £50 million, which, although based on a number of assumptions about average prices, is not likely to be wrong by over £20 million, which is the difference between this estimate and the one based on the tables of operating costs.

The expenditure has been taken as £60 million, i.e. midway between the two estimates.

3.6. *Depreciation*

Depreciation and interest cease to be a charge once the vehicle has been fully written off. Trying to allow for this raises two problems: at what stage in a vehicle's life it is written off, and how many vehicles there are which are older than this. *The Commercial Motor*'s allowances for depreciation are based on expected "lives" of 75,000 to 300,000 miles, according to the size of the vehicle. Using the average mileages quoted by the Ministry of Transport (1959) lives of about 10 years are implied; in other words, vehicles more than 10 years old have been fully depreciated. (This was the dividing line taken by Rudd (1952).) There is little information on how many vehicles are more than 10 years old. In the "Survey of commercial traffic in London" (Morton-Williams *et al.* (1962)), 24 per cent. of the vehicles were more than 9 years old and these were responsible for 21 per cent. of the total mileage. The original amount calculated for depreciation and interest has therefore been reduced by one-fifth.

4. Taxis

The number of taxis (assumed to be hackney vehicles seating up to eight persons) was 11,500. By comparing the amount of petrol used, as given by the Petroleum Information Bureau (1961), with the cost of fuel per mile for taxis, given in the tables

Table 3

Expenditure on taxis, cars and motorcycles

£ million

Item	*Taxis*	*Cars*	*Motorcycles*
Licences	0·2	73·0	4·5
Wages	7·9	—	—
Insurance	0·9	95·0	9·0
Garages and parking	0·3	77·0	—
Interest	0·5	42·0	7·0
Fuel	1·9	298·0	20·0
Lubricants	0·2	28·0	1·0
Tyres	0·5	40·0	2·5
Maintenance and repairs	0·8	200·0	6·0
Depreciation	1·7	307·0	25·0
Profit of taxis and hired cars	6·1	6·0	—
Retailing used cars	—	40·0	—
Total	21·0	1,206·0	75·0

of operating costs, the total annual mileage is estimated to be 245·4 million miles, or an average weekly mileage of 325 miles per vehicle. By taking the operating costs and the recommended minimum charges for a 1,750 c.c. engine doing 400 miles a week

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from the tables of operating costs, the total expenditure, in 1960, was estimated to be £21 million. The breakdown of expenditure (together with that for cars and motorcycles) is given in Table 3.

5. Cars and Motorcycles

As some of the statistics give only a combined figure for cars and motorcycles they are considered together. Several sources of information were used, in particular the A.A. tables of running costs (Blandon, 1960).

The number of vehicles was taken as the number licensed at any time in the third quarter of 1960 given by the Ministry of Transport (1961b).

5.1. *Licences*

The Ministry of Transport (1961b) gives the sum spent on motor-vehicle licences for cars as £70·5 million; to this figure must be added £2·5 million for driving licences. For motorcycles the corresponding figures are £3·6 and £0·8 million.

5.2. *Insurance*

The premiums for comprehensive policies for cars, according to the engine size and the district where the car is usually garaged, are given by Blandon (1960). The average for all cars, weighted by the number in each size category and the number registered in each district (from Ministry of Transport, 1961b), is £23·3. If it is assumed that the average value for which the car is insured is £500, the comprehensive premium would be £24·8 (the average value assumed has little effect on the total; a change of £100 would alter the total by £1·7 million). On the same assumption, the rate for third-party insurance only would be approximately £8.

In the London travel survey (Dawson and Wardrop, 1962) it was found that 70 per cent. of car owners were comprehensively insured, 15 per cent. were insured for third party, fire and theft, and 15 per cent. for third-party risks only. What is not known is the proportion of motorists who are in receipt of no-claim bonuses. If it is assumed that a third of all motorists receive a no-claim bonus of 40 per cent. then the average premium per car is £17·5 which gives a total expenditure of £95·0 million.

According to the London travel survey only 39 per cent. of motorcyclists were comprehensively insured, and of the remainder, half were insured for third party, fire and theft, and half for third party only. If it is assumed that the annual premium is £4 then the total annual expenditure is £9 million.

5.3. *Garages*

It is not possible to do much more than guess at the expenditure on this item as it is not known how many cars are garaged nor how much is paid to garage them. With lock-up garages the required amount is the rent and rates paid for it, but where the garage forms part of combined property it is difficult to say what is paid for the garage, whether in the form of rent, rates or "Schedule A" tax. There is the further complication that garages are often used for purposes other than garaging cars.

If it is assumed that the average cost per week of garaging is 5*s*. per car, including those cars with no garaging cost, then the overall annual cost is about £72 million. The cost of garaging motorcycles is comparatively small and can be ignored.

Apart from regular garaging there are expenditures on parking at garages, at parking meters and at open sites. It is difficult to assess what the total is, but, though it is increasing year by year, it is probably small when compared with other costs.

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The annual income from just under 2,000 parking meters in Westminster is estimated as £140,000 (Hogg, 1960); so the income from all parking meters, in 1960, was probably about £0·5 million. If it is assumed that motorists paid on average £1 per year for parking then the overall total is about £5 million.

5.4. *Petrol*

From the statistics issued by the Petroleum Information Bureau (1961) the expenditure on petrol for private cars and motorcycles in Great Britain is taken to be £318·0 million, to which should be added about £0·2 million for government vehicles. This figure probably consists of about £20 million for motorcycles and the balance for cars. (From the A.A. tables of running costs given by Blandon (1960) the total for cars is estimated to be £321 million.)

5.5. *Lubricants*

From the A.A. tables of running costs the total expenditure on lubricants for cars is estimated to be £20 million. If it is assumed that the expenditure per mile on motorcycle lubricants is a quarter of that per mile for cars then the expenditure for motorcycles would be £1·0 million. Adding the £16 million previously arrived at for goods vehicles and a further £3 million for public service vehicles to the figure for cars and motorcycles, gives a total of £40 million.

From the Petroleum Information Bureau statistics it can be estimated that the total expenditure on motor lubricating oils and greases is £60 million at standard prices. This figure includes some expenditure on greases, included here as maintenance, and makes no allowance for cheaper prices to commercial consumers, so that the final estimate of £29 million for cars and motorcycles does not seem unreasonable.

5.6. *Tyres*

From the A.A. tables of running costs (Blandon, 1960) the total expenditure on tyres for cars is estimated to be £43·8 million.

TABLE 4

Cars and motorcycle tyres sold for replacement purposes in Great Britain, in 1960

	Number (thousands)	*Cost per tyre (£)*	*Expenditure (£ million)*	
Cars				
New covers—conventional	1,755	7	12·3	37·5
New covers—tubeless	1,329	8	10·6	
Tubes	2,034	1	2·0	
Remoulded tyres	3,357	3·75	12·6	
Motorcycles				
New covers	478	4	1·9	2·4
Tubes	474	0·75	0·4	
Remoulded tyres	23	3	0·1	

The number of tyres sold in the home market for replacement purposes is given by the Secretariat of the International Rubber Study Group (1961); after making

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allowances for sales in Northern Ireland the sales figures are as shown in Table 4. Table 4 also gives the approximate average cost of tyres and the estimated total expenditure on them.

The total expenditure is obviously very much affected by the assumed average cost but the estimate of £44 million from the A.A. tables of running costs seems to be rather high. The expenditure on tyres for cars was taken to be £40 million (nearly mid-way between the two estimates) and £2·5 million for motorcycles.

5.7. *Maintenance*

One difficulty in assessing the amount spent on maintenance is that a considerable but unknown amount of maintenance is carried out by owners at negligible expense. Routine maintenance calculated from the A.A. tables of running costs (which appears to agree closely with the rates for the manufacturers' suggested maintenance) comes to £80 million per year. Repairs, which are more variable items than routine maintenance, are calculated from the A.A. tables to be £158 million, thus giving a total for repairs and maintenance of £238 million. If it is assumed that half the routine maintenance is carried out by the owners at a comparatively small cost then the total expenditure on repairs and maintenance is approximately £200 million. This is assumed to exclude repair work paid for by insurance companies.

Maintenance of motorcycles will cost less per mile than that of cars and probably a greater proportion of the work will be done by the owner. On the assumption that the cost per vehicle-mile is a third of that for cars and that a half of all maintenance is carried out by owners, the total expenditure is approximately £6 million.

5.8. *Depreciation*

There is, of course, no precise value of depreciation as it can never be more than an estimate, whereas the other costs are in theory exact amounts, even if it is not possible to measure them precisely.

If it is assumed that the average "life" of a car is 100,000 miles, then, from the known distribution of cars by cubic capacity of the engine (cars licensed according to horse power can be assumed to be fully depreciated), the average rate of depreciation is 1·76 pence per mile, which gives an annual figure of £307 million for cars.

The A.A. tables of operating costs give a total of £581 million, but if cars taxed on a horse-power basis are assumed to be fully depreciated and the relation between age of car and mileage is neglected then the total becomes £483 million. The figures in the tables of running costs appear to be based on lives of less than 100,000 miles, which would account for the difference between the figure quoted in the tables and the figure estimated in this paper.

Assuming an average life of 50,000 miles for motorcycles the total depreciation is approximately £25 million.

5.9. *Retailing of Used Cars and Motorcycles*

The expenditure under this heading is in effect the profit made by dealing in second-hand cars; to calculate this the number of transactions per year must be known together with the average profit per transaction. Approximately 660,000 used cars were bought under hire purchase agreements (at an average of £300 plus down-payment) so the total number of transactions is probably less than a million. It is

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difficult to estimate the average profit per transaction; the motor trade was rather depressed at the end of 1960 and some losses were made. Assuming an average net profit of £40 per transaction a total profit of about £40 million is indicated.

5.10. *Interest*

By preferring a car to a bank balance, interest is foregone and although this is an opportunity cost it is not an item of expenditure. The interest which is paid by those purchasing their vehicles under a hire purchase agreement is, however, a real cost. Although to the purchaser this might appear as part of his investment it is more accurately considered as a current payment.

It is estimated, from the figures published by the Board of Trade (1960), that the average amount of hire purchase outstanding at that time on vehicles was about £400 million, of which about 22 per cent. was for commercial vehicles. If it is assumed that the total credit charges on the original credit extended were 16 to 19 per cent., the annual total would be about £42 million for cars and £7 million for motorcycles.

5.11. *Car Hiring*

Car hiring is a business which has increased considerably since 1950, in contrast to taxis which have decreased to a quarter of the number there were 10 years ago. The general expenses of these cars were included in the estimates for all cars, but the expenditure on wages and on running the businesses plus the profit earned must also be included.

It is impossible to estimate the profit made by car-hiring firms but a figure of £6 million has been assumed.

5.12. *Summary of Expenditure on Cars and Motorcycles*

All the estimates for expenditure on taxis, cars and motorcycles are summarized in Table 3; this shows that the total expenditure on these vehicles, in 1960, was approximately £1,300 million. This includes a number of items that are subject to wide margins of error, and the bias, if any, in the final result is likely to be to underestimate, particularly as the implicit assumption was made that all cars have the same annual mileage whereas it is probable that larger, newer and more expensive cars travel further than smaller and older ones.

6. Pedal Cycles

The expenditure on pedal cycles is not easy to estimate but it is small compared with that on the other forms of road transport.

The total number of pedal cycles is not known; in 1949 it was estimated as 11 million plus those used by children under 16. If it is assumed that the number in use has fallen in the same proportion as the production of cycles or in proportion to the vehicle-miles travelled by pedal cycles, then, in 1960, there would have been between 6 and 7 million pedal cycles.

6.1. *Tyres*

After adjusting for sales in Northern Ireland the sales of cycle tyres and tubes in Great Britain, in 1960, were 3,104 thousand and 3,074 thousand, respectively, at an average cost of about £1 for a tyre and a tube, the total expenditure on cycle tyres was approximately £3 million.

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6.2. *Maintenance, including Lighting*

It was estimated that cyclists spend about 5*s.* a year on batteries and bulbs; if it is assumed that they spend on average 10*s.* on other maintenance, then the annual total for maintenance will be £5 million.

6.3. *Depreciation*

On the assumption that the average cost of a bicycle is £23·5 (*The Financial Times*, September 21st, 1961) the vehicles sold on the home market in 1960 cost £18 million; the estimate in the *National Income and Expenditure, 1961* (Central Statistical Office, 1961) is £15 million. If a bicycle is assumed to last six years then the depreciation in 1960 would amount to £25 million, i.e. there was a disinvestment of £7 million. If bicycles were assumed to last 10 years the depreciation would be only £15 and there would be a net investment of £3 million. With a steady decline in vehicle-miles and presumably in the number of vehicles the former figure seems more probable; therefore, the depreciation on pedal cycles, in 1960, was assumed to be £25 million.

Thus the total expenditure was about £33 million. This is obviously only an approximate estimate but as it is only about 1 per cent. of the total expenditure on transport, the effect of the error on the overall total will be small.

7. TOTAL EXPENDITURE ON ROAD TRANSPORT

The total expenditure on road transport—apart from that by vehicles belonging to the Service Departments which, as indicated above, might amount to £50 million —is given in Table 5.

TABLE 5

Total expenditure on road transport

£ million

Class of vehicle	*Expenditure*
Cars, motorcycles and taxis	1,302
Commercial vehicles	1,405
Public service vehicles	315
Pedal cycles	33
Total expenditure at market prices	3,055

The figure for the total expenditure (Table 5) is a gross estimate in that it includes some duplication. For example, the expenditure of the petrol companies on road transport is included in the gross expenditure of goods transport and again in the expenditure on petrol. It is impossible to estimate accurately the amount of duplication. The census of production by the Board of Trade in 1960 (Part 63 — Motor-vehicle manufacturers) shows that vehicle manufacturers spent £5·6 million on transport in 1958, "excluding the value of transport provided by the business covered by the return". From information supplied by one of the main petroleum companies it can be estimated that the expenditure of all the companies on road transport is about £20 million. These two examples are probably the main sources of duplication; the total duplication is probably about £50 million.

An outlay which has not been included in this estimate is the investment of new vehicles—the gross figure for this, in 1960, was approximately £900 million. The net

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investment in new vehicles is this total less depreciation of £470 million, including an allowance for public service vehicles; net investment is therefore £430 million.

Several of these costs include taxation, licences, fuel tax and purchase tax, the total of which, in 1960, was £640 million.

The total net expenditure at factor cost is therefore £2,790 million (3,050 less 50 plus 430 less 640) which is approximately 13 per cent. of the national income at factor cost. At market prices the £3,430 million (2,790+640) is approximately 14 per cent. of the total domestic expenditure.

Apart from users' expenditure on road transport there are some expenses which are not paid for directly by the users, notably expenditure on roads (£85 million on investment and £115 million on maintenance and administration in 1960). There is also the expenditure the police incur in controlling road transport, the expenses of street lighting and a number of other miscellaneous items of expenditure.

Acknowledgement

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Statistics of Human Conflict†

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Lewis Richardson was an unusually versatile scientist, who made important advances in a number of superficially unrelated fields. He died in 1953. By initial training a physicist, his early interests came to lie principally with meteorology. It is a measure of the extent to which he was ahead of his time in this subject that he devised a theoretically acceptable method of weather prediction from physical observations a quarter of a century before the advent of electronic computers made it into the practical approach which is now the basis of all scientific weather forecasting. He studied atmospheric turbulence, and made mathematical and practical contributions to the general problem of eddy diffusion and to other aspects of the theory of gases. He was elected a Fellow of the Royal Society in 1926.

In his later years he turned increasingly from physics to mathematical and statistical studies of the social psychology of international relations, particularly as exemplified in wars and arms races, and in this field too he broke new ground. These studies were the outcome of a long-cherished intention "to spend the first half of my life under the strict discipline of physics, and afterwards to apply that training to researches on living things". As part of this programme, he took a degree in psychology at London University at the age of 48. The two books under review summarize the results of his researches into the mathematics of human conflict. Richardson published the manuscripts on microfilm (*Arms and Insecurity* in 1947, with a second edition in 1949, and *Statistics of Deadly Quarrels* in 1950), and made further amendments and additions up to the time of his death; they have now appeared in print for the first time.

Although the books were published together, each is a self-contained treatment of a distinct aspect of the scientific study of international relations. They are reviewed jointly because they are pioneer—and still almost unique—contributions to a subject, of increasingly urgent practical importance, which should be capable of much further development.

It is convenient to start with *Statistics of Deadly Quarrels*—although it was conceived by Richardson later than the other book—because it is the more likely to be intelligible to the general reader. It consists of a statistical study of the frequency of occurrence of wars and other deadly quarrels, according to their magnitude, at different times and in different places and circumstances. The approach is always objective, thorough and dispassionate.

Richardson classifies deadly quarrels basically by magnitude, which he takes as the logarithm to the base 10 of the number of persons killed as a result of the quarrel.

† A review article on *Statistics of Deadly Quarrels*. By Lewis F. Richardson. Edited by Quincy Wright and C. C. Lienau. xlvi, 373 p. 9¼″; and *Arms and Insecurity. A Mathematical Study of the Causes and Origins of War*. By Lewis F. Richardson. Edited by Nicolas Rashevsky and Ernesto Trucco. xxv, 307 p. 9¼″.
Pittsburgh, The Boxwood Press; Chicago, Quadrangle Books Inc.; London, Stevens & Sons Limited; 1960. 63*s*. each volume, 115*s*. the set.

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He is thus able to include on a single manageable scale the Second World War, of magnitude 7·4, and a murder, involving only 1 death, of magnitude 0. In 1940 (on his retirement to the banks of Holy Loch) he started a careful search to compile a list of all wars of magnitude greater than 2·5 throughout the world, which ended in the period A.D. 1820–1929 (later extended to 1949 and then to 1952). The final (or very nearly the final) version of this list is part of the present volume; Richardson believed that the record of the 92 wars of magnitude greater than 3·5 (that is, involving 3,163 or more deaths) ending in the period 1820–1929 in this list was complete, except for the possibility that there might have been in the middle of Africa, before 1860, some wars of this magnitude that were never recorded. The list continued to grow by the addition of later and smaller wars until the author's death.

A record of the number of persons killed in a war is often uncertain by a factor of 2, but much of this essential uncertainty is removed by grouping the quarrels in units of magnitude, as defined above. Because the parties to a quarrel often publish partisan statistics, Richardson consulted authorities whose bias, if any, was likely to be opposite, and struck a mean between their statements of casualties; for the same reason, as well as for simplicity, he abandoned the attempt to allocate the numbers killed to the various belligerent groups.

The first two chapters of the book (which occupy one-third of it) contain a full discussion of the criteria on which this list was compiled, and the list itself. Each war is characterized by its dates and its magnitude, by a matrix of the belligerent groups involved, by their several aims and the result of the war, and by a detailed classification of the pre-war social situations between the various pairs of belligerents, whether these were likely to make for amity (such as a common language) or for enmity (such as previous fighting between them) or were ambivalent (such as exceptional war preparations on one or both sides). The sources of information on each war are given. The result is a remarkably comprehensive collection of historical facts concerning all recent wars.

Not surprisingly, Richardson was largely defeated in his attempts to study quarrels with magnitudes in the range 2·5 to 0·5 (315 to 4 deaths), such quarrels being "too small to interest historians and too large to interest criminologists", and he makes no effort to list them. Nevertheless, he brings together a certain amount of numerical data of doubtful reliability on quarrels in this class, concerning slave-raiding in East Africa, murders by groups of thugs in India, banditry in Manchoukuo and ganging in Chicago. There is more information on the range 0·5 to 0 (1 to 3 deaths), as these are classed as murders. The author tabulates statistics from many countries and estimates that the world total of murders from 1820 to 1945 was about 6 million, involving (with the inclusion of multiple murders and judicial executions) nearly 10 million deaths.

The rest of the book consists of a variety of ingenious analyses of this accumulated material. The distribution of wars in time is studied first. The numbers of outbreaks of war each year, and the outbreaks of peace, each agree closely with a Poisson distribution, with the disturbing implication that these events may be occurring purely by chance. The outbreaks of war in another list of wars, compiled on different principles by Professor Quincy Wright over a longer period of time (432 years), follow the same distribution, although a more detailed subdivision of this material indicates that the parameter λ fluctuated rather unpredictably during the period. Richardson's shorter, but more objective, series shows only chance fluctuations in λ for quarrels of magnitudes 2·5 or more. In particular, there is no evidence in his series

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of any general trend towards more, or fewer, fatal quarrels, even when the doubling of the world population between 1820 and 1940 is taken into account. There was, however, a tendency for large wars to become more frequent, and small wars less frequent, perhaps as a result of improved communications.

The author turns to a study of the relation between the frequency of wars and their magnitude. By plotting the number of quarrels, suitably transformed, against their magnitude, a linear relationship is found over the range of magnitude 3·5 to 7·5. A slightly steeper line, or the introduction of a curved section, permits this part of the diagram to be linked acceptably with that for murders, at the other end of the magnitude scale. In the interests of continuity Richardson joins the two parts by a circular arc (which gives an estimate of 354 wars of magnitudes between 2·5 and 3·5 compared with the author's incomplete total of 188 in the same period), and is then able to estimate the world total of persons who died because of quarrels of all magnitudes between 1820 and 1945. The heaviest losses of life occurred at the two ends of the scale, namely, 36 millions in the two World Wars (magnitude $7 \pm \frac{1}{2}$) and 10 millions in connection with murders (magnitude $0 \pm \frac{1}{2}$); the estimated total for all magnitudes was 59 millions. This represents about 1·6 per cent. of deaths from all causes. "Those who enjoy wars can excuse their taste by saying that wars after all are much less deadly than disease."

It is widely believed that certain nations or states may be inherently more belligerent than others. A study of 94 wars of magnitude greater than 3·5 shows that throughout the period 1820–1945 new belligerent groups were continually appearing, and at a fairly constant rate. Participation in wars was widespread; for example, Britain took part in 28 of these wars (more than any other state), Russia in 18, Germany (or Prussia) in 10 and Japan in 9. Without attempting to resolve the "obscure and controversial distinctions between aggression and defence", Richardson is content to point out that none of these states can have been the aggressor in more wars than the number in which they participated. Further analysis shows that the involvement of states in external wars increased with the number of their frontiers, suggesting that geographical contiguity plays some part in quarrels, but a more detailed study of this topic is postponed until later in the book. In an examination of the economic causes of wars, Richardson finds that marked economic disparity between the opposed belligerents (indicative of a "class war") was noted by the historians in only 11 of 83 wars of magnitude greater than 3·5 (in an earlier version of his list), whereas 33 of these wars were for territory.

For his next group of studies Richardson analyses not complete wars, but pairs of opposed belligerents in wars of magnitude greater than 3·5. He first distinguishes civil from international conflicts according to the presence or absence of a common government just before the outbreak of hostilities; between a fifth and a sixth of all these conflicts were civil. He finds a tendency for civil wars to decrease in frequency with an increase in the duration of common government preceding the quarrel; and concludes that common government probably has a pacifying influence. He investigates "retaliations", that is, the recurrence of war between previously opposed belligerents, in a similar manner; more than 40 per cent. of pairs of belligerents had fought against each other previously. Again the numbers of retaliations decrease with an increase in the number of years of peace, as if there was a slow process of forgetting and forgiving. There is a curious pair of associations between retaliations and the magnitude of wars; larger wars appear to lead to more retaliations than smaller wars, but these retaliations tend to occur in connection with smaller wars.

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Two chapters deal respectively with the associations between languages and wars and between religions and wars. It is relatively simple to enumerate pairs of belligerents according to their languages or religions. But to decide whether diversity of language, or of religion, has been a cause of war, some estimate is needed of the *expected* numbers of conflicts between groups speaking particular languages, or following certain religions. This problem is much complicated by the fact that, by virtue of the occurrence of civil wars, the conceivable belligerent groups are not the same as political states. Richardson therefore develops a number of theoretical models involving pairs of population groups of a defined size, which lead him to upper and lower limits for the numbers of pairs of opposed belligerents with the same language (or religion), on the basic assumption that sameness of language (or religion) is irrelevant. He concludes that there were fewer wars in which both sides spoke one of the Chinese languages, and more in which both sides spoke Spanish, than would be expected from the total populations speaking those languages. "For the other chief languages the statistics neither confirm nor refute Zamenhof's belief that a common language would have a pacificatory effect." As regards religions, there were many fewer wars than expected between those adhering to the Chinese mixture of religions (Confucian–Taoist–Buddhist); the figures suggest, but do not prove, that Christianity incited wars between its adherents, and that the Moslem religion prevented wars between its adherents. Further, there were many more wars between Christians and Moslems than would have been expected if religious differences had played no part.

A chapter on the number of nations on each side of a war is nearly identical with Richardson's paper in this *Journal* (Richardson, 1946). He enumerates the wars, both in his own and in Quincy Wright's list, according to the numbers (r, s) of belligerents on each side, in the form of a bivariate frequency distribution in which, when $r \neq s$, half the observed frequency is entered in each of the possible positions. The type (1, 1) is the most frequent, the cell frequency decreases more rapidly along the diagonal than along the lines $r = 1$ or $s = 1$, and the type (3, 2) is rarer than the type (4, 1). After fitting an empirical formula which adequately describes the distributions from the two lists, Richardson considers a variety of theories which might account for the observed pattern. He finds, for example, tolerable agreement with history by supposing that each dispute interested only eight nations, and the probability that any two of the eight came to war with each other about it was uniformly 0·35. This clearly fails to explain the rare wars in which $(r+s) > 8$, and so the influence of geography is introduced explicitly into a further theory, which allows differing probabilities of conflict between powers according to their geographical circumstances, and in particular according to whether they rank as sea-powers or as local powers. This theory, of a "chaos restricted by geography", succeeds in part, by explaining the rare occurrence of very complicated wars, but fails in so far as the observed probability of two local powers being in conflict is not (as postulated by the theory) constant, but increases with the number of powers involved, as if fighting were infectious. Richardson therefore concludes that "a chaos, restricted by geography and further modified by the infectiousness of fighting", offers the best description of the observed distribution of wars by type. "Whoever says that these results are rough should compare them with our previous blank ignorance."

In the final two chapters Richardson returns to and develops a theme touched on in various places earlier in the book, namely whether geographical contiguity between population groups provides a sufficient explanation of the frequency of occurrence of

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war between them, or whether some other influence is operating. The last chapter is nearly identical with Richardson's paper in this *Journal* (Richardson, 1952). He shows first that if the population of the world is regarded as divided into equal-sized groups, each liable to quarrel with its neighbours, the actual numbers of quarrels are far fewer, if the groups are small (or far more numerous, if the groups are large) than would be expected from the geographical opportunities for conflict, suggesting the existence of some local pacifier. A study of the duration of fighting in wars of different magnitudes, taking the geographical contiguity of the belligerents into account, confirms the existence of this local pacifying influence. Its nature is less easy to discover; in an attempt to do so Richardson developed a theory of "mapping by compact cells of equal population". (A monograph on this subject, which promises to be of theoretical interest, formed part of the manuscript of *Statistics of Deadly Quarrels* and is to be published separately.) This theory provides measures of the separate geographical opportunities for civil and foreign conflicts, and permits the conclusion that some influence suppressed civil fighting relative to foreign fighting. Thus the local pacifying influence appears to be the habit of obedience to a common government; however, intermarriage, a common language, a common religion and a tendency to dislike foreigners may also have local pacifying effects which are obscured by their correlation with common government.

Arms and Insecurity is an inherently more difficult book, consisting of a mathematical study of the psychology of international relations, as exemplified particularly in arms races. The first chapter is in the nature of a preamble, in which Richardson examines the theory that the possession of armaments is an insurance against war. He finds, for the nations involved (including the European neutrals), that there was no obvious correlation between the percentage of their population killed in the First World War and their annual *per capita* defence expenditure in the preceding few years. Thus armaments appeared neither to prevent, nor to promote, casualties in war.

The remainder of the book consists of the development of mathematical equations to describe the relationships between nations, and the testing of these, with a disconcerting degree of success, against observed data, particularly in connection with the two arms races of 1908–14 and 1929–39. The whole theory stems from a pair of differential equations relating the "defences" x, y of two nations, namely

$$dx/dt = ky - \alpha x + g,$$
$$dy/dt = lx - \beta y + h.$$

The first of these equations summarizes the contention that the rate of change of the defences of one nation (x) with time is proportional to the level of the defences of the other nation (y), which the first nation interprets as a threat to its security, k being a positive "defence coefficient"; but that this rate of change is restrained by the cost of the first nation's own defences, α being a positive "fatigue and expense coefficient", and is affected also by a constant feeling of "grievance" ($g > 0$) or "contentment" ($g < 0$) towards the other nation. The second equation summarizes the reciprocal contention concerning the defences of the second nation. These equations were first published in 1935, as a simplification of an earlier theory developed during the First World War. There are certain obvious consequences of these equations; disarmament and satisfaction ($x, y, g, h = 0$) lead to permanent peace; disarmament without satisfaction ($x, y = 0$; $g, h \neq 0$) is not permanent; nor is unilateral disarmament ($y = 0$), in this theory for two nations.

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Further consequences of these equations are studied algebraically, and also geometrically, regarding them as the equations of motion of the point (x, y) in a plane. A point of balance, which may be stable or unstable, exists in this plane. There are also two perpendicular straight lines in the plane ("barriers"), which intersect at the point of balance only if $g = h = 0$. These define four areas in the plane; unless the coefficients change, the point (x, y) is confined to a curved trajectory within one of these areas, or moves along one of the barriers, either towards or away from the point of balance.

Although historians mention arms races occasionally in connection with wars before 1914, the available statistics of pre-war military expenditure do not suggest that there was an increasing annual rate of expenditure on armaments in the years preceding the France/North German Confederation War of 1870, or the Russia/Turkey War of 1877, though they do suggest such a trend before the Russia/Japan War of 1904. Arms races therefore appear to be a modern phenomenon, dating from about 1900, this being possibly connected with the increasing application of science to warfare, making long-term preparations for war necessary, and so more obvious to other nations, and more likely to be interpreted as threats.

Richardson devotes several chapters to a study of the European arms race of 1908–14. During these years the rate of increase of the combined defence budgets of the Triple Entente and the Triple Alliance itself increased steadily—sufficiently steadily for a curved regression line not to be justified. This finding is consonant with the above theory; further comparison of the facts with the theory suggests that the point (x, y), equated to the defence expenditures of the two groups, moved along one of the barriers away from the point of balance (the grievances between the two groups being regarded as negligible). Extrapolation (supposing this to be justifiable) then indicates that a reduction of £15 million in the warlike budget of the Entente, and one of £11 million in that of the Alliance, in the year 1906–07, would have brought (x, y) to the point of balance, with the implication that the arms race might never have begun. These sums represent between 10 and 15 per cent. of the defence budgets for that year, and are of the order of the expenditures of the same nations in two days of the subsequent war.

Richardson points out the possibility of negative values for x and y in his theory and extends his original idea of regarding these variables as "threats" between nations to a concept of "threats minus cooperation". Throughout the book he therefore endeavours to set some measure of cooperation against the warlike expenditure of his analyses; the best measure he could find was expenditure on trade between the countries concerned, but, as might be expected, this does not prove very satisfactory. During the period 1904–13 there was an increase in the total value of the goods passing between the Triple Alliance and the Triple Entente, although there was a marked recession in 1908. Richardson examines the effect of equating x and y to the defence expenditures less a proportion of the trade expenditures; taking this proportion as a quarter gives once more a good fit between theory and observation.

The study of the arms race of 1908–14 was simplified by two main considerations: first that, viewed retrospectively, there were essentially only two groups of nations involved, and secondly that the national currencies were stable and could be expressed in terms of gold. Richardson's original study of the arms race of 1931–39, outlined here, was completed in 1938, when the grouping of nations into belligerents was still uncertain, and so it had to be handled as a multi-nation rather than a two-nation problem. In re-examining this arms race in detail in the present book, Richardson

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deliberately uses the same multi-nation approach, so that the reader may assess its potentialities. Moreover, between 1921 and 1939 there was no stability of currencies, nor any unequivocal means of expressing one in terms of another. Richardson had therefore both to develop the theory to cope with more than two nations, and to explore how to assess the warlike effort of a nation, other than by the amount of its defence expenditure expressed in the national currency. The latter he achieved by replacing the annual defence expenditure of the country by the number of persons who could earn that amount of money in the course of a year, at current average wage-rates in that country, a statistic he refers to as the "warlike worktime".

Richardson's generalization of the original equations from 2 to n nations is

$$dx_i/dt = g_i + \sum_{j=1}^{n} \kappa_{ij} x_j \quad (i,j = 1, \ldots, n)$$

where x_i is a measure of "threats minus cooperation" for nation i, and g_i and κ_{ij} are constants. He considers first, however, three nations (or three groups of nations), and again handles the equations algebraically and geometrically, considering various special cases, particularly that of one "compliant" nation ($\kappa_{ij} = 0$ for $i = 2, 3; j = 1$) and two "pugnacious" nations. If the compliant nation had no grievances against the other two, the aggressive preparations of the pugnacious nations towards it would settle down to a constant level or disappear, and the two pugnacious nations would interact with each other, drifting in certain circumstances towards war and in others towards an alliance.

Richardson next discusses in some detail a number of special solutions of the equations for n nations, and in particular investigates criteria for stability of the various configurations. He is able to obtain a general criterion for stability among nations of different "sizes", however, only if their relations are unaffected by distance, and commends the more general problem, for a world of imperfect communications, to the attention of mathematicians.

He then turns to the practical problem of estimating the various coefficients for the 10 main nations involved in the 1931–39 arms race, preparatory to testing the adequacy of his theory. He uses values of the fatigue and expense coefficients (κ_{ii}) estimated from a study of rates of demobilization (as shown by reductions in warlike worktime) in different countries from 1919 to 1924, and values of the defence coefficients ($\kappa_{ij}, i \neq j$) based on the warlike worktimes in the 10 countries in 1935. A study of the equations incorporating the resultant matrix shows that they implied not only a general drift towards war, but also the existence of two opposed groups of nations, with France, Russia, Czechoslovakia and the United Kingdom on the one side, and Japan, Italy and Germany on the other; the U.S.A., Poland and China were, according to the analysis, not clearly on either side in 1935. The inference, from data before 1939, of an eventual alliance between Britain and Russia against Germany is of particular interest in view of the surprise non-aggression pact between Russia and Germany in August 1939 and the equally unexpected attack of Germany on Russia in June 1941.

Richardson, having purposely ignored foreign trade in the foregoing analysis, then introduces it. By subtracting a fraction of the "foreign-trade worktime" (which he gives reasons for taking in this context as one-seventh) from the warlike worktime he obtains a more satisfactory fit of the annual values to his theory during the period 1929–38 than with the warlike worktime alone. He concludes that although trade

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between nations has tended to prevent them quarrelling, its effect is clearly inadequate as a pacifying influence.

Richardson turns finally to the whole period 1919–39, to see whether his linear theory (for two groups of nations) holds from the end of the First World War to the beginning of the next. He considers Germany on the one hand and France, Britain, Poland, Czechoslovakia and Russia on the other. From 1919 to 1933 the point corresponding to $(x+y, x-y)$ appears to have moved along a barrier to a point of balance, where it remained from 1922 to 1931, and then began to move along a second barrier, apparently in the same field. Between 1933 and 1934, however, the trend changed abruptly, and the point then moved along what appeared to be a barrier in a new field, with a different point of balance. The change would correspond to an abrupt alteration in the defence coefficients and in the fatigue and expense coefficients to new constant values, at the time when the Nazis came to power in Germany.

The arms race from 1933 onwards has already been studied in detail. Further consideration of the period from 1919 to 1933 leads Richardson to the conclusion that the trend between 1919 and 1930 cannot be explained entirely in terms of his basic pair of linear equations, with coefficients appropriate to a period of "war-weariness", since these would require an increase in German warlike worktime (y) during the period, which did not occur. A modification of the basic equations, to

$$dx/dt = ky\{1-\sigma(y-x)\}-\alpha x+g,$$
$$dy/dt = lx\{1-\rho(x-y)\}-\beta y+h$$

where ρ, σ are positive coefficients of "submissiveness", is therefore introduced (and its properties studied) to take account of the possibility that one nation may tend to submit to another if confronted with a marked disparity in armaments. Here ρ (for Germany) is taken as positive and σ as 0. With this modification, the equations fit not merely the period of disarmament and the 10-year pause near the point of balance, but also the turn into the arms race from 1930 to 1933. Alternative explanations of the turn into the arms race are offered by the trade depression, or by the fading of war-weariness, in about 1930, or by a combination of these with submissiveness; it is not possible to discriminate between these possibilities.

The editors contribute a preface to each volume. The books have in common certain introductory material and a list of references, a photograph and a biographical note of the author (reprinted from the *Obituary Notices of Fellows of the Royal Society*), and a complete bibliography of his publications on the causation of wars.

The duration of composition of each book presents some difficulties to the reader; the early chapters in each were written, and in some instances the findings were published, many years before the later sections, by which time the author's basic data had usually been extended or improved and his ideas had developed, often to a point at which they supersede the earlier approaches. The author does a little to help us, and so do the editors, but both could have done more to indicate the relation of different sections to each other.

Mathematically, apart from the theory of mapping by compact cells of equal population, referred to above, there is little which is new, but much of interest. In *Statistics of Deadly Quarrels* Richardson uses a variety of standard statistical approaches and develops a number of probabilistic models. In *Arms and Insecurity* he is concerned primarily with algebraic and geometrical solutions of the various differential equations, and this involves him in much matrix algebra. Here and elsewhere Richardson introduces several ingenious methods either for estimating or

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for assigning upper and lower bounds to parameters which cannot readily be evaluated explicitly. An electronic computer could greatly assist future extension of the work.

A feature of particular interest in both books is the surprising extent to which Richardson is able to discriminate between alternative explanations. Since there were only 92 wars of magnitude greater than 3·5 ending in the period 1820–1929 it might be supposed that they would fit plausibly to a wide range of theoretical explanations, but they do not. A particularly good example of this occurs in the study of the number of nations on each side of a war, outlined above. Richardson considers in turn 13 possible explanations of the observed distribution. Some are admittedly little more than exploratory approaches, to enable him and the reader to get the feel of the problem, both mathematically and politically, but not until the tenth of these theories does Richardson reach a simple formulation which gives tolerable agreement with the figures, although this theory deliberately excludes the small number of very complicated wars. The remaining three theories explore what modifications are necessary to the earlier formulation to enable it to incorporate the larger wars, and at the finish only 1 of the 13 theories is judged adequate to fit the numerical facts. This still does not, of course, mean that it is the only possible explanation. "It is well to remind ourselves that a theorem does not prove its converse."

Similarly it might be supposed that a large number of alternative mathematical formulations could provide statistically adequate explanations for the numerical facts associated with only two arms races. But again it does not seem to be so, although Richardson does not make it clear in this context what other basic theories he has had to reject. He does, however, emphasize that his guiding principle throughout has been that of Ockham's razor, namely to try first the simplest formulae that are not obviously wrong, introducing modifications only when the data call for them. The equations he derives are certainly simple, and they require so little fundamental modification, and there is sufficient ancillary evidence in support of them, for it to be hard to envisage how any radically different theory could fit the facts as successfully. However, Richardson would have been the first to object if its very simplicity encouraged us to accept his basic theory uncritically. Yet almost the only point which strikes me as odd concerns the trajectories found by Richardson in various contexts; all the points seem to move along one or other of the barriers, none to follow one of the curved trajectories. This may have no importance, but it is a little curious to find only straight lines where some curves at least would have been expected.

Both texts are lightened and illumined by the author's entertaining style, which is both humorous and profound, and particularly by passages of Socratic dialogue in which he discusses objections, brings out the limitations of a particular approach, or summarizes his findings. The following example was originally written in April 1939:

"CRITIC: I still don't like the fatalistic look of your mathematics. The worst disservice that anybody can do to the world is to spread the notion that the drift toward war is fated and uncontrollable.

"AUTHOR: With that I agree entirely. But before a situation can be controlled, it must be understood. If you steer a boat on the theory that it ought to go toward the side to which you move the tiller, the boat will seem uncontrollable. 'If we threaten', says the militarist, 'they will become docile.' Actually, they become angry and threaten reprisals. He has put the tiller to the wrong side. Or, to express it mathematically, he has mistaken the sign of the defence coefficient.

"CRITIC: But how can experienced statesmen possibly be mistaken about the sign of an important political effect?

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"AUTHOR: They are not altogether mistaken. They attend to the immediate effects of fear, and they ignore the after-effects, which are of the opposite sign, and in the long run more important."

The reader cannot fail to be impressed with Richardson's obvious enthusiasm for his subject and the scientific care with which he approaches it. "The first duty of a social scientist is to declare his prejudices. So I had better mention those of mine which are relevant to arms races. I have a prejudice that the moral evil in war outweighs the moral good, although the latter is conspicuous. This prejudice is derived from the Quakers, who brought me up. I am not ashamed of it; indeed it has been one of the two principal motives for writing this book. The other principal motive is my prejudice that scientific method is more trustworthy than rhetoric. I am not ashamed of that either." Richardson has clearly taken an immense amount of trouble throughout to avoid bias and meet objections, and to present his arguments and findings lucidly, so that his results would be both acceptable and intelligible. If he fails, it is more on grounds of intelligibility than acceptability, and the fault is probably more ours than his. Richardson had an extraordinary range of knowledge; he was particularly competent mathematically and in the practical handling of numerical data, and was unusually well informed on history, economics, geography and psychology. The chief obstacle to wider appreciation of his work would seem to be essentially the problem of communication in a complex subject involving so many specialties. At one point Richardson refers to the need to formulate "a relation between history and geography, mediated by mathematics", and reflects, rather sadly, that "the experts in any one of those three subjects seldom take any interest in either of the other two". In situations like this, however, the statistician should be better placed than any other "expert" to appreciate what Richardson was attempting and what he achieved.

I have considered the scope of these two books at such length because their subject matter is so important and a scientific approach to it so novel. They represent a quite remarkable pioneer achievement in a field which many would previously have regarded as not amenable to mathematical treatment; Richardson has shown unequivocally how much may be learnt from a mathematical and statistical approach to human conflict. *Statistics of Deadly Quarrels*, a comprehensive and highly ingenious descriptive analysis of past wars, reveals statistical regularities in human conflict which few observers would have suspected, although this knowledge unfortunately does little to further the author's desire to discover means of preventing future wars. *Arms and Insecurity*, however, is a more promising essay to this end. Again there are regularities, this time in the progress of arms races, which suggest inevitability. But Richardson insists throughout that he is describing only what would happen (and has happened in the past) if instinct and tradition were allowed to act uncontrolled—in fact, if people did not stop to think. Richardson's chief hope for this work was that it might help people in authority to understand the circumstances in which the deterioration of international relations would be progressive, and to recognize that these had an emotional rather than a rational basis, so that they might in future act more rationally—a visionary hope, perhaps, but none the less laudable.

The main question raised by this consideration of Richardson's work is therefore not whether, but in what way, his approach should be taken further. We are at present involved in a third major arms race; it is surely a matter of urgency to discover whether this is progressing according to the same basic pattern as previous arms races, and if so, whether Richardson's theories indicate a trend towards instability or

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stability. (Richardson himself made a start on this in *Sankhyā*—see Richardson, 1953).) It is necessary, too, for the potentialities of radically different theoretical systems to be explored; if we are not convinced that Richardson's linear equations represent a close first approximation to the mechanism underlying the regularities of past arms races, then the onus is now upon us either to demonstrate the shortcomings of his theory, or at least to provide a plausible alternative explanation for consideration alongside it. But certainly the most difficult problem, if we are convinced, is how to communicate this conviction both to peoples and their governments, so that the nations can avoid or "pull out of" potentially unstable configurations in the future. The first stage in this slow, perhaps perilously slow, process must be a wider appraisal by scientists of Richardson's work; for unless scientists judge and approve the findings of their fellows, they cannot expect non-scientists to accept and act upon those findings, whether the subject is the hazards of fall-out or of cigarette smoking or of arms races. And even then, emotional barriers often militate against the just appreciation and full acceptance of scientific findings, quite apart from their practical implementation.

It was a matter of great regret to Richardson during his lifetime that, despite his efforts, his work did not gain wider acceptance; many people must have dismissed it as a curiosity of no great importance and its author as a crank. The publication of these books should at least dispel this view by bringing his work to the attention of more people, both scientists and non-scientists; in addition their publication provides a basis from which others can advance the subject he has pioneered so diligently and impressively.

"CRITIC: So you have got your fine equations finished, have you?

"AUTHOR: Science is never finished. But I have at least got an approximation which is mathematically tractable and politically interesting."

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For publication details of the two books here discussed, see footnote on p. 473.

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Reviews

PAGE

1.—*Bartlett, M. S.* Essays on Probability and Statistics . . 484
2.—*Jeffreys, Harold.* Theory of Probability (3rd ed.) . . . 487
3.—*Alexander, Howard W.* Elements of Mathematical Statistics . 489
4.—*Naas, Josef and Schmid, H. L.* Mathematisches Wörterbuch 490
5.—*Morrison, Philip and Morrison, Emily* (Eds.). Charles Babbage and his Calculating Engines: Selected Writings by Charles Babbage and Others . . 491
6.—*Kulldorf, Gunnar.* Contributions to the Theory of Estimation from Grouped and Partially Grouped Samples . . 493
7.—*Chung, Kai Lai.* Markov Chains with Stationary Transition Probabilities . . . 494
8.—*Dynkin, E. B.* Theory of Markov Processes 495
9.—*Vajda, S.* Mathematical Programming 495
10.—*Bailey, N. T. J.* Introduction to the Mathematical Theory of Genetic Linkage . . . 496
11.—*Gibbs, J. Willard.* Elementary Principles in Statistical Mechanics Developed with especial Reference to Thermodynamics 497
12.—*Sorsby, Arnold, et al.* Refraction and its Components during the Growth of the Eye from the Age of Three . . . 497
13.—*Talbot, P. A. and Mulhall, H.* The Physical Anthropology of Southern Nigeria: A Biometric Study in Statistical Method 498
14.—*Jewkes, John and Sylvia.* The Genesis of the British National Health Service . . . 499
15.—*Cole, Dorothy, with Utting, J.* The Economic Circumstances of Old People 499
Lynes, Tony. National Assistance and National Prosperity 499
16.—*Gibson, Evelyn and Klein, S.* Murder: A Home Office Research Unit Report . . 500
17.—*McArthur, Norma.* Introducing Population Statistics . . 501
18.—*Bennett, Merrill K. and Peirce, Rosamund H.* Change in the American National Diet, 1879–1959 502
19.—*Aitken, A. C.* The Case Against Decimalisation . . . 503
20.—*Frisch, Ragnar.* Planning for India: Selected Explorations in Methodology . . . 504
21.—*Devons, Ely and Crossley, J. R.* The Guardian Wage Indexes . 506
22.—*Kuznets, Simon, assisted by Jenks, Elizabeth.* Capital in the American Economy: Its Formation and Financing . 506
23.—*Moore, Geoffrey H.* (Ed.). Business Cycle Indicators
Shiskin, Julius. Signals of Recession and Recovery: An Experiment with Monthly Reporting 508
24.—*Nurkse, Ragnar.* Equilibrium and Growth in the World Economy 509

1. Essays on Probability and Statistics. By M. S. Bartlett. London, Methuen; New York, Wiley: [1962]. viii, 127 p. 8½″. 21*s*.

"Survey articles and lectures usually receive scanty attention from reviewers and abstracters, being dismissed as expository, or even without any comment at all. Yet among the welter of new research contributions much represents temporary and even useless activity, which a little more general reflection might have avoided. Moreover, there is not much opportunity to indicate one's general philosophy and outlook in research

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papers. I have consequently risked collecting together the present set of lectures, given by invitation since the last war in various places, in the hope that out of an apparently somewhat miscellaneous bag a unity and coherence of statistical theory, wherever applied, will emerge."

This is the opening paragraph of the author's preface to the volume and we cannot improve upon this statement of his aim in presenting it to the world. There are eight essays: (1) Probability in Logic, Mathematics and Science; (2) Some Remarks on the Theory of Statistics; (3) Factor Analysis in Psychology as a Statistician sees it; (4) Stochastic Processes as the Statistics of Change; (5) The Statistical Analysis of Stochastic Processes; (6) Applications of Probability and Statistical Theory in Biology; (7) Irreversibility and Statistical Theory; (8) Statistical Inference.

Essays (1), (2), (4) and (8) are the least technical; many readers will have to refer to some of the original papers quoted in order to derive maximum benefit from the others. The first and last essays are particularly concerned with Professor Bartlett's views on probability and inference, though these are naturally in evidence throughout the book. We shall, however, postpone any comment on them until the end of this review.

The second essay was read to the Manchester Statistical Society in January 1954 and printed in its *Transactions*. It contains a historical survey, which stresses "the close link, in the early days of statistical developments, between statistics and social problems, especially if . . . we include the problems arising in vital statistics and human biology"—and also the importance of the study of probability theory which is regarded as the essential mathematical basis of statistical theory.

Then the relation of statistical theory to economic and social statistics is discussed. At that time there had been some controversy of the extent to which the methods of mathematical statistics should be taught to economists, and the author dealt with these issues with moderation, tact, and that luminous common sense—to quote Professor Greenwood's phrase—which, as Voltaire pointed out and Professor Bartlett himself reminds us, is not so very common. He did not hesitate to point out, when he read this paper, the importance, even to economists, of specification and inference, and appears to us—no mean achievement—to have held the scales fairly between Keynes and Tinbergen. This essay continues with some remarks of great interest on the problems of statistical teaching, and concludes with a section which foreshadows those questions of stochastic processes which were shortly to dominate his statistical thinking.

The reviewer found essay (3) extremely interesting because of his familiarity with factor analysis in earlier days. He could follow the general argument without difficulty, though he would have had to look up the original papers to follow all the mathematical details. Professor Bartlett's view of factor analysis seems to coincide with his own—it is essentially an exploratory technique, though the method of principal components is more empirical than methods which postulate a definite factorial structure. Even such definite structural models may be thought of as arising in different ways. One way was Godfrey Thompson's sampling interpretation of factors "in which he suggested that one might regard the mind as a synthesis of a large number of components or 'bonds' of which only a portion or 'sample' will be manifested in any particular test". "Such a statistical interpretation of factors would not rule out their possible usefulness, provided we realized their essential statistical definition and consequent limitations", and "their essentially statistical character is in fact not dependent on the above interpretation". Again, "While interpretations of factors extending beyond the statistical consequences so far confirmed are not in any scientific sense established, they may be a useful stimulant to further research." The reviewer would once more emphasize that factor analysis is an exploratory technique. If we are lucky in such investigations, the analysis may suggest a factor which is found to be highly correlated with some previously unsuspected objective criterion. If we are unlucky, like many explorers before us, we may get lost.

At this stage we may ask: "What is the unity and coherence of statistical theory which the author hopes will emerge from these essays?" There are two aspects to it. The first is

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clearly stated in essay (6), as well as elsewhere in the book. This essay is notable for the occasion of its delivery, the Third Soviet Mathematical Congress in Moscow, July 1956. "I should perhaps make it clear", the author says, "that by statistical theory I mean the application of the mathematical theory of probability to statistical aggregates, in the sense that has been admirably stated by Khintchine. . . . 'The science has for its main task the study of group phenomena, that is, such phenomena as occur in collections of a large number of objects of essentially the same kind . . . the discovery of such general laws as are implied by the gross character of the phenomena and depend comparatively little on the nature of the individual objects'."

The second is really the view of the universe as "Process". To the reviewer, who feels that the philosophy of Whitehead embraces more truth than any other he knows anything about, this point of view appeals strongly. The author's belief in the importance of developing and applying stochastic process theory is evident in almost every essay in the book. Essay (5) deals with the statistical analysis of stochastic processes emphasizing where such analysis differs from classical methods. The points involved are technical and do not lend themselves to summary. Essay (4) gives a particularly lucid account of the development of stochastic process theory and practice from the time of McKendrick and Yule onwards. It is pointed out that sequential sampling methods are themselves an example of stochastic process theory, since the determination of the average size of sample required in any application is solved by the same sort of distributional theory as is involved in "random walks" and diffusion processes. In an instructive example on the Monte Carlo method it is shown how the diffusion equation can be used to solve the classical problem of duration of play, or conversely how the actual results of 100 such games can be used to obtain an approximate solution of the diffusion equation. It is shown how the particular case of *stationary time series* theory can be applied to communication theory. Further examples are given from problems of queueing or machine waiting times and from problems of *sampling* a continuous output (for instance, cotton, wool or flax in industry, where it is important to maintain a uniform quality of yarn) and the problem of economic time series is briefly discussed.

Then comes a discussion of stochastic processes in biology starting with the fascinating problem of extinction of surnames to which is related the problem of mutation and fluctuation in bacterial populations. To the reviewer the most instructive example is an investigation by Monte Carlo methods of a fictitious school measles epidemic.

In essay (7) the conceptions of irreversibility and entropy are discussed in a way which should be a warning against any facile identification of entropy with "Time's Arrow". The paradox of Clerk Maxwell's daemon, it is claimed, has been resolved by Szilard who showed that in order to obtain information (in negative entropy measure) of amount I, it was necessary to increase the entire entropy of the system by at least I. Moreover, the maximum reduction in entropy of any sub-system possible by the use of this information was I. While the author believes that information in this general context must not be identified too glibly with negative entropy, he thinks Szilard's analysis established a close relation, or even identity, between them in at least the example he was discussing.

Nevertheless "if we regard the entire universe as an isolated system and try to associate an absolute time with increasing entropy, then such a time has no meaning for the universe as a whole". Professor Bartlett concludes, "If I were to sum up the broad conclusions from my discussion, it would be that statistical reversibility may impose a lower limit to the size of systems having an intrinsic time direction. At the other end of the scale, the irreversibility of time in terms of entropy definitions has no meaning for the whole (isolated) universe, but may have a more local space-time meaning for comparatively large systems, including human beings."

We return to the first and last essays. For use in statistical theory Professor Bartlett believes in representing the possible alternative fortuities by conceptual probability numbers which correspond to empirical frequencies. The correspondence consists in the fact that they may be combined in the same way. This leads to the additive axiom and the

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multiplicative axiom. The latter amounts to a definition of *conditional* probability. For a finite number of alternatives these follow at once; the extension to an infinite number involves the mathematical theory of measure. After the mathematical theory has been set up, its relevance is a matter for general consideration; in particular its theorems on conceptual frequency ratios in an increasing sequence of independent trials are tests of its appropriateness. On the theory there is a probability of 1 that the ratio r_s/n will tend to p in an increasing sequence of independent trials. We say that the result is "almost certain", whereas a probability of 1 corresponds to complete certainty when there are only a finite number of fortuities. We recognize that "almost certain" is logically different from "certain", but postulate that it is observationally equivalent. The problem of appropriateness of a probability model to the phenomenal world is then no different in nature from that of the appropriateness of any scientific conceptual model. It is the problem of the validity of induction. Professor Bartlett considers probability in the subjective sense of degree of belief but does not advocate its use in statistics. He would, I think, agree with the opinion expressed in a quotation from Jeffreys that to justify induction by a subjective theory of probability is neither necessary nor possible. I think he would agree, too, that a belief (conscious or unconscious) in the validity of induction and deduction is a necessary pre-requisite for all scientific work.

J. O. Irwin

2. Theory of Probability. By Harold Jeffreys. 3rd ed. Oxford, University Press, 1961. x, 447 p. $9\frac{1}{4}''$. 84*s*.

In order to appreciate the historical significance of Sir Harold Jeffreys's work on probability it is necessary to understand that the word "probability" has been used in various senses by various writers. There is not space here to discuss these senses in detail, but a few distinctions can be briefly indicated by merely naming them. (A fuller, but still brief, account is presented in *Science*, **129**, 443, 1959.) The most important distinction is that between *physical*, material, or intrinsic probabilities, chances, or propensities on the one hand, and non-physical or intuitive probabilities on the other. This distinction was clearly made by Poisson in 1837. Intuitive probabilities can be subdivided into *credibilities* = logical probabilities = unique rational degrees of belief or intensities of conviction on the one hand, and on the other hand subjective or personal degrees of belief to which some canons of consistency, honesty and maturity have been applied, in which case they are called *subjective* or personal probabilities.

From about 1925 to 1950, nearly all professional statisticians made official use only of physical probabilities. But during much the same period, there were others, such as Keynes, Ramsey, de Finetti and Jeffreys, who argued that no description of the use of physical probability could be given except in terms of intuitive probability, and that it is impossible to make direct use of a definition in terms of limiting frequencies of "successes". For, as Keynes said, "In the long run we shall all be dead." Ramsey and de Finetti were subjectivists, whereas Keynes and Jeffreys argued in favour of credibilities. The basic distinction between the philosophies of Keynes and Jeffreys was that Keynes regarded probabilities as "partially ordered", whereas Jeffreys assumes unique numerical credibilities to exist, with the aim of avoiding vagueness. He has by no means succeeded in attaining this aim, the reason being, in the reviewer's opinion, that some degree of vagueness is inevitable in the foundations of reasoning, of which the theory of probability is the main part. For an example of the troubles, see the top of page 188, dealing with the initial distribution of a correlation coefficient.

In the Preface of the second and third editions of his book, Jeffreys states that Keynes withdrew his assumption of partial ordering in his biographical essay on F. P. Ramsey. This is a mistake. What Keynes withdrew was his belief in credibilities. (See J. M. Keynes, *Essays in Biography*, 1933, 1951, "F. P. Ramsey, 1903–1930", pp. 239–252, esp.

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p. 243.) In the reviewer's opinion, each person must indeed make implicit or explicit use of partially ordered subjective probabilities, as the basis of his reasoning if any.

In order to apply a calculus of *credibilities* to statistics, it is necessary in each application to make an assumption concerning initial probabilities, the classical form of which is a Bayes–Laplace postulate of equidistribution, and which Jeffreys modifies in various ways. It is most instructive to see how, when selecting a rule for initial probabilities, Jeffreys arrives at his estimates by means of subjective judgements. That a man who makes a judgement is a famous personality does not change the fact that the judgement is personal.

A very familiar objection to Bayes's postulates, and to their modifications, is that a transformation of the parameter space can lead to a different postulate, and hence to an inconsistency. Jeffreys made a valiant attempt to overcome this difficulty by means of an invariance theory (1946), in which initial probability density is taken as proportional to the square root of the determinant of the information matrix. But he is not himself entirely satisfied with the theory, and is forced to use some *ad hoc* arguments when deciding whether or not to use the theory.

In a thorough-going subjective theory there is no obligation to make use of Bayes's postulates, so that the above difficulty does not arise. It might indeed be a psychological law that we often behave as if we divide up the parameter space into a minimal number of non-overlapping regions, in each of which the points are indiscernible by means of the apparatus at our disposal, and then attach equal probabilities to each region. (The number of regions is always finite in practice.) Such behaviour would be consistent in the sense that it would not be affected by a transformation of the parameter space.

In my opinion Jeffreys is one of the best living unorthodox philosophers of science. His arguments are often not as rigorous nor as lucid as they ought to be, but he has the great merit of having a practical and mathematical background, without which it is almost impossible to write usefully about the philosophy of probability, and he is witty and stimulating. Consequently his book is of greater importance for the philosophy of science, and obviously of greater immediate practical importance, than nearly all the books on probability written by professional philosophers *lumped together*. His was the first book which attempted to formulate a coherent theory of modern statistics, based on credibilities, and the first edition was thus a landmark in statistics. The new edition is not very different from the second one (1948). It is unfortunate that Jeffreys makes no attempt to deal with the philosophical developments of the last 12 years, being apparently satisfied to brush aside the theory of partially ordered subjective probabilities with his incorrect prefatory remark concerning Keynes's recantation. Pioneers often ignore the work of those who have stood on their shoulders.

As an example of a lack of rigour, Jeffreys argues (p. 29) that the following assumption is only a convention: "If, given p (a proposition), q and q' are exclusive, then the number (probability) assigned on data p to q or q' is the sum of those attached to q and to q'." An examination of the argument shows that the following postulate still remains: For some function, f, $f(P(q \text{ or } q'|p) = f(P(q|p)) + f(P(q'|p))$. It has in fact been proved (R. T. Cox, *Amer. J. Phys.*, **14**, 1–13, 1946; I. J. Good, *Probability and the Weighing of Evidence*, London, 105–106, 1950; J. Aczél, *Vorlesungen über Funktionalgleichungen und Ihre Anwendungen*, Basel and Stuttgart, 219–223, 1961) that this postulate follows from much more primitive assumptions. But in any case Koopman showed in 1940 that it is virtually impossible not to accept something like the usual axioms for intuitive probability, on the assumption that intuitive probability has a meaning at all. Even this assumption is not required in the behavioural approach used for example by F. P. Ramsey, L. J. Savage and C. A. B. Smith.

Again, on page 43, there is a very interesting argument which is intended to show that an inductive inference can approach certainty; formally that $P(p_n|p_1 \ldots p_{n-1}) \to 1$, where $p_1, p_2, \ldots$ are the results of experiments, all of which were "successful". But it seems somewhat inconsistent to deny the validity of a limiting definition for physical probabilities, and then to use a limiting argument for the justification of induction. The conclusion is

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that "repeated verifications of the consequences of a hypothesis will make it practically certain that the next consequence of it will be verified". In practice (after a very long but of course finite sequence of successes) this would not be true if the new consequence were known to be of an entirely different character from the previous ones. This important gloss seems to have been overlooked by Jeffreys, and I think most good professional philosophers of science would not have overlooked it. And yet the argument is worth more for the philosophy of science than some whole books on the subject.

On page 128, there is another argument concerning scientific induction. The hypothesis is considered that all animals with feathers have beaks. It is again argued that the probability will approach 1 as the number of successes, without failures, increases. The argument and the conclusion are both undermined by the fact that the distribution of essentially distinct species of feathered animals might be very skew, like a Willis, Pareto or Zipf distribution, or even worse, so that there would always remain species that had not been sampled until about half of the entire population of feathered animals had been examined. (If the rth commonest species had relative frequency, in the population, of $2^{-(r+1)}$, then the number of species represented in the sample would be only about $\log n$, where n is the size of the sample, and by the same token the number of genera might be only about $\log \log n$, and so not large enough for an application of scientific induction. The geometrical distribution is assumed here merely for the sake of argument.) It seems to me that a general law which does not itself refer to probabilities will not usually tend to become certain, however often it is verified. If the law could be qualified by saying that it is to be interpreted as applying only to experiments and observations of a similar nature to those already made, then it might well tend to certainty, but it is not easy to make this kind of qualification precise.

Although most of this review refers to philosophical matters, it seems worth while to repeat that the book contains a large number of detailed applications to mathematical statistics. But Jeffreys never forgets for long that every technique has both its foundations and its applications. As one would expect, some of the applications are to geophysics, and it is not surprising to find 12 index references to earthquakes, but there is no impression that the Earth has been dragged in.

One can forgive Jeffreys for the lack of reference to recent writers, since the hunting up of references can be very time-consuming, and his time can be put to better uses in advancing science. But it should not be forgotten that he has probably been influenced by these writers. For example, perhaps his interest in the possible application of the theory of types to probability might have stemmed from some comments of the reviewer in the above-mentioned book, and in *J. R. statist. Soc.* B, **14**, 107–114 (1952). We all find it easiest to remember our own work. Moreover, if Jeffreys had tried to bring the book up to date, rather than to revise it in part, we might have had to wait much longer for the present edition.

I. J. Good

3. Elements of Mathematical Statistics. By Howard W. Alexander. New York and London, Wiley, [1961]. xii, 367 p. 9″. 64*s*.

For a concise description of this book it would be difficult to improve on the blurb: "The principal intention . . . is to make mathematical statistics really accessible to the college senior or junior whose background includes a typical calculus course. The approach is basically instructive. . . ." When the author advises his readers to use an icosahedral die, he gives constructional details; when he illustrates the idea of a uniform distribution in terms of a spinning pointer, he includes sketches for making one: diagrams, analogues, examples and explanations abound. There can be no doubt that within its self-imposed limits the book does achieve a high standard of communication.

There is a good account of the basic standard distributions. The derivation of the distribution of a function of a given variate is clearly explained. The Central Limit Theorem

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is not shirked and there is even an outline of the multinormal distribution (subject to unstated "mild conditions" on the coefficients of the quadratic form).

The section on inference consists of an elementary introduction to confidence intervals and to maximum likelihood estimation—unfortunately, with no reference to the accuracy of the estimate, apart from the somewhat disproportionate attention paid to bias. The name of Bayes is conspicuously absent.

The book ends with an elementary and understandable account of the analysis of variance. There are throughout the book a large number of exercises, one half of them having answers provided. There is a good selection of tables—random numbers, the normal integral, t, χ^2 and F, and logarithms of factorials.

There is a short bibliography (in which, incidentally, the only British publication cited is Karl Pearson's 1900 paper on χ^2; Wilks's new book was evidently out in time to be advertised on the wrapper, but not for the bibliography, which cites it in its 1944 incarnation); and an index.

E. H. LLOYD

4. Mathematisches Wörterbuch. By Josef Naas and H. L. Schmid. Vol. 1 (A–K) xii, 1,043 p. Vol. 2 (L–Z) viii, 952 p. Stuttgart, Teubner; London, Pergamon Press: 1961. $10\frac{3}{4}''$. 800*s*.

These two volumes together contain about 2,000 pages, in double column, clearly printed in Roman type, comprising, at a rough estimate, about 10,000 substantive definitions, apart from numerous cross-references of synonyms. About 130 mathematicians, mostly German, cooperated with the main authors in preparing the definitions. The field covered includes theoretical physics and calculating machines, there being 82 branches of mathematics distinguished in a list of symbols at the end of volume 2. The major items give references to leading books or papers on their respective subjects. In addition there are about 400 short biographies of mathematicians who are no longer living.

So much for the statistics of this ambitious work. As concerns content and quality, readers of this *Journal* will be mainly interested in the group of terms classified as Probability, Statistics, Biomathematics, Games Theory, and Theory of Errors. The treatment here is somewhat mixed. To take a few specimens at random:

There is no biographical note on Karl Pearson. The "Pearson Differential Equation" is referred to the item "Classical Orthogonal Polynomials". There follows the "Pearson–Lee–Fisher Functions" which appear to be some of the functions required to estimate the point of truncation of a normal distribution; and a condensed but accurate account of "Pearson curves".

Under Fehler and its compounds we have, *inter alia*, two pages on classical theory of errors, a classification of "type of machine errors", items on the "Error Ellipse", the so-called "Propagation of Error", a reference of "Standard Error" (mittleres Fehlerquadrat) to the article on "Deviation" (Abweichung einer Approximationsfunktion), and an item on "Error of the Second Kind" (but not of the First Kind).

On sampling topics we have two entries under Stichprobe, referring for more extensive developments to an author called A. Chapter, Population Sampling, Washington, 1947. There is also a cross-reference to the Maximale–Stichprobenwahrscheinlichkeit–Methode (method of maximum likelihood) which is described as "ein auf Gauss zurückgehendes, von R. A. Fisher 1912 allgemein eingeführtes and von D. Dugué 1937 exact begründetes Verfahren". There are, however, entries under estimation, point-estimation, consistent, efficient and sufficient estimators. In one respect at least the German term is superior to the English, an unbiased estimator being described as "erwartungstreu".

The treatment on the whole seems well up to date. *Inter alia*, there are good articles on the Theory of Tests, Markoff chains and processes, and on various topics connected with Probability, Decision Theory and Quality Control.

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To review a work of this kind adequately is practically an impossible task. The labour and reading involved must have been enormous and disarm criticism. So far as I can judge, it is unlikely to tell statisticians much that they do not know already, but it would be useful to mathematicians working on the fringes of the subject; and, contrariwise, there is a great deal here which would be useful to the mathematical statistician who encounters new mathematical expressions in the literature, or has forgotten the meaning or precise definition of some of the old ones.

This book costs £40, which puts it outside the range of purchase by nearly all individuals and makes it a proposition for consideration by libraries only.

M. G. KENDALL

5. Charles Babbage and his Calculating Engines: Selected Writings by Charles Babbage and Others. Edited and with an introduction by Philip Morrison and Emily Morrison. New York, Dover Publications Inc., 1961. [London, Constable.] xxxviii, 400, 15 p. 8″. $2.

Charles Babbage is nowadays recognized as the pioneer of modern automatic digital computing machines. It came as a surprise to many of the early workers in electronic computing to find that the structure which they had built up on purely logical grounds for a digital computer was in fact precisely that which had been derived by Babbage over a century before. Because of this discovery, any account of Babbage and his work is always received with interest. The chief sources of information are, however, Babbage's own writings and those of his contemporaries and, not unnaturally, these have become increasingly rare.

"Passages from the Life of a Philosopher", by Babbage himself, contains some account of his scientific work, but mainly deals with his exploits as one of the great characters of Victorian times. The best record of his scientific work is "Babbage's Calculating Engines" edited by his son, Major-General H. P. Babbage. In addition to these works, the book, "Economy of Manufactures and Machinery", written by Babbage himself, contains a penetrating analysis of the impact of various forms of automation on the efficiency of production of goods, and might with advantage be studied by modern proponents of this art.

The rarity of these books may be judged from the fact that the reviewer has been attempting to obtain them for the past 20 years or so and has succeeded only in acquiring the "Economy of Manufacturers". It appears that only about six copies of the large volume, "Babbage's Calculating Engines", exist in this country, and it is pleasant to record that one of these is in the library of the Royal Statistical Society.

Against this background, it is obvious that the new Dover publication is likely to be one which is of importance and wide interest to all of those interested both in computing machines and in the history of Victorian science. The new book consists of a reproduction of about half of the chapters from "Passages from the Life" and of nearly the whole of "Calculating Engines". There is also an appendix of miscellaneous papers by Babbage.

The reprint of the two books is prefaced by a fairly long introduction by the editors. This is well written and contains an accurate and interesting account of the principal features of Babbage's work, and is followed by a note on the history of punched cards. This appears to the reviewer to be far less satisfactory. Many of the salient points, for example the work of Baldwin, receive no mention at all, and although, perhaps, punched card machinery is likely to play increasingly less importance in the field of computation, it is a pity that it is dismissed in two pages.

These introductory chapters are followed by chapters from the "Passages from the Life". The first chapter of the original is omitted, justifiably since it is chiefly concerned with the history of the name Babbage, which is unlikely to be interesting to the majority of readers. The following chapters, which are reprinted, contain an account of Babbage's childhood, boyhood and days at Cambridge, leading to a description of the way in which he

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conceived his first Difference Engine and to a short account of the Analytical Engine. It is only fair to say that Babbage's own accounts of his machines are so incomplete as to give little impression of the importance of his ideas. What is perhaps more interesting in these early chapters is the picture which Babbage gives of the state of mathematical study in Cambridge during the period of his young manhood.

After these essentially scientific reminiscences, the editors have chosen to reproduce two chapters which deal with scientific biography: descriptions by Babbage of his meetings with Wallaston, Davy, Rogers, Laplace, Biot and Humboldt. These are quite interesting but, to an Englishman at least, it seems a pity that the preceding chapters, which deal with the exhibition of 1862, the Prince Consort and the Duke of Wellington, have been selected for removal. The reviewer would strongly recommend readers who have tasted the style of Babbage in the present collection to refer to the original for these missing chapters.

After his scientific biography, Babbage continues with an account of what he calls "Experience by water", in which he describes his invention of a device to enable a man to walk in water and of a practical form of submarine. This is followed by "Experiences in fire", in which he first describes some experiments on himself, conducted in a large furnace to test the resistance of the human body to heat, and then tells of his descent into the volcano, Vesuvius, to investigate the flow of lava.

The next chapters in the reprint are those on picking locks and deciphering, and on railways. Unfortunately, in the former of these, few practical hints are given on the art of lock picking, but it is interesting to notice that Babbage was one of the pioneers of modern lock design since, having devised a means of picking existing locks, he went on to formulate the design for a lock which was unpickable by his system. His remarks on deciphering are more amusing than useful, and conclude with a discussion of the fact that "a bishop cannot be squared", whereas all other ranks of the church can!

Although not a railway enthusiast, the reviewer has always found Babbage's account of railways most amusing. It includes descriptions of journeys on the first of railways to be constructed, and also a spirited account of a contest between Babbage and Isambard Kingdom Brunel, on separate engines travelling in opposite directions on a single-track line. Perhaps, not unnaturally, the chapter on railways is followed by some hints for travellers which display a considerable shrewdness on Babbage's part, although the remarks on the design of a stomach pump are unlikely to find favour with the more squeamish.

The reprint continues with some of the "further contributions of Babbage to human knowledge". Tucked into this small section is a wealth of important material. For example an account of the motion of glaciers, a discussion of the economy of the book and parcel post, a statement about the transmission of Greenwich time signals and, of great importance and interest to statisticians, a section which is concerned with the automatic playing of games of skill. It is interesting to note that Babbage devised a scheme for the automatic playing of noughts and crosses which is almost precisely that used in recent times at the National Physical Laboratory. From the point of view of the statistician the really surprising part of this discussion is that Babbage, after showing that all noughts and crosses games can be derived from a limited set by means of symmetry operations, goes on to show how, by using as a random number source a machine statement of the results of previous games played, it is possible to produce games in sufficient variety to make it very difficult for a human being to realize that any mechanical procedure is being applied. So far as the author is aware, this is the first record of a mechanized Monte Carlo process which exists in the published literature.

It is perhaps ungracious to cavil over omissions when so much is given, but the reviewer felt deeply the removal of the chapter in which Babbage castigates street musicians. Admittedly this reveals more of his character than of his distinction as a scientist, but nevertheless it shows him as a human and likeable personality and, after the publisher's blurb in which it is stated that Babbage was "considered a crackpot by his contemporaries", which incidentally is untrue, it is a pity that this chapter which provides a basis for judgement should have been omitted.

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The second part of the new book reproduces "Calculating Engines", edited originally by his son. Here sketches of Babbage's thoughts set down by Menabrea are provided, and these are accompanied by notes by the Countess of Lovelace, surely the prototype of the glamorous woman mathematician of science fiction. No purpose is served by discussing in detail the material which is reproduced in these chapters, so suffice it to say that they give a clear picture of Babbage's priority in the field of the invention of calculating machines. They show, for example, one of the first programmes ever written for a computing machine, for the evaluation of Bernoulli numbers, and they also discuss the way in which the Analytical Engine itself was to be constructed. There is one omission from these chapters: a considerable passage consists of descriptions of certain drawings, but these are missing from the reprint; whilst, with considerable mental effort, it is possible to reconstruct them, it is not too much to expect the editors and their publishers to have given us the advantage of the originals. As an excuse it is mentioned in a note that some of the drawings are in unpublished collections listed in the original book. A scholarly service would have been rendered had the editors tracked these down and made efforts to reproduce them.

The book concludes with a list of some 80 of Babbage's papers and with a number of drawings which illustrate aspects of the calculating machines and of Babbage's so-called "mechanical notation". There is also the original table of contents from "Passages from the Life". It should be quite clear to readers that the reviewer is enthusiastically in favour of the present book. His grouses are merely on the grounds of omission, which after all could be justified by the extremely low cost of so valuable a book. It is certainly to be hoped that all those concerned either with the design of calculating machines, or with their use, will take advantage of this new book to read this history of a great pioneer. The book and its price can be unreservedly commended.

A. D. Booth

6. Contributions to the Theory of Estimation from Grouped and Partially Grouped Samples. By Gunnar Kulldorf. Stockholm [etc.], Almqvist and Wicksell, 1961. 144 p. 9″. *Cr.* 18.

This publication, written in thesis style, collects together some results on estimation from grouped samples taken from normal and exponential populations. The majority of these results concern the asymptotic behaviour of the "conditional maximum likelihood estimate"—the condition being that an estimate exists—in samples for which each observation is recorded only as lying in one of the intervals $(x_i, x_{i+1}), i = 0, \ldots, k$, where x_0 is the lower limit of the range of x and x_k is the upper limit of the range of x. An extension to samples for which the information is recorded partially in the grouped form and partially by recording the observation is considered in the last four chapters.

After an introductory chapter on the theory of maximum likelihood estimation from grouped samples in general, the author deals with the problem of estimation for a number of specific situations relevant to normal and exponential distributions. In particular he deals with the estimation of θ and α in the exponential distribution, $F(x) = 1 - e^{-\theta(x-\alpha)}$ $(x \geqslant \alpha)$, for those situations in which θ is unknown and α is known, θ is known and α is unknown, and where both are unknown. This work is extended to cover estimation from samples drawn from exponential distributions truncated at a known point. Also dealt with is the estimation of μ and σ in the probability density function,

$$f(x) = \frac{\sigma^{-1}}{\sqrt{2\pi}} \exp\left\{-\frac{1}{2}\left(\frac{x-\mu}{\sigma}\right)^2\right\},$$

where one is known and the other unknown.

The tabulated results on the optimum allocation of a fixed number of intervals are of some interest as they provide a theoretical background to the oft-repeated advice to the

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beginner to group his data into about 15 groups, usually quoted as between 10 and 20 groups. Little efficiency is lost by grouping into 10 groups, the optimum intervals are not of equal length but restricting the intervals to those for which $x_{i+1}-x_i$ is a constant for $i = 1, \ldots, k-1$, reduces the efficiency by an appreciable amount only when estimating σ. For completeness it would have been satisfying to have the simultaneous estimation of μ and σ included with cases μ known and σ unknown, and μ unknown and σ known.

This book is not likely to form part of the library of the practising statistician for whom the choice of grouping intervals will continue to be determined by considerations other than asymptotic efficiencies.

D. J. G. FARLIE

7. Markov Chains with Stationary Transition Probabilities. By Kai Lai Chung. Berlin, Heidelberg [etc.], Springer-Verlag, 1960. ix, 278 p. $9\frac{1}{4}''$. *DM.* 65.60. (Die Grundlehren der Mathematischen Wissenschaften, Band 104.)

Professor Chung is concerned with general properties of the Markov chain (i.e. a Markov process with at most a denumerable infinity of states) in both discrete and continuous time. By dint of making original contributions of a whole range of magnitudes, he has produced a thorough and systematic account of the subject.

The first 40 per cent. of the book is devoted to the case of discrete time ("parameter" is the word used but, as usual, the parameter space is one-dimensional and the conditioning values are the "past" ones). Sections 3–6 cover the classification of states, recurrence, criteria and examples, and the limit theorem expressing occupation probabilities in terms of mean recurrence times. This is the material familiar to all readers of Feller, but treated in greater detail. The author then generalizes the idea of first passage or first recurrence by considering the probabilities of transition from one state to another without passage through a "taboo" set of states. This concept leads to a number of imit theorems, for ratios of probabilities and moments of first entrance times. It also indicates a need for the Markov property to hold at random as well as fixed instants, the so-called "strong Markov property". Finally, in this part, the author considers functionals defined on the process, and the related questions of ergodicity, normal convergence and other limit properties of additive functionals.

The second part (continuous parameter) covers the same questions for the continuous time case, and also local properties of the transition probabilities and of the realization itself (e.g. limit values as one approaches a given time value from left or from right, behaviour of the "sets of constancy" of the realization and results on the number of times a given state is entered in a finite time). Many of the topics having analogues in discrete time are far less amenable in continuous time, at least at Professor Chung's level of rigour. This is true, for example, of the strong Markov property, and of the relations between quantities such as "occupation probabilities" and "first occupation probabilities".

The second part also contains a discussion of Kolmogorov's two systems of differential equations and their solutions, a treatment of the first infinity of a realization and a set of illustrative examples.

For all that it is a rigorous book, it is a readable one, particularly the first half. Professor Chung divides his presentation into sections of convenient length, and concludes each with a short historical discussion. He considers that rigour does not absolve him from the responsibility of pointing out the "reason" for the course followed, and there are many illuminating passages of explanation and preparation.

In his preface Professor Chung refers to the existence of "misgiving towards the modern rigorous approach to stochastic processes"; his scholarly and human book should go far towards allaying that part of the misgiving which is unfounded.

P. WHITTLE

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1962]

8. Theory of Markov Processes. By E. B. Dynkin. Translated from the Russian by D. E. Brown. Edited by T. Kovary. Oxford, London, [etc.], Pergamon Press, 1960. ix, 210 p. $8\frac{1}{2}''$. 60*s*.

This is a highly concentrated account of the analytic theory of Markov processes, the material consisting largely of the author's own work. The general level is fairly characterized on the jacket: "The treatment is very rigorous, and consistently analytic. It will appeal much more to pure mathematicians than to statisticians. A basic knowledge of measure theory and topology is indispensable for prospective readers."

The first chapter deals with the measure-theoretic preliminaries. The second sets the scene for Markov processes but also contains some novelties. Like Doob, the author emphasizes the random function itself rather than the conditional distributions but broadens this approach by simultaneously considering the functions for all possible initial times and conditions, obtaining what he calls a *Markov family* of random functions. The possibility that the process may end at some random instant is a standard feature of his descriptions.

Chapter 3 introduces the idea that a process may be modified to a *sub-process* by the demand that it cease at an earlier (random) instant of time than it otherwise would (for example, one could consider that the process ceases when the variable first takes a value outside some prescribed set, at the "first departure time"). If the probability that the process has not ceased by time t, given that it had not done so at an earlier instant s, is α_t^s, independently of what happens after time t, then for $s \leqslant t \leqslant u$

$$\alpha_u^s = \alpha_t^s \alpha_u^t$$

almost certainly, so that the idea of a sub-process can be coupled with that of a multiplicative functional of the process.

Chapter 5 contains the principal thesis of the work: the concept of a strictly Markov process, due to Doob. For such a process the familiar restriction on the form of the conditional probabilities must hold at randomly chosen instants of time as well as at given instants (such an instant could be, for example, an instant of first passage). The author develops sufficient conditions for validity of the strict Markov property. As an example of a process which is Markov but not strictly so he gives what is, essentially, a one-dimensional diffusion process with absorption at a single point.

The final chapter gives conditions for boundedness and continuity of the process and there is an addendum concerned with conditions for the measurability of instants of first departure.

The austerity of the text has as consequence that the 200 pages contain many more detailed results than I have indicated here, but also that these are accessible, and possibly also relevant, only to workers in a rather small and rarified field.

Professor Dynkin's work is obviously important and it is a pity that he devotes virtually no space to explanation of its motivation and implications. Despite this, the book will certainly be a standard reference for results on the continuous-variable process for some time to come.

P. WHITTLE

9. Mathematical Programming. By S. Vajda. Reading, Mass., and London, Addison-Wesley, [c. 1961]. x, 310 p. 9″. 68*s*.

With one reservation, this book is wholly admirable. Its great strength is the effortless way in which it leads the student right up to the frontiers of the subject. From the algebra of linear inequalities and duality it passes to the theory of graphs and combinatorial theory. The chapter on general algorithms covers simplex in all its variants of inverse matrix, dual simplex, bounded variables, cross-section, etc., and briefly expounds multiplex. "Special Algorithms" includes the familiar transport problem and many related topics. The "Selected Applications" are well chosen to demonstrate the range of different problems

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which have been considered in recent years. The remaining chapters cover parametric, discrete, stochastic, non-linear and dynamic programming. Each is up to date and illustrated with well-chosen examples both in the text and as exercises for the student.

The reservation is that, though the book is designed as a text book for a graduate course in mathematical programming, it restricts itself to "fairly elementary mathematics". It is not clear why the author should have denied himself the use of matrices—presumably the idea is to make the subject accessible to non-mathematicians. Speaking as one who came to the subject from that position, I feel that the easiest approach is to grasp the nettle of the elementary matrix theory required. As a mathematician himself, Dr Vajda may not realize that pages of summation signs and subscripts can be more forbidding than the more elegant matrix notation. But there is no doubt that Dr Vajda is experienced and particularly successful in the teaching of mathematical programming, and his approach will surely appeal to some.

Quite apart from its merit as a text book this is an invaluable book of reference. This is Dr Vajda's fourth book in the field of programming, and I am sure we can count on him to keep up the good work. It may be too much to hope for, but a regular series of supplements to this latest work would provide a painless way of "keeping up with the literature".

A. H. LAND

10. Introduction to the Mathematical Theory of Genetic Linkage. By N. T. J. Bailey. Oxford, Clarendon Press, 1961. x, 298 p. 9½". 55*s*.

Since the advent of television and the New Scientist every schoolboy knows about DNA, the material genes are made of, and which transmits developmental information from parent to fertilized egg. However, in most organisms, including Man, we are still a long way from knowing the chemical fine structure of the genes and are still obliged for practical purposes to regard the gene as an irreducible atom of inheritance much as Dalton or Avogadro regarded the atom of physical chemistry. These practical purposes are, however, not of trivial importance. The genes do not constitute a totally random assemblage but are carried in an orderly structure possessing a definite topography. They are in fact transmitted in separate groups (the "linkage" groups), as a result of their being located on the thread-shaped chromosomes (protein structures to which the genic DNA material is attached). Usually in their passage from one generation to the next the order of the genes in a linkage group is not scrambled. Consequently with appropriate experimental techniques a "map" of each linkage group (i.e. chromosome) can be made. The success of these techniques depends, of course, on the fact that although order is preserved during the formation of germ cells, the chromosomes which exist in the parent in homologous (or quasi-duplicate) pairs are apt to break and rejoin, criss-cross fashion, prior to entry into the germ-cell. The distance between two identifiable marker genes is inferred statistically from the amount of observed crossing-over.

Apart from the pleasure resulting from satisfaction of the age-old impulse to make maps, chromosome mapping has many less purely academic applications. The dynamics of population evolution can in some cases only be understood if we have accurate knowledge of the relative locations of the genes operative in natural selection. Thus the present research of Professor Thoday and his co-workers in Cambridge on population genetics involves mapping techniques. Again, in medical genetics the knowledge that the gene for a congenital defect was at a certain "distance" from a marker-gene (e.g. a gene for eye-colour or blood antigen) would be invaluable for genetical prognosis.

A genetic map of an animal or plant is the end-product of an involved process of experimentation and inference from the data. Consequently it involves sound experimental design, statistics and mathematics. It is possibly not entirely coincidental that the greatest innovator in modern statistics, R. A. Fisher, has had a life-long interest in genetics which possesses a more developed mathematical basis than any other biological science.

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The first three parts of Dr Bailey's book are concerned with problems of mapping in experimental organisms largely as developed by Fisher and his co-workers at Cambridge in the post-war period—a school of research to which both Dr Bailey and the reviewer belonged. Dr Bailey's treatment is both extremely clear and comprehensive, assembling in a lucid and attractive presentation all important methods and results in this field. His authoritative book is therefore excellent both for study and reference as many research workers will find. The inclusion of such new topics as polysomic inheritance, affinity, and bacterial genetics is noteworthy. At its higher levels the mathematical theory of mapping leads us into the study of stochastic processes basically identical with renewal theory, but requiring to be treated *suis generis* to obtain results appropriate to the subject matter. Dr Bailey has made important improvements in the mode of proof of many results and is to be congratulated on the elegance and ingenuity of his presentation and methods.

The remainder of the book deals with linkage in humans where data are observational and not experimental. Dr Bailey's wholly admirable account owes much to his own researches in unifying and extending methods developed by earlier workers such as Fisher and Finney. Those readers who are interested in the theory of statistical inference will appreciate Dr Bailey's lucid account of the theory of "odds" and of tests based on the likelihood ratio as developed at London by Haldane, Smith and Barnard.

A. R. G. Owen

11. Elementary Principles in Statistical Mechanics Developed with especial Reference to Thermodynamics. By J. Willard Gibbs. New York, Dover Publications; London, Constable: 1960. xviii, 207 p. 8″. $1.45.

Very little needs to be said about this classical volume on statistical mechanics (now issued in a paper-back), which has provided one of the bases of work in this field for more than half a century. Although it was accessible in Gibbs's Collected Works, the cheapness of the present volume (presumably made possible by the fact that it is in content identical with the original 1902 edition) will make it readily available to a new generation of students.

This availability is especially welcome, for apart from a somewhat turgid literary style, and an irritating number of footnotes, the book compares favourably with many more modern treatises on the subject. In particular the author's treatments of the idea of the ensemble (which now bears his name) and of the relation between probability and entropy are models which later authors might well have followed more closely.

F. Downton

12. Refraction and its Components during the Growth of the Eye from the Age of Three. By Arnold Sorsby, B. Benjamin and Michael Sheridan, with the assistance of Janet Stone and G. A. Leary. Medical Research Council. London, H.M. Stationery Office, 1961. viii, 87 p. 9¾″. 9*s*. 6*d*.

How much is sight worth? Only a blind person could answer that. This study set out to determine some of the fundamentals of the mechanism of the eye. Other characteristics of the human body have been measured endlessly, the development of the human frame is now pretty well understood but measurement of the development of the eye has presented difficulties. The measuring procedures used in this survey were intricate and have only been made possible by recent advances in photography, especially in the speed of camera film.

The study was mainly of children from the age of 3 to 15 years with some small additional data from young servicemen. The eyes of 1,432 children and 98 R.A.M.C. recruits were measured and this was no mean task; as well as the intricacy of the methods used there was considerable computational effort involved in the calculation of the optical dimensions of each eye from the experimental data.

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Essentially this was a cross-sectional study though some limited data were available by which to assess the rate of change of the different components of the eye and their co-ordination. The data are given adequate statistical treatment without elaboration and, therefore, should be comprehensible to the interested readership—the medical profession.

A. T. GORE

13. The Physical Anthropology of Southern Nigeria: A Biometric Study in Statistical Method. By P. A. Talbot and H. Mulhall. Cambridge, University Press, 1962. xvi, 127 p. $9\frac{3}{4}''$. 32*s*. 6*d*.

This is a strange book, wrapped in successive layers of curious but not unpleasing unreality. To start with only the first 9 and last 50 pages have anything to do with the title: in between is an elementary account of statistical procedures, up to but not including analysis of variance. The last and most interesting portion applies the D^2 statistic to the solution of the problem of classifying population groups in Southern Nigeria by anthropometric means.

The book had its origin when the means and standard deviations of 12 measurements, taken in the 1920s by Dr Talbot, on some 7,000 Nigerians belonging to 50 or so supposedly distinct population groups arrived in Cambridge after Talbot's death. On the subsequent arrival of a mathematician, Mulhall, from Australia, these data were said to be "ideal" for the purpose of "investigating certain problems in multivariate analysis" despite the fact that all the original records were lost and no covariance calculations existed or, presumably, could be made. Thus the D^2 computations were all carried out on a different sample of measurements of 1,700 Nigerians, made by Dr G. I. Jones and published by him and Mulhall in 1949. The correlation matrix of this sample was then applied to the older one.

It seems from internal evidence that this work was done in 1952, or thereabouts, and presumably that is why the authors do not refer to Hiernaux's work in Ruanda Urundi, published in 1956. The difference in orientation between that and the present work can hardly go unmarked. Scarcely a hint of biology obtrudes into "The Physical Anthropology of Southern Nigeria"; it is an essay in pure classification. The Coefficient of Racial Likeness, thought by many to have disappeared for good a long time ago, is calculated and shown to be every bit as fallacious as mathematical analysis leads one to suppose. The D^2's are calculated, it would seem, on a desk machine. This part of the book will be of much interest to those versed in the anthropology of Southern Nigeria, but scarcely to the general run of human biologists.

The larger part of the monograph, however, consists of something like a text book of statistics, written with clarity and succinctness. Mr Mulhall describes and illustrates how to calculate means, standard deviations, standard errors, regressions and correlations, and the D^2 statistic. Though he writes a good chapter on inferences from samples, he uses n as the divisor in calculating the standard deviation (p. 22) and remarks (p. 42), when discussing the significance of a difference between two means, that "more refined tests, based on Student's t-distribution, will not be required in this study". In general, however, the approach is sound, but the reviewer fails to understand for whom this part of the book is written. The complete absence of tables of t, F, etc., alone make it unsuitable as an introductory text, and in any case no human biologist of the modern school could be content with what is given here; he will inevitably have acquired a greater knowledge of biological statistics before he embarks on any research. As additional rapid reading for the student concerned with anthropometric material, however, it can be recommended.

In short, this is a most worthy book; the fruit of a prodigious work by Dr Talbot and his wife, and much care and persistence both by Dr Trevor of Cambridge and by

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Mr Mulhall. Though the rapid progress of biology as applied to human populations has left it behind the advancing front of research, it is a handsome late arrival amongst the source documents upon human morphological variability.

J. M. TANNER

14. The Genesis of the British National Health Service. By John and Sylvia Jewkes. Oxford, Blackwell, 1961. xii, 67 p. 8½″. 6*s*. 6*d*.

Professor and Mrs Jewkes have studied the medical systems of Britain, the United States, Canada, New Zealand and Switzerland which constitute, they suggest, a fair sample of Western Countries. The present publication is, therefore, the first of a series of essays that they anticipate publishing on the economic and administrative aspects of medical systems. They warn the reader that since this is the first and will therefore be read in isolation, it may seem unduly critical of the British National Health Service.

Critical the pamphlet certainly is, but it is also provocative and many a reader who will study it with interest will, for good measure, find no lack of things to criticize. The statistician will naturally turn to the appendix to which almost all the authors' evidence is relegated, reference to the appropriate tables being made in footnotes. Following the authors' comments, he will find that, per 1,000 of population, the U.S.A. has more doctors and more dentists than the U.K., but fewer nurses and fewer hospital beds. He will have no difficulty in accepting the Jewkes's advice that "such statistical exercises must always be interpreted with caution" and he will note their reminder that the *quality* of care is not brought into the equation. He may well have difficulty in seeing how from these differences the Jewkes's themselves can conclude that "the average American now has more medical services than the average Briton". Do the problems of medical care and services yield to such very simple and differently distributed measures? It seems very doubtful.

Other comparisons are made with Switzerland and with the pre-war services of our own country. "If it is true", the authors conclude, "that the British people embarked upon the experiment of the National Health Service in the belief that their existing medical facilities were noticeably inferior to those found in other countries, that British medicine had failed to provide widespread service in time of sickness to the whole community and that administrative changes of themselves could produce miracles, then undoubtedly they were the victims of illusion."

Underlying this broad conclusion, the pamphlet contains (a) 30 statistical tables of expenditure on medical services, numbers of doctors, beds, etc., the distribution of consultants, indices of the health of the nation, and so on; (b) many quotations with critical commentary; and (c) a closely reasoned argument. They all need to be read in close association and with thought.

A. BRADFORD HILL

15. The Economic Circumstances of Old People. By Dorothy Cole, with J. Utting. Welwyn, The Codicote Press, [1962]. 139 p. 8½″. 12*s*. 6*d*.

National Assistance and National Prosperity. By Tony Lynes. Welwyn, The Codicote Press, [1962]. 55 p. 8½″. 6*s*.

These two pamphlets, Nos. 4 and 5 in the series of Occasional Papers on Social Administration, deal with the living conditions of a section of the population whose incomes fall well below the national average. Mr Lynes is concerned with recipients of national assistance, and traces changes in scales paid since 1948 comparing them with changes in the cost of living and in disposable real income per head. The principal conclusion is, that if changes in scales are compared with a special price index for low-income households, changes in rates of national assistance have more or less kept pace with increases in the cost of living, and from 1958 to 1960 there has been some improvement in

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real terms. Thus, rates of 45*s.* in 1958 and 1959 and 50*s.* in 1960 compared with 42*s.* 9*d.*, 43*s.* 0*d.* and 43*s.* 8*d.* respectively, which would have been necessary to maintain the same purchasing power as the rate of 24*s.* in 1948. Mr Lynes argues, however, that a "fair" increase in national assistance rates should take account also of the fact that between 1948 and 1960 disposable income per head (excluding housing expenditure, for rent allowances are given by the Assistance Board in addition to scale rates) increased by a further 38 per cent. On this reckoning, a "fair" rate of assistance would be 60*s.* 3*d.* for a pensioner in 1960.

Mr Lynes's criterion of "fairness" is based upon certain views on social policy. He implicitly rejects the idea of a level of subsistence, defined in terms of a fixed amount of goods and services below which assistance would not fall. As society becomes more affluent, so the level of subsistence would have to increase. It is, of course, possible to have different views on social policy but Mr Lynes provides figures which would enable scales to be calculated on a number of different assumptions, or criteria.

Miss Cole and Mr Utting present an interim report on a survey of the conditions of pensioners carried out by the Department of Applied Economics at Cambridge with the help of the Nuffield Foundation. As the full analysis of the 1,078 interviews will take a long time to prepare, they present the results of the first 400 received. The sample was one of seven areas, not randomly selected, and the questionnaires relate to six of them: within each area a random sample of addresses was selected, and all persons of appropriate age found at these addresses were interviewed. The account is mainly a straightforward description of the circumstances in which the old live, and the main impression one receives is of the variety of conditions. There are, of course, a large number living at the margin of subsistence. Of the income units in the sample 60 per cent. received £5 or less per week, against 19 per cent. of the general population. Over one-quarter were solely dependent on government pension, and a further 16 per cent. primarily dependent, defined as having less than £1 per week in addition to their pension. There was considerably more poverty among female pensioners than among males. One-quarter of the women had less than £3 per week. A valuable finding of the survey relates to the fact that a considerable number of persons who were entitled to national assistance did not in fact receive their entitlement. In their sample the authors found 12 per cent. of the total in that position.

Among other matters dealt with are the assets held by old people, the structure of households in which they live, their health and social contacts. The full report of the enquiry will be awaited with interest; it is to be hoped that it will contain a more extended analysis of sampling errors than does the interim report.

E. GREBENIK

16. Murder: A Home Office Research Unit Report. By Evelyn Gibson and S. Klein. London: H.M. Stationery Office, 1961. iv, 43 p. $9\frac{3}{4}''$. 4*s.*

Much of the research conducted by Government bodies is fated to remain unpublished. This is unfortunate both because it deprives outside workers of the opportunity of using the results obtained by their civil service colleagues and because by this concealment the professional civil servant is partly deprived of status in his profession and of the stimulus of writing for a public audience. The existence of the Home Office series of Studies in the Causes of Delinquency and the Treatment of Offenders is to be welcomed as providing some evidence of the work done by the Home Office Research Unit. The report under review is the fourth to appear in the series, the first having been published in 1955.

Murder is an attempt to review trends in this type of crime, particularly in relation to the passing of the Homicide Act, 1957. This Act made two important changes in the law: first, it introduced the distinction between capital and non-capital murder (the death penalty being retained only for the former), and second, it introduced the defence of diminished responsibility as a result of which a person may be convicted of manslaughter rather than murder. The latter change has had a marked effect on the statistics of murder because it

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1962]

affects some 20 to 30 cases a year out of an annual total of between 150 and 170 homicides a year.

The report is concise and covers all the obvious points which a straight documentary study could be expected to consider. Its most significant findings are as follows: first, that the increase in murder since the war has been very small compared with the increase in other forms of crime. Second, the proportion of deaths finally recorded as murder has fallen, owing to the introduction of the defence of diminished responsibility. (We have, unfortunately, to take the authors' retrospective classification of pre-Homicide Act cases of diminished responsibility on trust. The report falls below the admirable in indicating neither how this classification was done, nor on what information it was based.) Third, the ratio of capital : non-capital murders was unchanged as a result of the Act (once again the basis of the pre-1957 estimates is not stated). Fourth, the proportion of those convicted found to be mentally abnormal was virtually unchanged between the periods before and after the Act, the figures being 47 per cent. for 1952–56, and 46 per cent. for 1957–60.

There is a useful analysis of the victims which mainly confirms what was already known to criminologists. It is important, however, to bring to public knowledge certain facts, such as that three-quarters of all child victims were murdered by their parents or older relatives. In recent years 61 per cent. of victims were women, mostly murdered by their husbands or lovers. The majority of male victims, on the other hand, were killed by strangers. One disquieting feature was that the proportion of men killed for robbery or financial gain rose from about 8 per cent. to 20 per cent. Partly related to this was the marked increase in the number of those convicted and sentenced for murder who also had previous criminal records (43 to 60 per cent.), indeed the *whole* of the increase in the annual average number of men sentenced for murder was attributable to those with criminal records.

The authors comment that the Homicide Act appears to be working in that the capital murders appear mainly to be committed by professional criminals in the furtherance of criminal activities, but they do not refer to another obvious conclusion, namely that the "unique deterrent" of the death penalty was reserved for a form of murder the frequency of which appears to have increased *more* than that of those murders for which it was not retained. Perhaps, however, Government departments must preserve due discretion in such matters.

It is regrettable that a valuable report should bear signs of hasty proof reading: there are mistakes in addition in Table 10, an extra 10 cases of strangulation or asphyxiation by insane offenders appear as raw numbers in Table 34 (though the percentages are correct), the total number of male offenders after the Homicide Act given in Table 29 is inconsistent with the data given in Table 27, while it is also impossible to reconcile the figures for reprieved and H.M.P. cases shown in Table 30 with those given in Table 10. Too much weight, however, should not be attached to these inaccuracies as they do not affect the conclusions of the report, which itself is a useful contribution to the subject.

J. P. MARTIN

17. Introducing Population Statistics. By Norma McArthur. London, Oxford University Press, 1962. xiv, 137 p. 8½″. 37*s.* 6*d.*

This book is published under the auspices of the South Pacific Commission and Australian National University and it is primarily intended as a guide to people in the South Pacific engaged in population work, i.e. it is written to inform and assist officers of Pacific Island administrations called on to engage in demographic work as part of their normal duties. In contrast to many purely theoretical books this manual has a welcome and appealing practical flavour because it is clearly a concentrate of Miss McArthur's own extensive experience of population problems arising in the administration of the Pacific Islands.

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There are four chapters: Population Structure (sex and age distributions and the effects thereon of fertility, mortality and migration); Measures of Change (mortality and fertility factors as indicated in both cross-sectional and cohort studies); Censuses and Surveys; Uses and Abuses of Population Statistics.

On these matters the book provides a lucidly written account of elementary principles and it is certain to provide valuable help in an area of the world where population difficulties abound but where for lack of sufficient experts the local administrators have, perforce, to grapple with them on their own. Even where experts are available the book is still likely to fill a gap; for there must be adequate communication between experts and administrators and the latter will need to learn the language of the former.

There are a few places where the going is made a little difficult for the novice. In the first chapter there is an unnecessary reference to the chromosome theory of sex determination. The introduction of the median age seems too to be of doubtful value. On the other hand, there is a most interesting account of the effect of the measles epidemic in Fiji in 1875. Chapter II deals effectively with consistency between census and vital registration data but there is confusing reference to the extension of the simple $\sqrt{(pq/n)}$ concept of sampling error to the testing of the difference between two rates as $\sqrt{\chi^2}$. The life table is referred to, as also is the difference between age death rates and probabilities of dying within a year of age, but there is no explanation of how to calculate the latter from the former. The section on standardized mortality rates would be clearer with a symbolic representation and if the arithmetical examples were more explicitly set out in work-sheet form. In dealing with reproduction rates the mixture of cohorts involved is mentioned but it needs to be emphasized much more strongly. The chapter on surveys is well done though some 20 pages are directed to the mere mechanics of tabulation which are not peculiar to population work. Chapter IV is very good indeed on uses but very weak on abuses; a better title would be "How can population statistics be put to work to help social administration?" Such a title would be a better tribute to the book as a whole and to the author's approach.

B. BENJAMIN

18. Change in the American National Diet, 1879–1959. By Merrill K. Bennett and Rosamund H. Peirce. Stanford University Press, 1961. 95–119 p. 10″. $1. (Food Research Institute Studies, II, 2, 1961.)

The United States Department of Agriculture has estimated the quantities of food moving into consumption at retail level annually since 1909. The authors have prepared comparable estimates for 1879, 1889 and 1899, and will supply details of their methods on request. During the 80 years under review, money incomes rose thrice as fast as food prices, but consumption declined from about 3,750 calories per head per day to under 3,200, the present rate of decrease being about 10 cal. per annum. The corresponding fall in product weight was smaller, from 1,615 lb. per capita per annum to 1,475 lb. Alcoholic drink may have contributed an additional 100 cal. under Prohibition and up to 200 cal. at other times.

Nutritionists would say with some assurance that in recent years actual food ingestion in the United States has been about 2,500 cal. daily. In the nineteenth century, when work was harder, mechanization in its infancy, homes less well heated and more of the population exposed to cold weather in winter, the level of ingestion was doubtless higher; but, throughout the period, between 600 and 1,000 cal. intended for human consumption appear to have been wasted, spoiled or fed to domestic pets, the current level being 700 cal. These figures appear to the authors "almost incredibly large". Greatly daring, they question the basic assumption that ingestion is no more than 2,500 cal., and call for enquiries on this point, as well as on the volume of non-ingestion and its components. Alas, direct enquiries on household food wastage are scarcely practicable; under observation, the housewife at once changes her behaviour, even if she herself is the observer.

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Further, is wastage of the order of 20 per cent. really incredible in the world's richest country?

Throughout these 80 years, animal products have contributed 1,200–1,300 cal. a day; the decrease has been in vegetable products, from nearly 2,500 to about 1,900, or from 66 to 59 per cent. of all calories obtained. The decline in calories from vegetable sources is more than accounted for by the fall in starchy foods, from 2,000 cal. (53 per cent. of the total) in 1879 to about 760 in 1957–59 (24 per cent.). This massive shift in American consumers' habits was facilitated by the rise in real incomes; price changes have not helped, for since 1890 cereal foods and potatoes have become relatively cheaper compared with most of the foods which have displaced them. The decline in maize was under way by 1879; it extended to wheat flour and bread by the end of the century, to potatoes after 1910 and to other grain products in the thirties. The trend is fairly regular, and by 1970 no more than 20 per cent. of calories may come from starchy foods; say 620 out of 3,075, compared with 815 from the meat group.

For several food groups the trends since 1930 have been markedly different from those in the previous half-century. Beef and veal, after losing ground to poultry in the twenties, recovered to a new high level, while pigmeat is slowly losing its old predominance. Here again price movements are not helpful in explaining these long-term changes in consumer preference. From 1940 onwards demand shifted from butter to margarine (a much improved product) and other vegetable fats. Total consumption of visible fats has tended to level off since 1930, but because of the continuing fall in total calories some 42 per cent. of the energy value of the American diet now comes from fat, compared with 32 per cent. 50 years ago. Sugar consumption reached saturation level about 1930, but milk consumption is still slowly increasing. Fresh fruit and vegetables have barely held their own, but processed varieties have found favour—first dried, then canned and latterly frozen. Purchases of coffee reached their peak around 1950, while those of tea have remained small.

The net effect of all these trends has been to reduce the carbohydrate and vegetable protein content of the diet, while fat and animal protein have increased. Calcium, iron, vitamin A and the more important B vitamins have risen, though not vitamin C; but some of these gains have come from additives, especially in bread after 1940, and cannot be credited to consumer choice.

Making large allowances for the roughness of the estimates, which the authors admit, the conclusion seems inescapable that long before America became the prototype of the affluent society, it was by the standards of most of the world a land of plenty. Averages for the United States as a whole may, however, conceal wide disparities between classes and regions, as in the United Kingdom. The national averages for Great Britain, which have long tended to follow those for the United States, would not themselves indicate the grinding poverty which Booth found in London and Rowntree in York.

Britain solved the problem of primary poverty between 1940 and 1944, as a by-product of the war effort; the National Food Survey records for this vital period remain largely unanalysed, and although they relate mainly to the urban working class it is of some importance to social historians that they should be retained for future reference. Now America's present diet is a guide to our future prospects, and the gap is nearer a decade than a generation.

A. H. J. Baines

19. The Case Against Decimalisation. By A. C. Aitken. Edinburgh and London, Oliver & Boyd, 1962. 22 p. $8\frac{3}{4}''$. 2*s*. 6*d*.

Professor Aitken's pamphlet is a very welcome return to the custom, which has so regrettably lapsed, of eminent experts writing pamphlets on current social problems of which they have special knowledge. The steps that the Royal Statistical Society has begun to take in connection with problems of food supply and of transport are in the same

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direction. The increasingly complex and technical character of social problems makes it most important for the survival of effective democracy that technically qualified persons should act in this way. It is very much to be hoped, from this point of view alone, that the initiative of the author and his publishers will be rewarded by a wide sale. Not that anyone who buys this pamphlet need feel that he is being altruistic. He will receive full value in literary style, education and even entertainment in the best sense of the word.

Professor Aitken argues that the proposal to decimalize the currency should be resisted mainly because it will be a step away from the more rational duodecimal system which is at least in partial use in our measures, weights, and coinage already. He proposes a new paper unit, the Royal, worth 12 shillings, a gross of pence. "The half-crown might stay for a while, but eventually might be superseded by a three-shilling piece, a 'quarter', easier than the half-crown to distinguish from the florin." He gives an outline history of development of our current system of notation which we would query at only two minor points. He seems to attribute the positional system to the Hindus but this was surely in use already by the Babylonians themselves; and the earliest invention of zero would seem to have been by the Minoans, who used a cross for filling in tax returns in much the same way as a stroke is used nowadays to represent no income in a given category.

In concentrating on the internal history of mathematical notation, Professor Aitken has not fully brought out the connection between this and social history in general. He refers to the proscription of Arabic numerals in fourteenth-century Florence, and to the fact that the change to a decimal currency, imposed in France in 1795, was a direct result of the French Revolution; but the general acceptance of changes in the symbols we use to express our thoughts sometimes seems to need the ferment of ideas occasioned by a social upheaval. Aside from the effects of the French Revolution, one may cite the improvements in the Russian alphabet and calendar after 1917 and the steps now being taken towards simplification of Chinese.

It is only in this respect that the reviewer would disagree with Professor Aitken. There is no question but that the duodecimal system has all the advantages he claims for it, and its ultimate general adoption is almost certain. The present reviewer feels, however, that the route to this probably lies *via* a decimalization of the British currency, followed by duodecimalization of world currency—world government being an even more urgent need than rationalized numeration. The best hope would seem to be for the International Congress of Mathematicians to resolve that henceforth all children throughout the world should be taught to calculate in both the decimal and the duodecimal system. Such a measure would increase world numeracy, in the sense of an understanding of the nature of symbolism and formal reasoning, and could have profound effects for good in many directions. The adoption of the measures advocated by Professor Aitken would then be a natural consequence.

G. A. Barnard

20. Planning for India: Selected Explorations in Methodology. By Ragnar Frisch. London, Asia Publishing House; Calcutta, Statistical Publishing Society: [1960]. iv, 86 p. 11″. 35*s*.

This volume contains a selection from the working papers produced by Professor Frisch for the Indian Statistical Institute during the period when the Second Five-Year Plan was being discussed. It leads up to a "plan-frame" to be applied to Indian data "organised so as to conform as much as possible to the principles underlying the interflow matrix produced by the University Institute of Economics, Oslo" (p. 45).

The mathematics is much above the head of the present reviewer, and there are not many occupants of the front rooms in Planning Commissions and Neddies round the world who could read it with profit; but their back-room boys could soon master it. The question is relevant, therefore, how far this kind of approach is practically useful.

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1962]

Clearly, it will be many a day before India either provides exact data to fill the cells in the matrices or commands political and administrative means to ensure the prescribed outcomes. (It will be interesting, now that mathematical planning is in vogue in the U.S.S.R., to see how far the conditions for success are present there.) At this stage the main point of exercises such as these is to contribute, as Professor Mahalanobis remarks in the foreword, to the "task of putting across to the public what planning really implies".

Here it seems to me that Professor Frisch cheats by sheltering behind the status of the technician who receives, ready-made, "the desired ends formulated by the appropriate political organs" (p. 4). Surely his business was rather to educate the politicians by showing them what is possible—what combinations of ends are compatible with each other and what outputs available resources make feasible. (Even a plumber must advise the housewife about flows and falls.) Frisch has had reports that his method has been successfully used in medium-sized projects (p. ii). Perhaps these were cases where the ends were clear and measurable—say, electric generating capacity (though even in that case conflicts between the claims of power and irrigation cannot well be left to "political organs" unaided to adjudicate). In discussing a general overall plan, the economist cannot excuse himself from evaluating ends.

In particular, Frisch seems to take over from the politicians the conception that avoiding unemployment is an end in itself. This leads to such nonsensical policies as subsidizing the use of a spindle, the product of which in an eight-hour day cannot even cover the necessary minimum wage of workers operating it. Surely an economist should look at the problem of employment in terms of finding the best use for available resources, rather than in terms of making work for idle hands?

The very method, based on input–output tables, seems to condemn its practitioners to accepting the existing technical coefficients and the existing pattern of production, making projections on that basis. But the task for Indian planners, to adapt Marx's famous slogan, is not to calculate the coefficients, but to change them. The task is to find out the most capital-saving (and in particular, the most foreign-exchange-saving) feasible methods and patterns of production. For this, of course, to get a picture of the existing position is an indispensable first step, but the politicians should not be led to believe that the present pattern is the best possible.

Hanging over the whole argument, there is a kind of miasma from a neo-classical production function, associating a high capital/labour ratio with a high capital/output ratio. In international comparisons, Norway seems to be the one country for which this holds good (whether for neo-classical or Kaldoresque reasons, or, what seems more plausible, because of the structural pattern of her investment in recent years). Perhaps Frisch has been influenced by this. Generally speaking, a low capital/output ratio (which is highly desirable for a developing country) may equally well be found along with a very high capital/labour ratio, in a fully automated modern installation, as with a very low capital/labour ratio in a primitive hand industry. This is a point on which politicians badly need educating, to discount both the snob appeal of the modern and the sentimental appeal of the ancient, so that programmes can be compared on their real merits.

Again, a discussion of the requirements for balanced growth is a necessary exercise but the problem confronting the planners is how to start building up the stocks of means of production that balanced growth at near-full employment will require. It is not much comfort to be told that a turnpike is the quickest way from south to north when our car is stuck in a bog some miles to the east.

There also seems to be a certain haziness about the relations between flows of goods and flows of money. In the description of balanced growth (p. 7 *et seq.*) the physical relations are set out. The desirable rate of growth is determined by the rate of growth of the labour force, and, given technical coefficients, this fixes the shares of consumption and investment in terms of flows of goods produced. Nothing is said about getting the flows of money demand and saving to fit (let alone dividing money demand between workers,

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capitalists, the professions and the peasantry). Here is a subject on which the politicians are only too pleased to escape from learning "what planning really implies".

Mathematical methods, certainly, are an indispensable adjunct to common sense, but they cannot serve as a substitute for it.

The first lesson that trainee back-room boys should be taught is to distinguish between accounting identities which express facts such as that, over a period, the output of a commodity plus withdrawals from stock is equal to the sum of its absorptions into various uses plus additions to stock; equations expressing equilibrium relations, such as the allocation of labour between uses compatible with the absence of inflation or unemployment; and models expressing behaviour reactions, such as the changes in demand and in supply of food grains, in each of the next five years, to be expected from starting work on an irrigation project next month.

I fear that in this respect Professor Frisch does not always set them a good example.

JOAN ROBINSON

21. The Guardian Wage Indexes: A Series of Indexes of Wage Rates in British Industry since 1948, Weekly and Hourly, together with Indexes of Average Earnings. By Ely Devons and J. R. Crossley. Manchester, The Guardian, 1962. 24 p. 11″. 10*s*. 6*d*.

The Guardian wage indexes first appeared under the title "An Index of Wage-Rates by Industries" by Professor Ely Devons and Mr R. C. Ogley in *The Manchester School* for May 1958. They were given a wider circulation by regular publication in *The Guardian* (Manchester Guardian) from May 1959. The pamphlet under review brings together the whole series of indices back to 1948 and gives a full explanation of the material and methods used.

The interesting feature is the industrial analysis of the indices. It is very difficult to make such an analysis because wage agreements and statutory wages regulation orders do not in general relate to the Minimum List Headings of the Standard Industrial Classification. Furthermore, the extent to which rates of wages are determined for different occupations and areas within each industry varies considerably. Indices for individual industries, therefore, are less reliable than the indices for all industries taken together. Nevertheless, providing these limitations are borne in mind, the series contained in this pamphlet should prove of some use to those studying problems related to inflation and wages policy.

Separate series are given for men and women and for weekly wage rates and hourly wage rates each month. Additionally, series based on the Ministry of Labour's half-yearly enquiries (average 1948 = 100) are given for the average earnings of manual workers each year. The distinction between basic wage rates and actual earnings is important and it is useful to have the two sets of figures together for comparison.

T. M. RIDLEY

22. Capital in the American Economy: Its Formation and Financing. By Simon Kuznets . . . assisted by Elizabeth Jenks. Princeton, University Press, 1961. [London, Oxford University Press.] xxx, 664 p. 9″. $12. (National Bureau of Economic Research, Studies in Capital Formation and Financing, No. 9.)

This is the final volume in the series of studies by the National Bureau of Economic Research of long-term trends in capital formation and its financing in the United States. Earlier volumes dealt with the main capital-using sectors of the economy, namely housing, public utilities, manufacturing and mining, agriculture, and the government. Kuznets has drawn on these sectoral studies, and supplemented them with estimates for those sectors not previously covered, to present an analysis of trends in capital formation and its financing in the American economy as a whole.

This is principally a study of long-term trends. The movement of the time-series analysed is found to consist of three components: short-period variations associated with

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business cycles; alternating accelerations and retardations of the rate of growth, often referred to as "long swings"; and the long-term trend. In order to show the long-term trend that is independent of the effects of business cycles and "long swings", most of the data are presented in the form of three-decade averages, the period of 30 years being long enough to exclude the effects of the "long swings". While comparisons between overlapping three-decade averages permit the detection of the long-term trend, a problem arises in that the most recent three-decade period includes both the Great Depression and the Second World War, economic disturbances of far greater magnitude than any others falling within the span of time covered by this study. The data for the decade 1946–55 are therefore shown separately, to permit comparison with trend values not affected by the Great Depression or the Second World War; but a decade is, of course, too short a period for the effect of the "long swings" to be excluded.

The study surveys trends in the ratios of the various types of capital formation to gross national product and national income, valued at current and constant prices; trends in the composition of capital formation by sector and type of asset; trends in its financing from funds obtained from internal or external sources; and in the type of external funds obtained. Two additional chapters analyse the "long swings" in the rate of growth.

It is impossible to summarize in a review of this length the findings of Kuznets's study, which itself to a considerable extent summarizes the findings of earlier volumes in the series. Only a few salient features can therefore be mentioned here. The upward trend of the capital/output ratio in the United States in the period between 1869 and just after the end of the First World War and the subsequent reversal of that trend are well known from earlier studies in the series. Not quite so well known, perhaps, is the marked increase in capital consumption relative to gross capital formation, a trend that is the result of changes in the composition of total capital formation and of the slowing down of the growth of the capital stock. In consequence there has been a much more substantial decline in net capital formation as a proportion of net national product than in gross capital formation as a proportion of gross national product, whether valued at current or constant prices, and whether defined in accordance with the concepts of national income accounting used by the National Bureau of Economic Research or those used by the Department of Commerce. Gross capital formation, on the other hand, appears to have declined appreciably as a proportion of gross national product if valued at constant prices; but if valued at current prices, and therefore abstracting from the apparent tendency of the prices of capital goods to rise faster than the prices of other goods and services, the decline in gross capital formation as a proportion of gross national product appears to have been very small, especially if the years of the Great Depression and the Second World War are excluded.

The analysis of capital formation by sector brings out clearly the increase, relative to other sectors, of the government, or rather governments, since a high proportion of public capital expenditure, other than that related to defence, is undertaken by State and local governments even today. The treatment of capital formation by the government is one of the least satisfactory features of the American national income accounts, so that the attention given to it by Kuznets in this study is especially to be welcomed.

Turning to the broader implications of this study, probably the most interesting is the view formed by Kuznets from this great accumulation of data about capital formation and its financing that in the *ex ante* sense the limit to the rate of capital formation in the United States in the period covered was set by the savings ratio, not by the inducement to invest. Kuznets agrees that it is only a "reasonable impression" and no more; but it is clearly an impression which has convinced him, for references to authors' "reasonable impressions" are seldom met with in National Bureau books. The significance of this impression is emphasized in the final chapter, "The Past as Prologue". There reasons are given for expecting that demands for capital will grow rapidly in the next three decades and will tend to exceed, *ex ante*, the supply of savings if the ratio of savings to gross national product and disposable income remains about the same as in recent years. In

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other words, the savings ratio will continue to set the limit to the rate of growth that the American economy can achieve. It must be emphasized, though, that this projection relates to the long period, and therefore in no way contradicts the view of the present Council of Economic Advisers that there still exists in the American economy a significant margin of unused capacity, such that in the immediate future a faster growth of effective demand would lead to a faster growth of output.

Whether or not one agrees with the discussion of future prospects, including radical changes in techniques for generating energy which will contribute to making the average capital/output ratio rise again, it is impossible to do anything but express indebtedness to Kuznets for this great contribution to our knowledge about capital formation. It remains for others to apply this knowledge in elucidating still further the economic processes at work in the United States in the last hundred years and in giving factual content to growth models. But thanks to Kuznets and his colleagues who wrote the earlier volumes in the series, and the Life Insurance Association of America whose grant made the investigation possible, a wealth of quantitative knowledge is there to be used.

A. E. HOLMANS

23. Business Cycle Indicators. Edited by Geoffrey H. Moore. National Bureau of Economic Research. Princeton, University Press; London, Oxford University Press, 1961. Vol. I. *Contributions to the Analysis of Current Business Conditions.* xxv, 757 p. 9″. $12.50. Vol. II. *Basic Data on Cyclical Indicators.* xvii, 179 p. 9″. $4.50. The set $15.00.

Signals of Recession and Recovery: An Experiment with Monthly Reporting. By Julius Shiskin. Occasional Paper 77, National Bureau of Economic Research, 1961. xii, 191 p. 9″. $3.

Short period cycles in economic activity in the United States have long been the subject of intensive and systematic analysis, and in recent years the development of electronic computers has given immense stimulus to this work. These two studies are concerned with one of the most interesting and potentially useful aspects: the techniques developed to facilitate early identification of turning points and more rapid agreement on the form and force of the counter-cyclical measures required. The essential feature of these techniques is the arrangement of economic series according to their customary timing relationship during the typical phases of expansion and contraction.

The basic technique is the classification of individual series according to whether their past fluctuations have generally preceded, roughly coincided with, or followed the reference cycles established by the National Bureau. A list of key indicators is then selected by criteria which include the consistency, regularity and clarity of their timing in relation to the reference cycles, and also the economic significance of the timing relationship. Examples of leading series are average weekly hours worked, new building contracts awarded, corporate profits and prices of industrial materials; the roughly coincident indicators include unemployment and gross national product; and among the lagging series are capital expenditure and unit labour costs.

Secondly, there are a variety of measures developed to summarize the movement of the basic series. The most useful of these is perhaps the diffusion index, which measures the *spread* of changes in activity and is obtained by calculating the percentage of indicators rising during successive periods. This can be applied in a number of ways: to all leading indicators, to all the component industries (or areas) for which a series (such as industrial production) is available, and so on. Other summary measures show the combined rate of change from month to month of standardized series, the proportion of series reaching peaks or troughs during successive periods, and the number of periods for which series have been rising.

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Thirdly, there is the information about the characteristic duration and pattern of the different phases gained from analysis of previous cycles. A most important example is the study of the correlation between the severity of a recession in its initial stages and its ultimate severity.

As Moore and Shiskin are very well aware, none of this vast array of statistical material can produce infallible judgements or automatic forecasts. But when used with intelligent caution and a knowledge of the economic and non-economic factors lying outside the range of this "early-warning system" it does provide a most valuable aid to diagnosis and prescription and is far in advance of anything being used in the United Kingdom.

A clear and up-to-date guide to these techniques can be obtained from the Occasional Paper by Shiskin and for the general reader this should be a quite adequate introduction. The specialist must turn to the first of the two volumes edited by Moore. This is intended for reference rather than reading, and brings together the main previous work in this field and also some new studies. (It would be much easier to refer to if the running titles on each page identified the twenty chapters rather than the three parts of the book.) Part I consists of ten papers on the selection and interpretation of the indicators. There is considerable repetition and also a fair amount of obsolescent material which is now of rather limited interest. Thus we are given two introductory essays (written in 1950 and 1956) which add little to what follows; and three chapters on the original selection of indicators by Mitchell and Burns in 1938, Moore's first post-war (1950) revision of this list, and a more recent selection by Moore, first made in 1955. This last chapter has been extensively revised and brought up to date and on its own contains most of what is wanted on the purpose and selection of the indicators. Three other chapters by Moore discuss the measurement of recessions at various stages during their course, and the measurement and significance of diffusion indexes.

Part II is devoted to six papers which analyse in great detail the behaviour of particular leading indicators. Three new studies deal with the cyclical aspects of business failures and corporate profits, the formation of new businesses and the timing of manufacturers' orders. Part III reproduces two long papers by Shiskin on the use of electronic computers and on various statistical problems, and two essays by Moore on summary indicators. There is again a certain amount of repetition.

Volume II describes fully the nature and source of some 70 main series and a large number of diffusion indexes; and gives the original monthly or quarterly series, the seasonally adjusted series and the seasonal adjustment factors from the first available date to 1958. The series are brought up to June 1961 in an appendix to Shiskin's Occasional Paper.

A concluding reflection is that a problem which might well become increasingly important in the future is the effect which the indicators will themselves have on economic activity: if every business man is authoritatively informed that, say, the boom still has nine months to run, both the timing and level of the actual peak might be very different from that which would have resulted from their original random anticipations.

C. H. FEINSTEIN

24. Equilibrium and Growth in the World Economy. Economic Essays by Ragnar Nurkse. Cambridge, Mass., Harvard University Press, 1961. xvi, 300 p. 8½″. 60*s*.

This is a collection of essays published over a period of 26 years by one of the most distinguished writers in the field of international economics, who died in 1959 at the tragically early age of 51.

Ragnar Nurkse was an Estonian by birth, studied economics at the Universities of Edinburgh and Vienna and was a member of the Financial Section and Economic Intelligence Service of the League of Nations from 1934 to 1945. After his native country had disappeared, he became an American citizen and from 1945 occupied a chair at Columbia University. These essays, re-published after his death and edited and introduced by

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Professor Haberler, show the development, under the influence of his environment and studies, of a remarkably sensitive and mature economic intelligence.

The first essay, written in Vienna at the age of 25, was strongly influenced by the peculiar Böhm-Bawerkian theory of capital taught by Austrian economists at that time. It was an attempt to relate international capital movements to the supposed "round-aboutness" of production in different countries and to the location of different stages of production in different parts of the world. It was a purely theoretical construction based on the Austrian theory of capital, but the author was clearly not very happy about this theory and expressed some concern about the impossibility of empirical verification of the conclusions reached.

The following essay, published two years later, was an open criticism of the linear structure of production underlying the Austrian theory. Nurkse suggested that this had arisen from a confusion between a socio-historical account of the evolution of the capitalistic system of production and a technical description of the system as it exists today; and was motivated by the need to introduce a time dimension to account for the existence of interest, which could not be explained by the Austrian theory of imputation. In place of the single "Hayekian" triangle of production, Nurkse proposed two triangular Departments, one producing fixed capital goods, the other consumer goods. Part of the output of each Department flowed back into its own production and part was exchanged with the output of the other. The process of saving and investment and the business cycle was traced through changes in relative prices in the two Departments and shifts of resources between them.

Although published in 1935, this was clearly a pre-Keynesian full employment model in which increased investment depended on increased saving and not vice versa. The model was perhaps less relevant to the time at which it was published than it is to the situation today. But it did emphasize a feature which occupied other economists a great deal later on, namely the close relationship between instability and progress in a capitalist economy.

There is a gap of 12 years and a striking advance in maturity between these two pre-war "Austrian" papers and the subsequent ones, during which period Nurkse was working on international financial questions as a member of the League of Nations Secretariat. The fruit of these studies, apart from appearing in his classic volume on "International Currency Experience" published by the League of Nations in 1944, is evident in the next series of papers produced during the period of international economic reconstruction immediately after the war.

Under the influence of Keynes the subject of full employment had in the meantime come very much to the fore, and the two 1947 papers deal with a problem with which we are still very much concerned today, that of reconciling a domestic full employment policy with balance of payments equilibrium.

Nurkse was a supporter of the Bretton Woods system. He contrasted the operation of the gold standard, under which exchange stability had been maintained by coordinating the incidence of the business cycle in different countries and movements of the reserves had served to transmit cycles from one country to another, with the new system under which a substantial degree of exchange stability was to be achieved by coordinating full employment policies, and reserves were to serve as insulators against, rather than transmitters of, the business cycle. Nurkse accepted that in any conflict between the requirements of internal stability and exchange stability, the former should have priority. Like many economists at the time, he was probably rather more sanguine about the possibilities both of controlling capital movements and of a rational adjustment of exchange rates than actual developments since the war have tended to justify. Nor, of course, did he foresee the balance of payments problems which could be created by a cost-push type of inflation, in a situation of full employment without excess demand.

Subject to the maintenance of internal stability, Nurkse favoured a multilateral system of trading as offering the technical advantages of the international division of labour.

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Short-term balance of payments deficits were to be met by using the reserves or short-term borrowing from the I.M.F., and longer-term disequilibria by adjustment of exchange rates. Unlike Professor Meade, however, Nurkse was opposed to the idea of freely fluctuating exchange rates, partly on the grounds that short-term elasticities of demand being so much lower than long-term elasticities would lead to unnecessarily wide fluctuations, and partly because of the difficulties of making the adjustments in internal purchasing power which were necessary to implement the effects of changes in exchange rates. Import restrictions he considered admissible only for relatively short-term purposes where reserves were insufficient, and as acting more quickly than tariffs or devaluation.

While the 1947 essays were written under the influence of the threat of balance of payments requirements to the maintenance of domestic full employment, a few years later Nurkse became aware of the opposite danger of the threat of inflation to balance of payments equilibrium. In his careful analysis of British experience made during his sabbatical year at Nuffield College in 1954–55 ("The Relation Between Home Investment and External Balance in the Light of British Experience, 1945–55") Nurkse applied the recently developed "absorption approach" to balance of payments policy (S. S. Alexander), which emphasized that in a fully employed economy an improvement in the balance of payments cannot be achieved without reducing domestic absorption of resources. In these circumstances measures such as import restrictions or devaluation are ineffective unless accompanied by the necessary degree of domestic disinflation. It was the need to release resources which could most readily find an export market which had led to repeated investment cuts during Britain's successive post-war balance of payments crises. Nurkse recognized Britain's need for a higher level of domestic investment, but in view of the importance of capital goods for exports, this could only be achieved consistently with a healthy balance of payments if capacity for producing capital goods were greatly expanded. He recommended special incentives for this purpose.

As an American citizen after the war, Nurkse became acutely conscious of the dominant influence of the United States in the world economy and often drew attention to the importance of the United States adopting "good neighbour" policies towards the rest of the world.

In the first of the post-war articles, "Full Employment in an Open Economy", he reflected the widespread concern felt in many countries about the disturbing effects on the rest of the world of United States business fluctuations. That such fluctuations would occur Nurkse regarded as inevitable. In fact in a later article (1954) on the inventory cycles which have played the major role in the post-war fluctuations of the American economy, Nurkse shows how these cycles flow quite naturally from the normal stock policies operated by individual firms: "there is nothing pathological", he writes, "about this type of inventory cycle . . . it strikes one as something like the breathing of a giant in vigorous activity" (p. 133). For the United States itself, with its high standard of living, the fluctuations were perhaps bearable or at any rate had to be accepted as an inevitable consequence of affluence and progress in a private enterprise economy. But something could and ought to be done, Nurkse felt, to soften the impact of these fluctuations on the rest of the world. The measures suggested were to stabilize the American demand for imports through buffer stocks and/or to stabilize the supply of dollars to foreigners through countercyclical foreign lending (p. 86).

Apart from the problem of stability, Nurkse felt that the United States should also take more responsibility than she appeared to be doing for dealing with the international dollar shortage which was such a dominating feature of the world economy in the first post-war decade. In his 1953 article on "A New Look at the Dollar Problem and the United States Balance of Payments", written at a moment when the dollar gap appeared for the first time to be closed, Nurkse emphasized the special short-term reasons for this phenomenon: the combination of a United States boom with a European depression, extensive American offshore purchases for military purposes, and continued widespread discrimination by other countries against dollar imports. In the longer run Nurkse felt

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that a tendency to imbalance would reassert itself: first because of the "demonstration effect", fostered by modern means of communication, of the high American standard of living in stimulating poorer countries to live beyond their means, and second because of failure of the United States to promote a balance in the way the United Kingdom had done in the nineteenth century either by allowing herself to become dependent on imports or by a sufficient flow of foreign investment.

"England made herself dependent on imports for her daily bread, whereas we—it seems—cannot even make ourselves dependent on imports for our Sunday cheese", he writes in the review of Viner's "International Economics" in 1952 (p. 361). Nurkse welcomed the movement towards free trade in the United States initiated by the Reciprocal Trading Act of 1934, but always emphasized that the *reciprocal* nature of the arrangements for tariff reductions prevented them from making any contribution towards restoring balance: *unilateral* tariff reductions were what were called for from the dominant economy in the existing world situation. Nurkse recognized that tariff reductions would necessarily involve painful internal adjustments in the United States economy, but he emphasized that it was better in the long run for the American economy to allow a decline in her least efficient industries, those that would be replaced by cheaper imports, rather than to curtail the expansion of her most efficient industries, those industries for which export markets were limited by dollar shortage.

But Nurkse never felt that free trade would—or should—provide the whole solution. Foreign lending from the United States played a minute role compared with the role which British foreign investment had played both in avoiding a "sterling shortage" and in promoting overseas development in the nineteenth century. Nurkse was a strong advocate of increased American foreign investment, not merely as a less painful way of correcting her balance of payments than the admission of more imports, but as something important in itself for the development of backward countries, and to reduce the enormous and widening gap in living standards between the less and the more fortunate inhabitants of the world.

In a later and bolder development of this train of thought in the 1956 paper on "International Investment Today in the Light of Nineteenth Century Experience" Nurkse recognized the real limitations on American foreign investment today and the quite peculiar circumstances which had led Britain at the turn of the century to export capital to the extent of as much as 7 per cent. of her whole national income (which would correspond to a rate of capital export of some $30 billions by the United States in the conditions of today). About two-thirds of Britain's capital exports went to relatively empty countries (the "regions of recent settlement") and were accompanied by some 60 million fairly educated emigrants, who were to a large extent "complementary" with the capital exported. The capital flowed freely under the stimulus of the profit motive, but served, in addition to earning dividends, to develop increased supplies of food and raw materials for the rapidly growing needs of Britain's own market. Most of the capital went in the form of bonds, and was used for productive purposes such as building railways and other public utilities.

The situation facing the United States foreign investor today is entirely different. There are hardly any empty countries left. Capital exports to underdeveloped countries serve as a substitute for immigration rather than as a complement to emigration. Most of the potential recipients are unversed in capitalistic techniques and are tempted to use capital imports for increased consumption rather than for productive purposes. The only major profitable field for foreign investment has been in the raw material extractive industries, such as oil, for which the United States market has been expanding; in backward countries American direct investment in these fields has often created problems of an unbalanced and "dual" economy and society in which an advanced export sector exists side by side with a primitive subsistence economy. Finally, profitable investment opportunities at home have acted as a constant disincentive to capital exports.

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Despite all these difficulties underdeveloped countries urgently require capital imports. Their greatest need is not so much for direct investment in extractive industries, which often creates social problems, as for fixed interest loans for public utility investment. This had been the major and most fruitful field of British foreign investment in the nineteenth century. The I.B.R.D. is operating in this field today but covers only a fraction of what is needed. If United States investors are not prepared to step into this gap as an economic proposition, Nurkse felt there was a strong case for doing so for political or moral reasons. He noted that the call for richer countries to assist poorer countries was becoming increasingly felt just as the call for richer sectors of the community to help its poorer members had already been increasingly recognized. He concluded his 1956 paper in the *Economic Journal* with a plea for international transfers to assist underdeveloped countries. He felt that these would not only help to meet the urgent needs of the underdeveloped countries themselves, but would also have the advantage for the United States of relieving her of the necessity of making the uncomfortable adjustment in her trade policy to meet the requirements of a major creditor nation.

The need for capital imports in underdeveloped countries is as acute as ever, but the fact that since these papers were written the United States economy no longer looks as dominant and the dollar problem has not only disappeared but actually been reversed has been due to a number of causes, but largely to the unexpectedly rapid rate of development in Western Europe. European rates of growth have been considerably faster in recent years than that of the United States, and European industrial exports have become increasingly competitive with American products both in third countries and in the United States itself. In addition, conservation policy has led to more sustained American imports of such products as oil and non-ferrous metals. Finally military expenditure abroad has continued at a very high level.

In the latest batch of essays in this volume, as in his major work on "Problems of Capital Formation in Underdeveloped Countries" published in 1953, Nurkse looked at the problems of development from the end of the underdeveloped countries themselves.

How or what should these countries develop in order not only to keep pace with their rapidly growing populations and resources, but also to achieve the rates of growth to which they aspired? It was in this connection that Nurkse evolved the concept of "balanced growth" with which his name was particularly associated and which aroused a surprising amount of criticism from certain other economists.

As in the case of foreign investment, Nurkse started out from the striking contrast between the situation of underdeveloped countries in the nineteenth century when international trade had operated as a tremendous "engine of growth", owing to the almost insatiable demand for imports in the United Kingdom, and the situation of most underdeveloped countries today whose traditional exports are confronted with a marked "lag" as a result of stagnation or agricultural protection in the advanced industrial countries or the development of synthetic substitutes. This export "lag" imposes a rigid limit on the rate of growth that can be achieved by "unbalanced" development in accordance with the existing international comparative cost structure. Only in oil-producing countries such as Kuwait and Iraq is investment confined to traditional export industries likely to produce a satisfactory rate of growth today.

The possibility of supplementing traditional primary exports by the development of cheap labour-intensive consumer goods for export markets is also a limited one: these industries may not at first be competitive without a subsidy, they depend on a lenient commercial policy on the part of the potential foreign customers, and they tend to consist of goods, such as textiles, for which demand is not rapidly expanding and which are therefore most likely to be protected in the older industrial countries. Possibilities of growth come up here also against rigid external limitations which the developing countries can do nothing about.

At this juncture Nurkse points to the possibility, and need, for resorting to investment in consumer goods industries for the domestic market. Given the existence of

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an export "lag" it is only by this type of investment that an underdeveloped country can achieve the rate of growth which it desires. It is to this field that the doctrine of "balanced growth" applies: a certain spread and balance determined by income elasticities of demand is essential if the type of goods produced is to correspond to the pattern of demand. It is less likely that investment of this kind will attract foreign capital, and if balance of payments difficulties are to be avoided it needs to be accompanied by increased domestic saving, which is always a difficult problem in poor countries. As in the case of England, Japan and the Soviet Union, industrialization generally calls for a prior agricultural revolution in order to finance the industrial investment.

To increase the profitability of investment for the domestic market, Nurkse continually emphasized the need for public overhead investment in basic services, and for the provision of these services *in advance* of the growth of the domestic market which would make use of them. He felt that the I.B.R.D., which is the only provider of finance for this type of investment today, adopted too restrictive an approach. In particular he criticized the limitation of I.B.R.D. finance to the direct foreign exchange cost of such projects: this tends to exclude some useful projects owing to lack of domestic finance, it encourages unnecessary imports, and it diverts such local savings as are available from direct industrial development for which foreign capital is even more difficult to obtain. Some of these criticisms have subsequently been met by the establishment of IDA (International Development Association) as a supplementary source of finance for economic development.

Nurkse was inclined to criticize the emphasis placed by many underdeveloped countries on the development of steel and heavy industries. He felt that these industries which were on the whole capital intensive and produced goods which could be imported should receive lower priority than public overhead investment which could be highly labour intensive, and therefore absorb some of the disguised unemployment in these heavily overpopulated areas, and which could not be replaced by imports. He was also rather critical of the revival in India of cottage industries which tend to compete with relatively efficient manufacturing industries for which capacity already exists. Labour intensive production he felt would be more useful in public utilities and in transport than in the already industrialized consumer goods industries.

The criterion applied in all these cases was that of long-run economic efficiency. Nurkse opposed the pessimistic suggestion of some economists that because of the inelasticity of demand for their exports, growth might in some circumstances lead only to "immiseration" due to the deterioration of the terms of trade. This Nurkse pointed out was only a short-term possibility based on the assumption that productive resources could flow only into the traditional export industries, however unprofitable these might have become. Underdeveloped countries are naturally more fortunate if, as in the nineteenth century, the demand for their traditional exports is expanding as rapidly as the resources which can flow into them. But where they are confronted with an export "lag" or even an export decline, it is still better for them to develop production for the home market than to leave their resources unemployed or to suffer "immiserizing" growth.

Nurkse always insisted that the science of economics must remain flexible and dynamic and related to current conditions in the world which it was to be used to interpret. It was of no use applying without modification economic theories developed in the light of conditions of the nineteenth century to the very different situation prevailing today. As he wrote at the conclusion of the Wicksell Memorial Lecture delivered shortly before his death, "An economics unaffected with real-world problems would court the danger of sterility."

M. F. W. HEMMING

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Current Notes

United Kingdom Official Statistics

1. *Bulletin on Incomes, Prices, Employment and Production*

In April 1962, the first issue of a new publication, *Statistics on Incomes, Prices, Employment and Production* was published by H.M.S.O. (price 12*s.* 6*d.*). It is prepared by the Ministry of Labour in collaboration with the Central Statistical Office and the Board of Trade and implements the proposal in the Government's White Paper, *Incomes Policy: The Next Step* (Cmnd 1626, Feb. 1962), "to collect together and publish in convenient form factual information on wage rates, earnings, hours of work and other conditions of employment, manpower, prices, production and profits and other relevant subjects so that due weight can be given by all concerned to the considerations set out in this paper at all stages of negotiation and arbitration".

The statistics presented (including a few comparable series in the form of graphs) are grouped under the following main headings: Part A—General—brings together statistics from 1950 relating to incomes, employment and production and shows how total domestic income has been shared by the main sectors; Part B—Wages and Salaries—includes regional indices of earnings of manual workers not previously published; Part C—Company Profits, Dividends, Assets, etc.; Part D—Hours of work; Part E—Manpower; Part F—Prices—wholesale, retail, import and export; and Part G—Production. There is also an appendix explaining the methods of construction of the various statistics.

It is intended to publish the next issue in September 1962 and the Bulletin will appear at regular intervals thereafter.

2. *Financial Statistics*

The first issue of a new monthly publication called *Financial Statistics*, prepared by the Central Statistical Office in cooperation with other Government Departments and the Bank of England, appeared in May (H.M.S.O. 7*s.* 6*d.*).

Amongst the tables published for the first time are those on local authority borrowing, the capital account of the Central Government, the appropriation and capital accounts of public corporations, superannuation funds in the public sector and the United Kingdom position in the International Monetary Fund. Other tables contain more information on interest rates and foreign exchange rates than was previously available. The publication contains a section on notes and definitions.

Further new series and additional details of existing series will be included in future issues.

3. *Statistics of Education*

In place of the statistics hitherto published as the second part of the Ministry of Education's annual report, the Department has introduced a new publication *Statistics of Education* to be published each year in two parts, the first part in the spring and the second in the summer at the same time as the annual report. Part 1 for 1961 (H.M.S.O., 15*s.*), published in April 1962, gives information about schools at January 1961, projections of school populations up to 1980, further education for 1960–61 and teachers employed at March 31st, 1961. Topics covered for the first time include analyses of the increasing tendency of pupils to stay on beyond the school-leaving age, teachers with mathematics and science degrees, entry and wastage of teachers, and salaries of teachers.

The second part of these annuals will contain the remainder of the Ministry's national statistics, covering principally G.C.E. examinations and school-leavers, further education, training of teachers, educational building and awards.

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4. *Unemployment*

A report on the manpower situation by a working party of the National Joint Advisory Council is published in the *Ministry of Labour Gazette* for February 1962. It contains a summary of the results of a special enquiry made by the Ministry into the characteristics of the unemployed. Figures are given of the proportion classified as difficult to place for various reasons, and of those considered to be good placing propositions. The extent to which the unemployed have received some training is recorded as is also the proportion requiring industrial rehabilitation, separate estimates being given for men and women. Other analyses cover the proportion of disabled persons and the extent of geographical mobility. Fuller details are given in the *Gazette* for April; a later article will deal with the unemployment history over four years of the persons covered by the enquiry.

STATISTICIANS AND TECHNICAL CO-OPERATION

The Department of Technical Co-operation came into being on July 24th, 1961, "for the purpose of co-ordinating, promoting and carrying out arrangements for furnishing countries outside the United Kingdom with technical assistance including, in particular, assistance in the fields of economic development, administration and social services". The countries concerned are mainly the "developing countries", that is to say, dependent territories, those countries which have recently become independent and others similarly engaged in rapid economic and social development.

The new Department deals with expert advice, the recruitment of experts and supply of equipment for particular projects, the training of persons from overseas countries in various expert disciplines in the United Kingdom and the recruitment of government officers for the dependent territories. The Department brings together the work in these fields formerly undertaken mainly by the Colonial Office, the Foreign Office, the Commonwealth Relations Office and the Ministry of Labour.

There is an urgent and immediate need for men and women, particularly with training in specialized techniques, to help the developing countries make full use of their present opportunities. The Department of Technical Co-operation is concerned that the United Kingdom should make its full contribution towards meeting this need. There is a demand under various schemes for qualified statisticians and those with allied experience, for example, of data processing. Most of the developing countries are engaged in a twofold task of filling gaps in their basic statistics as they endeavour at the same time to provide national income estimates and analyses for economic and other development plans. They may need guidance in devising ways and means of collecting data, as for example vital statistics and agricultural statistics, in conditions where the procedures in the United Kingdom are not applicable. Sometimes there is a need for a very experienced statistician to organize or take charge of statistical services or for specialized work on training programmes. The training of sufficient numbers of statisticians and statistical assistants and others at the middle level, without whom the proper operation of a statistical service is impossible, presents a special difficulty in many countries.

Occasionally, graduates with little or no post-university experience can find opportunities as government officials overseas on short-term contracts; those with degrees in statistics, mathematics or economics may be able to assist in building up a statistical service in a new country. For most Technical Assistance projects, however, mature knowledge in the selected field is necessary. This work cannot be arranged to provide career prospects as normally envisaged, but it should be possible in many cases to arrange with existing employers for men or women to be "seconded" for a period of overseas service.

The Department of Technical Co-operation would welcome enquiries from those who may be interested in this field of work. Details of specific appointments, as they arise,

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would be discussed with those having suitable qualifications and the Department would also be prepared to approach the employer of the applicant on the question of leave of absence or secondment.

Demand for Taxation Review

With the main hope for economic expansion in Britain pinned upon exports, the question of special incentives to exporters is of particular importance. In October 1961, the Federation of British Industries set up a working party to consider this matter; its findings were published in February this year in a 62-page pamphlet entitled *Export Incentives* (10*s*.).

There are two main difficulties in considering the relative positions of British and foreign exporters. First, there is the sheer inadequacy of the evidence as to the practice in other countries. Secondly, the big variation in fiscal systems makes comparison of the effect of taxation extremely complex. Nevertheless, the report attempts a factual review of the fiscal incentives, credit faculties and other export encouragement in a number of countries. It also records the international agreements on this subject.

The working party came to the conclusion that refunds of turnover taxes on exports, which are permissible under the rules of the General Agreement on Tariffs and Trade, do not amount to a subsidy. It found certain disadvantages in a turnover or general sales tax in place of purchase tax but considered that the advantage of a more widespread inducement to export outweighs such disadvantages, provided that the rate of tax is low.

The working party's recommendations are aimed more at limiting incentives afforded to foreign competitors than at specific assistance to British exporters. But it strongly recommends that the Government should make an urgent study of the fiscal system with a view to determining "whether the export effort could be assisted by changes in the present pattern of distribution of taxation and in the type and scope of indirect taxes, having due regard to the question of the harmonization of the U.K. tax system with those of the Common Market countries".

Handbook on London

The London County Council's new issue of *London Statistics* (Vol. IV, 1950–59, £2) covers, like its predecessors, a ten-year period, the information for the latest year being given in greater detail. The statistical tables for each section are preceded by explanatory text. On subjects such as weather, elections, vital statistics, transport and welfare services, the information available for the London area is quite detailed; on employment, wages and production it is very scanty.

Statistical Abstract of Israel

The twelfth *Statistical Abstract of Israel* (Central Bureau of Statistics, I£5) includes the preliminary results, from a hand-count, of the census of population and housing for 1961. These provisional figures suggest that estimates have been very satisfactory and confirm that the population of Israel has grown to about two and a half times its 1948 size. The abstract, with an English text, runs to over 500 pages and includes a revised series of national income estimates and the results of the 1959 census of manufacturing.

United Nations Statistical Publications

1. *Yearbook of International Trade Statistics, 1960*. English only. 603 p. U.S. $7; 50*s*. stg.

This is the eleventh issue of a compilation of national tables showing annual figures. Among the data shown are: value of merchandise imports and exports in national currency

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and U.S. dollars; trade in gold; quantum and unit value indexes, currency conversion factors; trade in principal commodities (quantity and value) according to the Standard International Trade Classification where possible, otherwise according to commodity classes based on the national classification; trade by principal countries of provenance and destination; duty receipts. There are also regional summaries and unit value indexes of exports of manufactured goods together with price indexes of primary commodities, distinguishing principal components.

2. *World Energy Supplies, 1957–1960.* English only. Statistical Papers, Series J. U.S. $1.50; 10*s.* 6*d.* stg.

This is the fifth in a series providing statistics on production, trade and consumption of solid and liquid fuels, gas and electricity for approximately 160 countries covering virtually all the world's population. Previous studies give data annually back to 1949 and for 1937 and 1929.

3. *Sample Surveys of Current Interest* (*Ninth Report*). English (French and Spanish in preparation). Statistical Papers, Series C. U.S. $0.50; 3*s.* 6*d.* stg.

Devoted exclusively to household surveys, Part I of this report describes surveys carried out in various countries. Part II consists of a list of reports on sample surveys, as well as a subject index of household surveys described in earlier issues of this series of reports.

4. *Bibliography of Industrial and Distributive-Trade Statistics.* English only. Statistical Papers, Series M. U.S. $0.75; 5*s.* stg.

In response to many requests for sources of data, this work lists the recent national enquiries into, and the statistical series compiled about, the industrial and distributive-trade sectors of the economy and of the publications in which the resulting data were issued. A brief description is given of the topics and field covered.

5. *Retail Price Comparisons for International Salary Determination.* English (French and Spanish in preparation). Statistical Papers, Series M. U.S. $0.75; 5*s.* stg.

In continuation of the work described in the previous (1959) report in this series, recent information is presented, including data on 22 cities.

International Statistical Institute

The following Fellows have been elected members of the Institute:

Professor P. Armitage; Dr W. R. Buckland; Mr J. Hajnal and Dr J. M. Hammersley (United Kingdom).

Dr C. Eisenhart (United States).

Professor H. O. Lancaster (Australia).

Dr M. G. Kendall has been appointed chairman of the Programme Committee. Mr C. T. Saunders and Mr W. G. Cochran (Honorary Fellow) have been appointed members of the Editorial Committee of the *Review* of the Institute.

Lt.-Col. N. Ahmed (Pakistan), Mr N. M. Idaikkadar (Ceylon), Dr W. P. D. Logan (United Kingdom) and Mr D. W. Maitland (Australia) have been designated as *ex officio* members of the Institute in virtue of the positions they hold.

Conference on Mathematical Probability

Preliminary notice is given of an instructional conference on Mathematical Probability to be held during the fortnight March 28th to April 11th, 1963, in the Department of Mathematics, Durham Colleges, University of Durham, under the auspices of the London Mathematical Society.

This will be the second in the London Mathematical Society's series of Instructional Conferences; the first, on Functional Analysis and some of its Applications, was held in

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April 1961. While it is hoped that there will be much in the programme to interest specialists, the main purpose of the 1963 Conference is to meet the needs of mathematicians and statisticians who would like a thorough introduction to mathematical probability.

The Conference will begin with two series of introductory lectures delivered in parallel on the first day; the first, on the applications of probability theory, will consist of informal accounts of some practical problems which have motivated much recent theoretical work, while the second will survey some of the mathematical prerequisites for the subsequent courses. These introductory lectures will be followed by a course of basic lectures on the fundamentals of the subject, supplemented by classes for informal discussion.

The second part of the Conference will be devoted to three series of advanced lectures. Each of these, starting where the basic course leaves off, will attempt to give a thorough review of some branch of the subject in which recent research has been most active.

Arrangements are being made for accommodation and full board to be provided at the Durham Colleges at a charge of 30*s*. per day per person, inclusive of gratuities. Full details of the organization and programme of the Conference will be advertised in due course, but those wishing to be kept informed of the arrangements are encouraged to register now. The registration fee of two guineas (which covers participation in all the scientific activities of the Conference) should be sent to the Secretary, Science Laboratories, South Road, Durham, England, and the envelope should be marked "Probability Conference"; remittances should be made payable to "The London Mathematical Society".

Appointments, Elections and Awards

Dr T. Barna has been appointed to the Chair of Economics at the University of Sussex and leaves the National Institute of Economic and Social Research this autumn to take up his new post.

Mr J. L. Nicholson has been awarded a Simon Research Fellowship for 1962–63 at Manchester University to study the economics of index numbers.

Dr Gertrude Cox, an Honorary Fellow of the Society, has been awarded the Gamma Sigma Delta award for distinguished services to agriculture.

Sir Austin Bradford Hill, C.B.E., F.R.S., has been elected an Honorary Fellow of the Royal Society of Medicine.

Professor L. J. Savage has been elected a Fellow of the Institute of Mathematical Statistics.

Professor D. V. Glass was nominated President and Dr B. Benjamin Secretary-General and Treasurer of the International Union for the Scientific Study of Population, at the General Assembly held in New York in September 1961.

Birthday Honours

We offer our congratulations to Mr J. D. North who was made a C.B.E. in the 1962 Birthday Honours List.

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List of Books Received for Review

(*These books are available to Fellows in the Society's Library*)

Aitken, A.C. The case against decimalisation. Edinburgh, London, Oliver & Boyd, 1962. 22 p. 8¾″. 2*s*. 6*d*.

Alder, Henry L. & Roessler, Edward B. Introduction to probability and statistics. 2nd ed. San Francisco, London, W. H. Freeman & Co., 1961. xii, 289 p. tables. 9¼″. 32*s*.

Beacham, R. H. S. Measurement of working capital. Bristol, Society of Commercial Accountants Ltd., [n.d.]. 81 p. 8½″. 10*s*. 6*d*.

Beijer, G. *and others*. Characteristics of overseas migrants; by G. Beijer, N. H. Frijda, B. P. Hofstede and R. Wentholt; with a foreword by B. W. Haveman, edited and introduced by G. Beijer. The Hague, Government Printing and Publishing Office, 1961. xvi, 319 p. tables. 9½″. 20 *guilders*.

Bellettini, Athos. La popolazione di Bologna dal secolo XV all'unificazione italiana. Bologna, Zanichelli, 1961. xv, [1], 447 p. plates, tables. 9¼″. *L*. 4,000.

Bertolino, Alberto. Cooperazione internazionale e sviluppo economico. Firenze, La Nuova Italia, 1961. [4], 198 p. 8½″. *L*. 2,000. (Il pensiero economico (nuova serie) 6.)

Bolza, Oskar. Lectures on the calculus of variations. 2nd ed. New York, Dover, [1961]. [London, Constable.] xi, 271 p. diagrs. 8″. 13*s*. 6*d*.

Bowman, F. Introduction to elliptic functions with applications. New York, Dover, [1961]. [London, Constable.] 135 p. diagrs. 8″. 10*s*.

Britnell, G. E. & Fowke, V. C. Canadian agriculture in war and peace 1935–50. Stanford, California, University Press, 1962. xvi, 502 p. 19 tables. 9″. $8.75.

Cambridge Philosophical Society. Author index to Proceedings of the Cambridge Philosophical Society: volumes 1–50 (1843–1954), and Transactions of the Cambridge Philosophical Society: volumes 1–23 (1822–1928). Cambridge, 1961. [6], 105 p. 9¾″. 20*s*.

Christenson, C. L. Economic redevelopment in bituminous coal: the special case of technological advance in United States coal mines, 1930–1960. Cambridge (Mass.), Harvard University Press. [London, Oxford University Press], 1962. xxi, 312 p. diagrs. 9¼″. 60*s*.

Coulmy, Geneviève. Exploitation des relevés expérimentaux. Paris, Gauthier-Villars & Co. & Eyrolles, 1962. xiii, 186 p. diagrs. 8¼″. 20 *NF*. (Manuels de calculs techniques, collection dirigée par Louis Couffignal. Volume IV.)

Cox, D. R. The role of statistical methods in science and technology: . . . an inaugural lecture delivered at Birkbeck College 21st November 1961. London, Birkbeck College, 1961. [3], 12 p. tables. 10¼″. 2*s*. 6*d*.

Crossan, Richard M. & Nance, Harold W. Master standard data: the economic approach to work measurement. New York, London, McGraw-Hill, 1962. x, 257 p. diagrs. 8½″. 58*s*.

David, F. N. & Barton, D. E. Combinatorial chance. London, Griffin, [1962]. x, 256 p. 9″. 63*s*.

Denison, Edward F. The sources of economic growth in the United States and the alternatives before us. New York, Committee for Economic Development, 1962. [8], 297 p. tables. 10″. 30*s*. (Supplementary paper no. 13.)

Devons, Ely, & Crossley, J. R. The Guardian wage indexes: a series of indexes of wage rates in British industry since 1948, weekly and hourly, together with indexes of average earnings. Manchester, The Guardian, 1962. 24 p. 12 tables. 11″. 10*s*. 6*d*.

Duckworth, W. E. A guide to operational research. London, Methuen, [1962]. 145 p. diagrs. 8½″. 20*s*.

East African Statistical Department. East Africa insurance statistics, 1959. Nairobi, E. African Statistical Department, 1961. 18, [8] p. tables. 10¾″. 3*s*.

Farnsworth, Helen C. Defects, uses and abuses of national food supply and consumption data. Stanford, University Press, 1961. Food Research Institute Studies (1961), 2, No. 3. Nov. tables. 10″. $1.

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1962]

Finney, D. J. An introduction to statistical science in agriculture. 2nd ed. [Copenhagen, Munksqaard]; Edinburgh, London, Oliver & Boyd, 1962. 216 p. diagrs. 9½″. 30*s*.

Gayen, A. K. *and others.* Measurement of achievement in mathematics: a study on effectiveness of board and university examinations in India . . . Report no. 1 of the research project on examinations sponsored by Ministry of Education, Government of India, New Delhi. [Kharagpur, Indian Institute of Technology, 1961.] 238 p. (Various paging.) tables. diagrs. 9¾″. *n.p.*

Greenwood, J. Arthur, & Hartley, H. O. Guide to tables in mathematical statistics. Princeton, University Press; London, Oxford University Press, 1962. lxii, 1014 p. diagrs. 10″. 55*s*.

Hogg, Robert V. & Craig, Allen T. Introduction to mathematical statistics. New York, London, The Macmillan Company, [1959]. xi, 244 p. 9¼″. 51*s*.

Ilersic, A. R. The taxation of capital gains. London, Staples Press, 1962. 167 p. 7¼″. 21*s*.

International Economic Association. International economic papers, no. 10; translations prepared for the International Economic Association; edited by Alan T. Peacock, Wolfgang F. Stolper, Ralph Turvey and Hans Liesner. London, Macmillan & Co. Ltd, 1960. 193 p. diagrs. 10″. 21*s*.

Ireland. *Central Statistics Office.* Census of population 1961, preliminary report. Dublin, Stationery Office, 1961. 25 p. tables. 9½″. 2*s*.

Jeffreys, Harold. Asymptotic approximations. Oxford, Clarendon Press, 1962. [4], 144 p. 8½″. 30*s*.

Jolley, L. B. W. Summation of series: collected by L. B. W. Jolley . . . . 2nd ed. New York, Dover, [1961]. [London, Constable.] xii, 251 p. tables. 8″. 16*s*.

Kendall, M. G. A course in the geometry of n dimensions. London, Griffin, [1961]. viii, 63 p. 8½″. 16*s*. (Griffin's statistical monographs & courses, 8.)

Kendall, Maurice G. & Doig, Alison G. Bibliography of statistical literature, 1950–1958. Edinburgh, London, Oliver & Boyd, 1962. xii, 297 p. 9½″. 63*s*.

Klein, L. R. *and others.* An econometric model of the United Kingdom; by L. R. Klein . . . R. J. Ball, A. Hazlewood and P. Vandome . . . . Oxford, Blackwell, 1961. xii, 312 p. diagrs. 8¾″. 60*s*. (Oxford University Institute of Statistics, Monographs 6.)

Li, Choh-Ming. The statistical system of Communist China. Berkeley, Los Angeles, University of California Press, 1962. [London, Cambridge University Press.] viii, [2], 174 p. tables. 8½″. 32*s*.

Loève, Michel. Probability theory. 2nd ed. Princeton, N. J., London, Van Nostrand Co., 1960. xvi, 685 p. 9″. 115*s*.

Mark, Mary Louise. Statistics in the making: a primer in statistical survey method. Columbus (Ohio), Bureau of Business Research, College of Commerce & Administration, Ohio State University, 1958. xxvi, 436 p. diagrs. 9″. $5. (Bureau of Business Research, publication 92.)

Merrett, A. J. & Bannock, G. Business economics and statistics. London, Hutchinson, [1962]. 271 p. tables, diagrs. 8¼″. 42*s*.

Miller, G. A. *and others.* Theory and applications of finite groups; by G. A. Miller, H. F. Blichfeldt [and] L. E. Dickson. New York, Dover, [1961]. [London, Constable.] xvii, 390 p. 8″. 16*s*.

National Bureau of Economic Research. The American baby boom in historical perspective; [by] Richard A. Easterlin. Princeton, University Press, 1962. [2], 60 p. tables. 9″. $1. (Occasional paper 79.)

Dividends under the income tax; by Daniel M. Holland . . . . Princeton, University Press, 1962. xvii, 189 p. 47 tables. 9″. 36*s*. (Fiscal studies 7.)

Freight transportation in the Soviet Union, including comparisons with the United States; by Ernest Williams, Jr . . . . Princeton, University Press; London, Oxford University Press, 1962. xxi, 221 p. tables. 9″. 36*s*. (General series 76.)

Growth of industrial production in the Soviet Union; by G. Warren Nutter . . . assisted by Israel Borenstein and Adam Kaufman. Princeton, University Press, 1962. xxvii, 706 p. tables, diagrs. 9″. 120*s*. (General series 75.)

The rate and direction of inventive activity: economic and social factors: a conference of the Universities—National Bureau Committee for Economic Research. Princeton, University Press, 1962. [4], x, [3], 635 p. tables. 9″. 100*s*. (Special conference series, vol. 13.)

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Small-scale industry in the Soviet Union [by] Adam Kaufman. Princeton, University Press, 1962. [6], xvi, 95 p. tables. 9″. $2. (Occasional paper 80.)

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The Statistical Evaluation of Educational Allocation and Selection

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[Read before the ROYAL STATISTICAL SOCIETY, on Wednesday, May 16th, 1962, the PRESIDENT, Dr M. G. KENDALL, in the Chair]

SUMMARY

This paper is an attempt to formulate some of the problems of educational allocation in statistical terms, and to indicate how optimal solutions can be sought. An oversimplified model of the process that selects university entrants is presented and examined in detail. Attention is concentrated on the two stages of selection at 11 + and at the age of leaving school, and the effects of varying proportions at these stages on the operating characteristics of the whole process are discussed. It seems clear that such a process must fail to discover a high proportion of those who are above the minimum standard for suitability for a university, even though it makes provision for correction of gross errors of judgement at 11 +, but these failures will consist largely of marginal cases.

In later sections, the model used is criticized and various modifications proposed. A more realistic model for the existing situation, taking account of additional factors and stages of selection, might have a better operating characteristic. The consequences of increasing the proportion of the population admitted to universities are briefly mentioned, but this step is in the main a change of standards rather than an improvement in detection of the most suitable entrants. A model appropriate to comprehensive secondary schools, with their opportunities for frequent sifting and re-grading, might give much better results, but at present the mathematical complexity prevents its further discussion. Comments are made on the adjustment of the model to take account of the effect of type of secondary school on the subsequent abilities of the children, and on the recognition that the dichotomy "university-or-not" is an inadequate summary of the purpose and success of education. The statistical techniques used in the paper could be adapted to some of the more general and more realistic problems; clearer definitions of aims and criteria are first needed, and very heavy computations could then be undertaken.

Finally, I must emphasize that I neither assert nor believe that educational policy on allocation or selection can be reduced to problems of pure mathematical and statistical analysis. I fully recognize that the fundamental questions are for experts on education to discuss and to answer. The sole virtues of the present approach are objective examination of how to implement most efficiently an agreed general policy and a consequent focusing of attention on the questions that need to be answered before a determinate policy can be stated. My intention throughout has not been to produce new educational dogma, or even to subscribe to old dogma, but to show the kind of help that the statistician ought to be able to give to the quantitative evaluation of educational policy.

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1. JUSTIFICATION

THIS paper is not an attempt by a statistician to usurp the functions of educational experts or to suggest that educational planning can be reduced to problems of statistical theory; it does advance the claim that some aspects of educational policy are largely statistical in character and that there is no excuse for not making the statistical treatment as efficient as possible.

A few months ago, in his contribution to a series of articles on "The Gifted Child", Sir Cyril Burt (1961) remarked that "On the dull, the defective and the mentally subnormal, the literature is nearly 10 times as voluminous as the literature dealing with the gifted." He later wisely commented: "The nation's pool of ability is strictly limited; and we still allow much of it to run idly to waste." Few today would question the need for providing adequately for the education of the subnormal, even though here the concern may be largely with the effect on the child and less with his individual contribution to the community. If we neglect the other end of the scale, at our peril we leave to haphazard development that most valuable of natural resources, the mental abilities of our ablest citizens. Moreover, any consideration of educating the gifted child must take into account not only the effect on him but also his potential value for the enrichment of the life of the community.

I hope no one will quarrel with these statements, but their relation to statistics may not be clear. However, if we are to develop to best advantage the mental abilities of all, emphasis on the manner in which children are guided into different channels of education must grow. Under authoritarian rule, guidance may be submerged in some more rigid control. Surely the principle needs equal recognition even under a democracy that leaves much to personal choice, at least to the extent that those responsible for the provision of educational facilities from public funds must be concerned to design them and encourage their use for the maximum benefit of the community. Some system of allocation to different types of school, or to different streams within a school, is then seen to be inevitable; the aims of that system deserve careful study and definition, and the manner in which those aims are to be achieved must then be examined. Of course, "allocation" might imply only recommendation rather than compulsion. (In most democracies, parents are permitted to by-pass the "system" by way of fee-paying schools; discussion of the educational merits and of the ethics of this is of undoubted importance, but forms no part of the present subject, and my paper does not relate to children from these schools.) The specification of the ideal pattern of allocation is a task for, and doubtless a source of great argument among, educational experts. Once the ideals have been agreed, however, there can be no excuse for not attempting to approach as closely to them as the available instruments for measuring ability will permit. Even if those who determine policy were to be unsure of their aims, so that, either on account of differences of opinion (in effect the present position in respect of different Local Education Authorities) or for an experimental period, different policies were allowed to coexist, implementation of each with maximal efficiency would be desirable.

This paper is an attempt to discuss some of the problems of achieving optimal allocation. It is open to criticism as being too strongly oriented to the success with which those of highest ability are allocated. As in other contexts, no evaluation of an optimal is possible until a criterion for deciding between alternatives has been agreed. The criteria used below may indeed be prejudiced in favour of the more able; the method, however, can be adapted to other criteria, and the only barrier is the

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necessity of first reaching agreement on the ideal criterion, which is not really a statistician's responsibility.

2. The Role of the Statistician

When a criterion has been decided, the statistician can begin to make his contribution. The measuring instruments available as a basis for allocation—tests, examinations, school records, interviews—are notoriously imperfect predictors of future performance, but there is no escape from using them. The best way of using them, "best" in the sense of maximizing agreement between the actual and the ideal allocation, is open to investigation. In particular, any simplified but tolerably realistic model of the system to be operated will recognize that there must be more than one point in the career of a child at which he is exposed to the process of allocation, and the statistician should be able to offer advice on the optimal balance between proportions at these different *stages*. The purely statistical problems involved are not easy, and any progress seems likely to require very extensive computations. This paper attempts a beginning by consideration of one very simple model, discussion of which will illustrate some of the difficulties, statistical and non-statistical, that are involved.

A complete separation between formulation of policy and statistical study of its optimal implementation is undesirable, for each will benefit from a proper understanding of the other. Moreover, examination of the results of the statistical analysis may sometimes be a pointer to the inadequacy of the information on which the policy was based or to oversimplifications in the statement of that policy. The analysis will be entirely objective, and, provided that mathematical and numerical errors have been eliminated, any foolish appearance of its results can derive only from some folly implicit in the model to which it was applied.

Sections 3 to 8 of this paper, then, present and discuss a very simple model and its far from simple analysis. I am well aware of some of the grave imperfections in this; however, I suggest that a broad general consideration of some of the flaws in the model, as in Sections 9 and 10, may indicate qualitatively the changes in conclusions that are desirable. And, perhaps far more important, critical study of these flaws may aid the fuller understanding of the problems and the subsequent formulation of a more realistic model, may indeed help to concentrate research on the gap between educational intention and educational practice.

3. The Population Model

I propose to discuss a model that involves allocation or selection at two stages only, perhaps the simplest non-trivial case. Although I am concerned more with method than with particular numerical values, I think it important to see examples of the numerical behaviour of the model; I shall therefore present the method as applied to two stages in the life of each cohort of children, grammar school admission (i.e. "11+") and university admission, for which certain statistical parameters will later be guessed (Section 7). Moreover, I propose to concentrate attention on the question of whether or not the right persons receive a university education, admittedly a grave narrowing of the objectives of secondary schooling.

For simplicity, suppose that at each of the two stages individual decisions are based on scores compounded from all test and examination results and other information then thought relevant; the optimal manner of combining many different measurements available at one stage into a single index or score is not under discussion here,

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but is supposed already to have been decided, perhaps by techniques of multiple regression. The grammar school admission score will be termed y_1 and the university admission score y_2. As an ideal criterion, suppose that there exists a quantity x for each individual, representing his suitability for university education. One would like to think that, for those who *are* admitted to a university, x was closely related to performance in the final degree examination, but that would imply a validity for university examinations greater than their more objective critics dare claim. Thus x cannot be determined for any individual, but perhaps some guess as to its distributional properties will prove reasonable.

Without any loss of essential generality, x, y_1, and y_2 may be taken as normally distributed for the whole population in an age group, with means zero and variances σ^2, ω_1^2, ω_2^2 respectively; any other distributions can be rescaled to these. (The further simplification of scaling so that all variances are unity would have the disadvantage of destroying numerous simple checks on the "dimensions" of formulae.) In addition, however, x, y_1, y_2 will be assumed to have jointly a trivariate normal distribution in this population, with correlation coefficients ρ_1, ρ_2 between x and y_1, y_2 and ρ_y between y_1 and y_2.

Suppose now that at the first stage the population is divided into two parts on the basis of the known values of y_1; a fraction P_1 of the whole with the highest values of y_1 forms one part and the remaining fraction Q_1 $(=1-P_1)$ forms the other. In general, the two sub-populations thus created will no longer be normally distributed in respect of any of x, y_1, y_2. However, unless P_1 or Q_1 is very small or ρ_1 and ρ_y are very large, the distortion of the form of the distributions of x and y_2 will not be great. Moreover, as is well known, correlation coefficients will usually be reduced in absolute magnitude within the sub-populations.

Properties of the distributions in these sub-populations are easily obtained (Pearson, 1902, 1912; Finney, 1956). They are expressible in terms of functions of the standardized normal distribution. If $T(P)$ and $Z(P)$ are functions of a proportion $P, O \leqslant P \leqslant 1$, defined by

$$P = \int_{T(P)}^{\infty} \frac{1}{\sqrt{(2\pi)}} e^{-\frac{1}{2}t^2} dt \tag{3.1}$$

and

$$Z(P) = \frac{1}{\sqrt{(2\pi)}} e^{-\frac{1}{2}\{T(P)\}^2}, \tag{3.2}$$

then the function $\nu(P)$ is defined as

$$\nu(P) = Z(P)/P, \tag{3.3}$$

and differential coefficients of ν *with respect to* T are denoted by $\nu', \nu'', \ldots$ etc. Pearson (1931) has tabulated T and ν for $P = 0{\cdot}001(0{\cdot}001)0{\cdot}999$. Elsewhere (Finney, 1956), I have tabulated T, ν, and its first four derivatives for a few values of P, and an unpublished table by Lipton covers the range up to $P = 0{\cdot}5$ fairly thoroughly.

In the sub-population with the fraction P_1 of highest values of y_1, the mean values are

$$E(x) = \sigma\rho_1\nu(P_1), \tag{3.4}$$

$$E(y_1) = \omega_1\nu(P_1), \tag{3.5}$$

$$E(y_2) = \omega_2\rho_y\nu(P_1). \tag{3.6}$$

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The corresponding variances are

$$V(x) = \sigma^2\{1-\rho_1^2\,\nu'(P_1)\}, \tag{3.7}$$

$$V(y_1) = \omega_1^2\{1-\nu'(P_1)\}, \tag{3.8}$$

$$V(y_2) = \omega_2^2\{1-\rho_y^2\,\nu'(P_1)\}. \tag{3.9}$$

The correlation coefficients in this sub-population are

$$\rho(x,y_1) = \rho_1\left\{\frac{1-\nu'(P_1)}{1-\rho_1^2\,\nu'(P_1)}\right\}^{\frac{1}{2}}, \tag{3.10}$$

$$\rho(x,y_2) = \frac{\rho_2-\rho_1\,\rho_y\,\nu'(P_1)}{\{1-\rho_1^2\,\nu'(P_1)\}^{\frac{1}{2}}\{1-\rho_y^2\,\nu'(P_1)\}^{\frac{1}{2}}}, \tag{3.11}$$

$$\rho(y_1,y_2) = \rho_y\left\{\frac{1-\nu'(P_1)}{1-\rho_y^2\,\nu'(P_1)}\right\}^{\frac{1}{2}}. \tag{3.12}$$

Hence as a first approximation, the distribution may be taken as trivariate normal with the nine parameters just specified. Similarly the distribution in the Q_1 fraction will have for its parameters these expressions with $\nu(P_1)$ replaced by $-\nu(Q_1)$ and $\nu'(P_1)$ replaced by $\nu'(Q_1)$.

When attention turns to the second stage, only the joint distribution of x and y_2 is of interest. The bivariate normal approximation will often be adequate, since the greatest distortion of normality will be that affecting y_1; both this approximation and an improvement on it are employed in Section 8. As Finney (1956, 1962) and Tallis (1961) have shown, the general moments or cumulants of this distribution are easily obtainable. If κ_{ij} is written for the bivariate cumulant of order i in x and j in y_2, then for $i+j>2$

$$\kappa_{ij} = (\sigma\rho_1)^i\,(\omega_2\,\rho_y)^j\left(-\frac{\partial}{\partial T_1}\right)^{i+j-1}\nu(P_1) \tag{3.13}$$

for the P_1 fraction and

$$\kappa_{ij} = -(\sigma\rho_1)^i\,(\omega_2\,\rho_y)^j\left(\frac{\partial}{\partial T_1}\right)^{i+j-1}\nu(Q_1) \tag{3.14}$$

for the Q_1 fraction.

4. Selection and Allocation

I have elsewhere (Finney, 1958) contrasted educational selection with the type of selection used by the plant breeder. There is superficial resemblance, in that the plant breeder has annually a new cohort of new varieties out of which, on the evidence of performance in one or more annual trials, he tries to select the best. One obvious difference is that each variety is, or should be, an entity of constant character not permanently affected by the environment to which it is exposed for a single season of growth, whereas one may expect the educational environment of a child at one period to modify permanently his capacity to respond to future environments. On this, more will be said in Section 10. A second difference of immediate importance is that new varieties of a crop of economic value will be discarded completely as soon as the evidence that they are outclassed by alternative varieties is adequate. The education of a child whose performance in a test at age 11 is markedly inferior to that of some of his contemporaries may on that account be modified in pattern but will rightly be

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continued. The fate of a discarded variety is of no interest to the plant breeder, and his plan for an optimal selection programme will take no account of it; to use an epithet such as "discarded" as applicable to a child of indifferent or poor intellectual capacity would be a gross distortion of human values, and nothing is further from the intention of this paper.

This vital difference lies at the root of the distinction that many are careful to draw between *selection* of children for a more academic type of education and *allocation* of each child to that form of education best suited to his abilities and needs. The 1944 Education Act requires Local Education Authorities to secure that the schools available for an area are "sufficient in number, character, and equipment to afford for all pupils opportunities for education offering such variety of instruction and training as may be desirable in view of their different ages, abilities and aptitudes". The recent tendency has therefore been to emphasize allocation rather than selection. In so far as the distinction serves to ensure that lesser intellectual capacity is not used as an excuse for giving a child an intrinsically inferior education, it is valuable. Once the ideal of having several streams of education, each excellent of its kind, is conceded, and children are placed in one or another according to estimates of their capacities, the purely verbal distinction becomes unimportant except for the images that the two words evoke in the public. Selection of the more academic minds for one educational stream or allocation of the less academically proficient to an educational pattern thought especially suitable will be much the same in practice. The one essential proviso, as safeguard against the extreme selectionist outlook, is that decisions shall not be irrevocable. First must come recognition that any allocation based upon imperfect information carries a certainty that errors of judgement will occur; then follows insistence that when mistakes at one stage subsequently become apparent they shall be corrected as soon as possible.

5. A Simple Selection Model

About the simplest mathematical model that covers the two stages of grammar school and university admission for a cohort is one that begins as in Section 3. At the first stage, the fraction P_1 having the highest values of y_1 is admitted to grammar schools. At the second stage, a fraction P_2 is selected as having the highest values of y_2 within the P_1 sub-population, these constituting the grammar school entry to the university. In addition, provision is made for correcting erroneous judgements at the first stage by also selecting a fraction P_2^* as having the highest values of y_2 within the Q_1 sub-population. The total proportion of the age group admitted to universities is therefore π, where

$$\pi = P_1 P_2 + (1 - P_1) P_2^*. \tag{5.1}$$

In practice, one would expect P_2^* to be much smaller than P_2, but the existence of a non-zero P_2^* is the assurance that a decision at the first stage is not irrevocable.

It is implied here that y_2 represents the same system of scoring within each sub-population, as would be true if it were based on G.C.E. results. This is mentioned again in Section 9.

This paper is chiefly concerned with the optimal choice of the allocation parameters P_1, P_2, P_2^*, subject to a fixed value of π and the constraint (5.1). The proportion of the cohort entering universities is supposed determined by external factors, but the allocation fractions at the two stages can be varied in order to meet other requirements.

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6. Optimal Selection

Vastly oversimplified though the model in Section 5 is, it includes a wide range of possibilities. One extreme is $P_1 = 1$, $P_2 = \pi$ (P_2^* is indeterminate), which implies accepting every child for grammar school education and in effect deferring all selection until the stage of university entrance. Another is $P_2^* = 0$, or "the irrevocable 11+", and even more particularly $P_1 = \pi$, $P_2 = 1$, $P_2^* = 0$, implying that the eventual university entrants are chosen at 11. Still more absurd, but serving to illustrate the range of possibilities covered, is any system with $P_2 = 0$: all university entrants are to be chosen from those who had the lower scores at 11!

From among all these, a choice of the best is to be made. In practice, the availability of suitable teachers and the necessity of dividing children between existing school buildings will impose some constraints, but these should be regarded primarily as inhibitors of sudden large changes. The aim of the present analysis is to seek the optimal steady state; if values of P_1, P_2, P_2^* very different from those now existing were proved desirable, they could be achieved by slow changes.

How then is "best" to be defined? One reasonable definition would be implicit in a decision to maximize the mean value of the "university suitability" for the university entrants, or rather to maximize its statistical expectation under repeated trial: the difference is small because of the large number of individuals concerned. The inevitable concomitant of this is that the mean value of x for those not securing admission to a university is minimized.

A proposal to judge the merits of an educational system primarily by the success with which it enables the universities to receive the intellectual cream might be criticized as paying too little attention to the needs of the great majority of the less gifted. An alternative, perhaps less open to misrepresentation, would be to state the ideal situation as that in which the division between those who enter a university and those who do not is made exactly correctly for the proportion π. In other words, a perfect allocation is that in which every child for whom $x \geqslant \sigma T(\pi)$ is admitted to a university and others are not; within the limitations of the simple model and a predetermined value of π, each child eventually receives the education for which he is best suited. Errors of allocation are then of two types: a child with $x \geqslant \sigma T(\pi)$ may fail to secure admission and a child with $x < \sigma T(\pi)$ may be admitted. However, if the proportion actually admitted is maintained at π, the two types of error must be equally frequent. "Best" is now taken to mean maximizing the expected proportion of correct allocations. As Dunnett (1960) has pointed out in a similar context, the two definitions are not as different as they may seem: the second maximizes the expected frequency with which x in the final selection exceeds $\sigma T(\pi)$, whereas the first can be regarded as maximizing the average amount by which individuals exceed $\sigma T(\pi)$. The one procedure simply counts correct allocations, whereas the other uses a weighted mean in which the weight is the deviation of x from $\sigma T(\pi)$; evidence presented in Section 8 suggests that the two may lead to very similar conclusions.

The problem is now completely specified. For any ρ_1, ρ_2, ρ_y, and π, mathematical and numerical analysis can be directed at finding the allocation parameters, P_1, P_2 and P_2^*, that unconditionally maximize either the mean value of x or the proportion of correct allocations. No account has been taken of costs or any other factors that may affect the relative numbers who are to experience different kinds of secondary education. Perhaps this is right; some would object strongly to any suggestion that the allocation of an individual to one of two alternative educational

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streams should be influenced by their relative costs. If the aim of maintaining alternatives of equal esteem is achieved, the contention that, irrespective of costs, each child should be allocated to the stream best suited to him has great force. If grammar schools both have higher prestige and cost appreciably more per child, any proposal to increase P_1 would inevitably raise questions about the justification for generosity to marginal cases. Again, it could happen that the unrestricted optimal involved a value of P_1 very much greater than π, so that inevitably a high proportion of grammar school entrants were later disappointed of their university hopes, which might be regarded as psychologically bad for the unsuccessful. This aspect of the general problem perhaps becomes more prominent if a third stage is introduced into the system, the possibility of dismissal from the university if performance during the first year is unsatisfactory. A university would probably find obvious attractions, and equally obvious disadvantages, in having a large number of admissions from whom a selection could be made at the end of the first year, so that those who eventually graduate have been selected largely on internal evidence of ability instead of on school examinations and records. An independent objection to such a policy might be the ill effects of discouraging large numbers of young men and women, who would secure university admission and then be classed as failures after only one year. Which is the better on balance, to admit say 5 per cent. of an age group and ensure that all but a very few graduate, or to admit 10 per cent., half of whom will be asked to leave after a year but the other half who continue including some individuals who were not placed in the top 5 per cent. at admission? Attention is drawn to such difficult questions of educational policy here only in order that the possibility of modifying the statistical investigation by taking into account additional constraints may be mentioned. For example, in a model such as that implicit in equation (10.1), the value of $\{P_1P_2+(1-P_1)P_2^*\}$ might have an upper limit; this would require replacement of the unconditional by an appropriate conditional maximization.

7. The Correlation Coefficients

This paper is a study in methodology, and not primarily a presentation of numerical answers. To discover reasonable values for the three population correlation coefficients ρ_1, ρ_2, ρ_{y} has proved surprisingly difficult. However, in order to illustrate the method, calculations have been made to specifications that are intended to be not too unrealistic.

The only estimates that could be found from the literature are based upon the work of Nisbet and Buchan (1959); for 102 students who entered the University of Aberdeen in 1953, they presented information on correlations between scores on which grammar school and university admissions were based with results of degree examinations. To identify x with a measure of performance in final degree examinations is far from satisfactory, but no alternative is at present available and, for the sake of the present discussion, x is assumed at any rate to have about the same correlation structure with y_1, y_2 as do these examination marks. Rough averages from Nisbet and Buchan then seem to be

$$\left.\begin{aligned}\rho(x, y_1) &= 0{\cdot}25,\\ \rho(x, y_2) &= 0{\cdot}30,\\ \rho(y_1, y_2) &= 0{\cdot}25,\end{aligned}\right\} \tag{7.1}$$

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within a university population derived by two-stage selection under conditions that are perhaps approximately described by $P_1 = 0{\cdot}2$, $P_2 = 0{\cdot}2$, and P_2^* very small.

Equations (3.10) to (3.12) can be applied to the two stages successively, so as to relate these correlation coefficients within the university to ρ_1, ρ_2, ρ_y for a whole age group. Solution of the equations leads to

$$\left.\begin{aligned}\rho_1 &= 0{\cdot}70,\\ \rho_2 &= 0{\cdot}75,\\ \rho_y &= 0{\cdot}70,\end{aligned}\right\} \tag{7.2}$$

as round figures. It is only fair to Nisbet and Buchan to mention that they explicitly declined to quote estimates for the population, in view of the small numbers in their university study and the consequent imprecision of their correlation coefficients within the university. Without imputing to them the responsibility, however, if any progress with this problem is to be made, extrapolation must be risked and used until such time as further studies provide more precise values. Such extrapolation may be in gross error if the various regressions involved are not linear, as would be the case, for example, if above a certain value of x the scores y_1 and y_2 are insensitive to differences in x because good performance in school examinations by children of this ability is nearly certain; heterogeneity of variances might also have serious effects.

The numerical values just obtained have the property of implying a partial correlation coefficient between y_1 and y_2, with x held constant, of magnitude

$$\frac{0{\cdot}70 - 0{\cdot}70 \times 0{\cdot}75}{(0{\cdot}51 \times 0{\cdot}4375)^{\frac{1}{2}}} = 0{\cdot}37. \tag{7.3}$$

Thus the two scores have characteristics in common independently of their correlation with x. This is not surprising, but an alternative situation in which this partial correlation coefficient is zero also deserves consideration. By approximate calculations based upon equations (3.10) to (3.12), but constraining the partial correlation coefficient to be zero, conditions not seriously at variance with Nisbet and Buchan's results, the values

$$\left.\begin{aligned}\rho_1 &= 0{\cdot}55,\\ \rho_2 &= 0{\cdot}60,\\ \rho_y &= 0{\cdot}33,\end{aligned}\right\} \tag{7.4}$$

were obtained. At first glance, these are unrealistic in meaning that scores at 11+ are more highly correlated with suitability for a university than they are with the scores on which university admission is based. Although this is perhaps an unlikely state of affairs, equations (7.4) do represent a lower extreme in which the scores at two ages relate to very different aspects of individual ability and their consequences merit comparison with those from equations (7.2).

8. The Algebra of Selection

If it be assumed that the distribution of x, y_2 within each of the two sub-populations created by the dichotomy at the first stage can be approximated by the bivariate normal, as will be true when neither P_1 nor Q_1 is very small and ρ_2, ρ_y are not very large, the formulae in Section 3 can be applied a second time in order to study properties of x in the sub-populations created at the second stage. There are now four distinguishable sub-populations, hereafter labelled f, f^*, g, g^*. A fraction $P_1 P_2$ of

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the age group, known as f, is selected from the grammar schools as having the highest values of y_2. Its expectation for x is

$$E_f \equiv E(x) = \sigma\left[\rho_1 \nu(P_1) + \frac{\rho_2 - \rho_1\rho_y \nu'(P_1)}{\{1-\rho_y^2 \nu'(P_1)\}^{\frac{1}{2}}} \nu(P_2)\right], \tag{8.1}$$

and the variance of individuals within it is

$$V_f \equiv V(x) = \sigma^2\left[1 - \rho_1^2 \nu'(P_1) - \frac{\{\rho_2 - \rho_1\rho_y \nu'(P_1)\}^2}{1-\rho_y^2 \nu'(P_1)} \nu'(P_2)\right]. \tag{8.2}$$

Similarly, the fraction $(1-P_1)P_2^*$, known as f^*, selected for university admission from those who had the lower y_1 scores at the first stage, has

$$E_{f^*} = \sigma\left[-\rho_1 \nu(Q_1) + \frac{\rho_2 - \rho_1\rho_y \nu'(Q_1)}{\{1-\rho_y^2 \nu'(Q_1)\}^{\frac{1}{2}}} \nu(P_2^*)\right], \tag{8.3}$$

and

$$V_{f^*} = \sigma^2\left[1 - \rho_1^2 \nu'(Q_1) - \frac{\{\rho_2 - \rho_1\rho_y \nu'(Q_1)\}^2}{1-\rho_y^2 \nu'(Q_1)} \nu'(P_2^*)\right]. \tag{8.4}$$

Analogous expressions are easily written for g, the fraction $P_1(1-P_2)$, consisting of the grammar school pupils who do not secure university admission, and for g^*, the fraction $(1-P_1)(1-P_2^*)$, constituting the remainder of the age group.

For the whole university intake, the mean and variance will therefore be

$$E_\pi = \{P_1P_2E_f + (1-P_1)P_2^*E_{f^*}\} \div \pi, \tag{8.5}$$

$$V_\pi = \left\{\frac{P_1P_2(1-P_1)P_2^*}{\pi}(E_f - E_{f^*})^2 + P_1P_2V_f + (1-P_1)P_2^*V_{f^*}\right\} \div \pi. \tag{8.6}$$

TABLE 1

Values of E_π/σ from equation (8.5), for $\pi = 0{\cdot}1$ and the correlation coefficients in equations (7.2)

P_2^* \ P_1	0·08	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·00	—	1·228	1·311	1·353	1·351	1·321	1·298	1·252	1·316
0·01	—	1·336	1·374	1·383	1·370	1·320	1·290	1·239	1·316
0·02	—	1·386	1·402	1·386	1·365	1·302	1·268	1·220	1·316
0·05	1·412	1·400	1·381	1·320	1·284	1·202	1·167	1·154	1·316
0·10	1·142	1·116	1·088	1·023	0·991	0·937	0·927	1·023	1·316

Tables 1 to 4 show E_π for $\pi = 0{\cdot}1$ and $\pi = 0{\cdot}04$, and for the sets of correlation coefficients given as equations (7.2) and (7.4). The second value of π is of the order of magnitude of current British practice, the first is a proportion of university admissions at which Britain may aim; both have been chosen here because of convenience for existing tables of ν and its derivatives. For these tables, particular pairs of P_1, P_2^* have been taken and the corresponding P_2 (not shown in the tables) calculated from equation (5.1). Higher values of P_2^* could have been included without violating the mathematical form of the model, but discussion of any situation in which P_2^* exceeds

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P_2 would scarcely be realistic and could only lead to values of E_π smaller than those tabulated. In Tables 1 to 9 and 16, the highest value of P_2^* tabulated is equal to π, which corresponds to $P_2^* = P_2$ and an indeterminate value for P_1.

TABLE 2

Values of E_π/σ from equation (8.5), *for $\pi = 0{\cdot}1$ and the correlation coefficients in equations* (7.4)

P_2^* \ P_1	0·08	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·00	—	0·965	1·071	1·152	1·165	1·159	1·142	1·076	1·053
0·01	—	1·079	1·139	1·188	1·191	1·167	1·143	1·070	1·053
0·02	—	1·138	1·175	1·198	1·194	1·159	1·131	1·059	1·053
0·05	1·179	1·186	1·184	1·161	1·142	1·091	1·064	1·015	1·053
0·10	0·973	0·962	0·949	0·922	0·908	0·885	0·881	0·922	1·053

TABLE 3

Values of E_π/σ from equation (8.5), *for $\pi = 0{\cdot}04$ and the correlation coefficients in equations* (7.2)

P_2^* \ P_1	0·025	0·032	0·04	0·05	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·000	—	—	1·508	1·595	1·659	1·657	1·634	1·616	1·566	1·540	1·507	1·616
0·002	—	—	1·582	1·642	1·685	1·679	1·647	1·624	1·565	1·535	1·499	1·616
0·004	—	—	1·628	1·674	1·699	1·689	1·648	1·622	1·557	1·525	1·490	1·616
0·010	—	1·669	1·710	1·726	1·708	1·688	1·630	1·595	1·517	1·481	1·457	1·616
0·020	1·728	1·738	1·735	1·726	1·660	1·628	1·550	1·508	1·419	1·385	1·395	1·616
0·040	1·518	1·500	1·480	1·458	1·370	1·335	1·256	1·217	1·150	1·138	1·256	1·616

TABLE 4

Values of E_π/σ from equation (8.5), *for $\pi = 0{\cdot}04$ and the correlation coefficients in equations* (7.4)

P_2^* \ P_1	0·025	0·032	0·04	0·05	0·1	0·125	0·2	0·25	0·4	0·5	0·08	1·0
0·000	—	—	1·185	1·298	1·420	1·435	1·440	1·433	1·398	1·372	1·299	1·293
0·002	—	—	1·260	1·344	1·445	1·456	1·454	1·443	1·401	1·372	1·296	1·293
0·004	—	—	1·309	1·377	1·459	1·466	1·457	1·444	1·397	1·367	1·291	1·293
0·010	—	1·338	1·401	1·436	1·473	1·470	1·446	1·427	1·372	1·339	1·271	1·293
0·020	1·405	1·433	1·446	1·452	1·438	1·424	1·384	1·359	1·300	1·271	1·230	1·293
0·040	1·247	1·239	1·230	1·220	1·181	1·166	1·131	1·115	1·087	1·082	1·131	1·293

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Inspection of the tables shows surprisingly little variation over wide ranges of values of the three allocation parameters. In any one table, the function E_π is evidently flat near the maximum. Nevertheless, the position of the maximum can be roughly located for each table, although that for Table 1 is perhaps just outside the range of values tabulated.

TABLE 5

Approximate percentages of university entrants having $x > 1{\cdot}282\sigma$, *for* $\pi = 0{\cdot}1$ *and the correlation coefficients in equations* (7.2)

P_2^* \ P_1	0·08	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·00	—	45·3	49·2	51·4	51·4	50·1	49·1	47·6	49·2
0·01	—	50·4	52·3	52·8	52·1	49·8	48·5	47·1	49·2
0·02	—	52·9	53·6	52·6	51·6	48·7	47·4	46·6	49·2
0·05	53·9	53·1	52·0	48·7	47·0	43·9	43·1	44·7	49·2
0·10	38·1	36·8	35·3	32·8	31·9	32·2	33·7	41·5	49·2

TABLE 6

Approximate percentages of university entrants having $x > 1{\cdot}282\sigma$, *for* $\pi = 0{\cdot}1$ *and the correlation coefficients in equations* (7.4)

P_2^* \ P_1	0·08	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·00	—	35·0	38·8	42·4	43·0	42·8	42·1	39·4	38·2
0·01	—	39·3	41·8	43·9	44·1	43·0	42·0	39·2	38·2
0·02	—	41·7	43·3	44·3	44·1	42·6	41·4	38·8	38·2
0·05	43·5	43·8	43·7	42·6	41·8	39·7	38·7	37·4	38·2
0·10	34·0	33·5	33·0	32·0	31·7	31·6	32·0	34·8	38·2

In order to examine also the alternative criterion suggested in Section 6, rough estimates of the expected percentages of incorrect allocations have been formed for each of f, f^*, g, g^*. For this purpose the distribution of x in f has been taken as normal with mean E_f and variance V_f, and the proportion of such a distribution falling below $\sigma T(\pi)$ obtained from standard tables, with similar calculations for f^*, g, g^*. This involves a more serious approximation, since departures from normality will be greater after two stages of selection, and not surprisingly the figures obtained for frequencies of errors of the two types are less close to equality than was hoped. There seems to be little point in discussing this discrepancy in any detail when the normal distributions used are obviously crude approximations. Perhaps the broad sense can be conveyed by saying that the proportion of the whole population wrongly placed in g or g^* (i.e. wrongly denied university admission) appeared consistently greater than the proportion wrongly placed in f or f^* (i.e. wrongly granted admission), the ratio of the two proportions being of the order of 1·1 for conditions used in forming Tables 1 and 3, of the order of 1·05 for Tables 2 and 4. However, the pairs have been averaged and, in Tables 5 to 8, expressed as the percentages of university entrants

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whose true values of x lie above the ideal point of division, $\sigma T(\pi)$. For example, for Table 6, with $P_1 = 0{\cdot}2$, $P_2 = 0{\cdot}42$, $P_2^* = 0{\cdot}02$, it was estimated that a proportion 0·1113 of the whole population is wrongly allocated, this being totalled from 0·0542 wrongly in f and f^* and 0·0571 wrongly in g and g^*; hence the fraction 0·1 admitted to universities includes 0·0575 for whom $x < 1{\cdot}282\sigma$, and therefore the proportion *of the university entrants* with $x \geqslant 1{\cdot}282\sigma$ is $(0{\cdot}1 - 0{\cdot}0557)/0{\cdot}1$, or 44·3 per cent.†

TABLE 7

Approximate percentages of university entrants having $x > 1{\cdot}751\sigma$, *for* $\pi = 0{\cdot}04$ *and the correlation coefficients in equations* (7.2)

P_2^* \ P_1	0·025	0·032	0·04	0·05	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·000	—	—	33·6	36·9	39·7	39·8	39·2	38·7	37·6	37·3	37·2	37·5
0·002	—	—	36·5	38·9	40·7	40·5	39·4	38·8	37·4	36·9	37·1	37·5
0·004	—	—	38·3	40·2	41·1	40·8	39·3	38·5	37·0	36·5	36·9	37·5
0·010	—	40·0	41·7	42·2	41·0	40·1	38·0	36·9	35·3	35·0	36·2	37·5
0·020	42·3	42·5	42·2	41·6	38·0	36·6	34·0	32·9	31·8	32·1	35·1	37·5
0·040	29·8	28·5	27·4	26·1	22·5	21·5	20·5	20·6	22·9	25·2	32·9	37·5

TABLE 8

Approximate percentages of university entrants having $x > 1{\cdot}751\sigma$, *for* $\pi = 0{\cdot}04$ *and the correlation coefficients in equations* (7.4)

P_2^* \ P_1	0·025	0·032	0·04	0·05	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·000	—	—	23·9	26·8	31·3	32·0	32·3	32·1	31·1	30·4	28·6	26·8
0·002	—	—	26·0	28·5	32·1	32·6	32·6	32·3	31·1	30·3	28·4	26·8
0·004	—	—	27·4	29·6	32·6	32·9	32·7	32·3	31·0	30·1	28·3	26·8
0·010	—	28·4	30·4	31·7	33·1	33·0	32·1	31·5	29·9	29·1	27·8	26·8
0·020	30·6	31·6	32·1	32·2	31·6	31·1	29·7	29·0	27·6	27·1	26·9	26·8
0·040	23·8	23·4	23·0	22·6	21·3	20·8	20·5	20·5	21·2	21·9	25·1	26·8

At first sight, these tables are very disturbing, for they appear to indicate that scarcely more than 40 to 50 per cent. of those who are most suitable for university education will be admitted under the systems here described! Of course, the trouble lies largely in the marginal cases. If 10 per cent. of the population is to be taken into universities, $x = 1{\cdot}282\sigma$ represents the ideal point of division, so that admission of one person with $x = 1{\cdot}290\sigma$ will be classed as correct and admission of another with $x = 1{\cdot}280\sigma$ will be classed as an error; the difference between these two persons is really unimportant, and the two will have almost equal probabilities of success in the two-stage selection. For any set of allocation parameters, a curve could be constructed to show the probability of admission as a function of x, the operating characteristic

† Of course, these figures can be presented in ways that put the emphasis very differently. "Only 44·3 per cent. of those worthy of university education are admitted" but "Of the whole age-group, 88·9 per cent. is correctly allocated in respect of university admission."

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of the system, and although this will approach unity for large x, zero for small x, it is bound to show poor discrimination in the neighbourhood of $x = \sigma T(\pi)$. Although further study of operating characteristics is desirable, it seems sufficient to note here the near coincidence of the positions of the maxima in corresponding tables from Tables 1 to 4 and Tables 5 to 8. Since maximization of E_π is the easier to examine computationally, concentration of effort on it could still be justified even if it were to be regarded only as an approximation to a preferred maximization of the proportion of correct allocations.

TABLE 9

Values of V_π/σ^2 from equation (8.6), for $\pi = 0{\cdot}1$ and the correlation coefficients in equations (7.2)

P_2^* \ P_1	0·08	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·00	—	0·593	0·514	0·478	0·472	0·468	0·470	0·490	0·533
0·01	—	0·541	0·501	0·472	0·468	0·470	0·477	0·507	0·533
0·02	—	0·508	0·487	0·472	0·473	0·486	0·498	0·534	0·533
0·05	0·506	0·506	0·509	0·524	0·536	0·572	0·595	0·634	0·533
0·10	0·574	0·582	0·592	0·626	0·649	0·722	0·768	0·825	0·533

Even those admitted to universities after the second stage of selection will vary widely in their values of x. To the order of approximation used in Tables 1 to 4, the variance of x in the sub-population formed by f and f^* is easily computed from equation (8.6). Table 9 shows this variance for the systems summarized in Table 1, and indicates that at best only about half the variance in the whole population has been eliminated by the selection process. Even though Table 3 corresponds to much more intensive selection, the variance of those finally selected remains large, never less than $0{\cdot}44\sigma^2$; under the conditions of Tables 2 and 4, the situation is even more disappointing, the minimal variances being about $0{\cdot}60\sigma^2$ and $0{\cdot}58\sigma^2$ respectively. Only Table 9 is presented here, and the approximation may not be good, but it is worth noting that the system of P_1, P_2, P_2^* that maximizes E_π seems always to have V_π appreciably above its minimum.

The assumption that normality will not be seriously disturbed by the first stage of selection generally leads to exaggeration of the value of $E(x)$ in the university population, $(f+f^*)$. By insertion of the general cumulants stated in Section 3 into the series obtained elsewhere (Finney, 1961), improved approximations have been obtained. The result for E_f is

$$E_f = \sigma\rho_1\nu(P_1) + \sigma\nu(P_2)\left[\frac{\rho_2 - \rho_1\rho_y\nu'(P_1)}{\{1-\rho_y^2\nu'(P_1)\}^{\frac{1}{2}}}\right.$$
$$+\frac{\rho_y^2\nu''(P_1)T_2}{6\{1-\rho_y^2\nu'(P_1)\}^2}\{3\rho_1 - 2\rho_2\rho_y - \rho_1\rho_y^2\nu'(P_1)\}$$
$$-\frac{\rho_y^3\nu'''(P_1)(T_2^2-1)}{24\{1-\rho_y^2\nu'(P_1)\}^{\frac{5}{2}}}\{4\rho_1 - 3\rho_2\rho_y - \rho_1\rho_y^2\nu'(P_1)\}$$
$$\left.-\frac{\rho_y^5\{\nu''(P_1)\}^2(2T_2^2-1)}{36\{1-\rho_y^2\nu'(P_1)\}^{\frac{7}{2}}}\{6\rho_1 - 5\rho_2\rho_y - \rho_1\rho_y^2\nu'(P_1)\}\right], \qquad (8.7)$$

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where T_2 is written for $T(P_2)$. The first additional term multiplying $\sigma\nu(P_2)$ is a first order improvement, the second and third additional terms together are a second order improvement. From the general series (Finney, 1961), the terms for two further orders of approximation can be formed easily, but numerical computation with them would be very laborious; for this paper, only the first additional term has been used. If more extreme selection were under examination, further terms would be needed, and indeed the approximation may be barely good enough for $\pi = 0{\cdot}04$. The corresponding series for E_{f^*} must of course have Q_1, P_2^* in place of P_1, P_2 respectively, and also the signs of

TABLE 10

Values of E_π/σ with first order adjustment, for $\pi = 0{\cdot}1$ and the correlation coefficients in equations (7.2)

P_2^* \ P_1	0·08	0·1	0·125	0·16	0·2
0·012	—	—	—	1·3692	1·3680
0·016	—	1·3422	1·3655	1·3714	1·3663
0·020	—	1·3548	1·3706	1·3711	1·3624
0·024	—	1·3638	1·3729	1·3686	1·3566
0·028	1·3457	1·3696	1·3728	1·3642	1·3491
0·032	1·3582	1·3724	1·3705	1·3578	1·3400
0·036	1·3660	1·3725	—	—	—
0·040	1·3700	1·3700	—	—	—

TABLE 11

Values of E_π/σ with first order adjustment, for $\pi = 0{\cdot}1$ and the correlation coefficients in equations (7.4)

P_2^* \ P_1	0·125	0·16	0·2	0·25
0·012	—	—	—	1·1899
0·016	1·1570	1·1822	1·1913	1·1908
0·020	1·1680	1·1875	1·1931	1·1898
0·024	1·1761	1·1906	1·1929	1·1871
0·028	1·1817	1·1916	1·1909	1·1828
0·032	1·1850	1·1907	1·1872	1·1770
0·036	1·1860	—	—	—

$\nu(Q_1), \nu''(Q_1)$ must be reversed (those of $\nu'(Q_1), \nu'''(Q_1)$ being left to correspond with those of the functions of P_1), in accordance with equations (3.13), (3.14). Equation (8.5) then gives the revised values of E_π, and for the interesting combinations of the allocation parameters these are shown in Tables 10 to 13.

Comparison of corresponding entries in Tables 1 to 4 and 10 to 13 shows the relatively small extent to which the crude approximation overestimates E_π. Nevertheless, this overestimation usually increases as P_1 decreases and P_2^* increases, so that the position of the maximum is shifted by the revision although the magnitude of E_π at

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its maximum differs little from what it was before. The change is more marked for the first set of correlations, equations (7.2), than for equations (7.4). Table 14 summarizes the comparisons but only very roughly; the function E_π is always so flat in the neighbourhood of the optimal that any careful interpolation seems a waste of effort.

TABLE 12

Values of E_π/σ with first order adjustments, for $\pi = 0{\cdot}04$ and the correlation coefficients in equations (7.2)

P_2^* \ P_1	0·032	0·04	0·05	0·08	0·1
0·002	—	—	—	—	1·6779
0·004	—	—	—	—	1·6831
0·006	—	—	—	1·6875	1·6835
0·008	—	—	1·6816	1·6882	1·6801
0·010	—	—	1·6890	1·6853	1·6736
0·012	—	—	1·6923	—	—
0·014	—	1·6905	1·6919	—	—
0·016	1·6854	1·6918	—	—	—
0·018	1·6892	1·6891	—	—	—
0·020	1·6882	1·6826	—	—	—

TABLE 13

Values of E_π/σ with first order adjustments, for $\pi = 0{\cdot}04$ and the correlation coefficients in equations (7.4)

P_2^* \ P_1	0·05	0·08	0·1	0·125	0·2
0·002	—	—	1·4434	1·4543	1·4550
0·004	—	—	1·4553	1·4624	1·4568
0·006	—	—	1·4620	1·4658	—
0·008	—	1·4577	1·4650	1·4657	—
0·010	—	1·4608	1·4647	1·4625	—
0·012	—	1·4608	1·4615	—	—
0·014	1·4424	1·4578	—	—	—
0·016	1·4445	1·4521	—	—	—
0·018	1·4432	1·4438	—	—	—
0·020	1·4385	—	—	—	—

Tables 10 to 13 may be better appreciated if they are compared with what would be achieved if direct selection on x were possible. Selection of the top fraction $\pi = 0{\cdot}1$ of the x distribution would take all values of x exceeding $1{\cdot}2816\sigma$ and their mean would be $1{\cdot}7550\sigma$. Similarly, with $\pi = 0{\cdot}04$, all values of x would exceed $1{\cdot}7507\sigma$ and their mean would be $2{\cdot}1543\sigma$. Evidently even the optimal selection systems are not very satisfactory, those in Tables 11 and 13 giving values for E_π below the lower boundary of the best fraction π of the distribution of x. Although at first

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sight this may seem paradoxical, it is unfortunately to be expected when the scores are not very highly correlated with the criterion, x; in the extreme case of zero correlations, the distribution of x would be unaltered by selection, and E_π would be zero whatever the values of π and the allocation parameters. One way of summarizing the present numerical findings is to specify the fraction of the x distribution which would have E_π as its mean; for Tables 10 to 13, the optimals correspond with the means of fractions 0·210, 0·284, 0·114 and 0·177 from the top of the x distribution. Put differently, the expected means for x in those selected for the universities are what would be achieved in a simple one-stage selection based on a score whose correlation coefficient with x is 0·783, 0·680, 0·786 and 0·680 respectively.

TABLE 14

Optimal values (very approximate) of allocation parameters and of E_π/σ for Tables 1 to 4 and 10 to 13

Table	P_1	P_2	P_2^*	E_π/σ
1	0·08	0·675	0·05	1·412
10	0·125	0·618	0·026	1·374
2	0·2	0·420	0·02	1·198
11	0·2	0·412	0·022	1·194
3	0·032	0·645	0·020	1·738
12	0·05	0·553	0·013	1·693
4	0·1	0·310	0·010	1·473
13	0·125	0·271	0·007	1·466

An interesting feature of E_f, E_{f^*} and therefore of E_π, is that they are linear functions of ρ_1 and ρ_2, though not of course linear in ρ_y. This is a general result, which will extend to many forms of multi-stage selection. It follows because of the assumption that x, y_1, y_2 are jointly normal in distribution, so that x can be regarded as having a normal linear regression on y_1, y_2; E_π is therefore a linear function of the means of y_1, y_2 in the final selection. For example, choosing $P_1 = 0{\cdot}16$, $P_2 = 0{\cdot}499$, $P_2^* = 0{\cdot}024$ as allocation parameters close to the optimals in Tables 10, 11, and taking $\rho_y = 0{\cdot}7$, use of all the terms in equation (8.7) and those of next higher order gives

$$E_\pi = 0{\cdot}4425\rho_1 + 1{\cdot}4043\rho_2;$$

with $\rho_1 = 0{\cdot}7$, $\rho_2 = 0{\cdot}75$, the value of E_π is 1·363, instead of 1·369 as in Table 10. Equation (8.7), indeed, makes clear that a score for grammar school allocation not itself correlated with x (i.e. $\rho_1 = 0$) may still be useful as long as it is correlated with y_2; first stage allocation based upon such a score then reduces the variance of y_2 in the two parts of the population by removing some of the variation not associated with x, and so eventually improves the quality of discrimination at the second stage.

From the last paragraph, it is apparent that the whole scheme of calculations could be approached differently, by first finding $E(y_1)$ and $E(y_2)$ in f and f^*, this involving bivariate truncation of the distribution of y_1, y_2 at points determined by P_1, P_2 and by $(1-P_1), P_2^*$ respectively. Appropriate formulae have been given by

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Young and Weiler (1960) and by Tallis (1961). Then the corresponding expectations of x are obtainable from the regression on y_1, y_2. Although exact formulae instead of approximations by series would be used, the computational labour might be increased by the necessity for interpolation in tables of the normal integral, especially as probabilities for the bivariate normal have been tabulated only at wide intervals.

As is apparent from equation (8.7), E_π is not linear in ρ_{yj}, and some interest attaches to the nature of the dependence on ρ_{yj}. Of course, the value of ρ_{yj} sets limitations on the possible values of ρ_1, ρ_2; for example, if $\rho_1 = \theta\rho_2$ where $0 \leqslant \theta \leqslant 1$, then ρ_2 must lie between the limits

$$\{\theta\rho_{yj} \pm (1 + \theta^2 - \rho_{yj}^2)^{\frac{1}{2}}\}/(1 + \theta^2);$$

these take the values $\pm(1+\theta^2)^{-\frac{1}{2}}$ at $\rho_{yj} = 0$, increase steadily to $-(1-\theta^2)/(1+\theta^2)$, 1·0 at $\rho_{yj} = \theta$, and the range contracts to $0, 2\theta/(1+\theta^2)$ at $\rho_{yj} = 1{\cdot}0$. A small study of the extent to which the magnitude of the correlation between the two scores influences E_π has been made on four sets of allocation parameters, all corresponding to $\pi = 0{\cdot}1$. These are

	P_1	P_2	P_2^*
(i)	0·080	0·675	0·050
(ii)	0·125	0·660	0·020
(iii)	0·160	0·499	0·024
(iv)	0·200	0·500	0·000

(8.8)

all of which are close to the optimal in Table 10. From equation (8.7) with only the first additional term included, it appears that if $\rho_1 = \rho_2$ the value of E_π declines steadily as ρ_{yj} increases from 0 to 1; on the other hand, if $\rho_1 = 0{\cdot}5\rho_2$, as ρ_{yj} increases E_π first decreases to a minimum at about $\rho_{yj} = 0{\cdot}5$ and then increases again until $\rho_{yj} = 1{\cdot}0$.

TABLE 15

Values of E_π/σ with first order adjustments, for $\pi = 0{\cdot}1$ and the sets of allocation parameters specified as (8.8)

	$\rho_1 = \rho_2 = 0{\cdot}6$				$\rho_1 = \frac{1}{2}\rho_2 = 0{\cdot}4$			
ρ_{yj}	(i)	(ii)	(iii)	(iv)	(i)	(ii)	(iii)	(iv)
0·0	1·300	1·319	1·361	1·319	1·361	1·232	1·352	1·198
0·1	1·271	1·289	1·321	1·283	1·343	1·213	1·327	1·176
0·2	1·242	1·260	1·283	1·250	1·327	1·197	1·306	1·158
0·3	1·214	1·232	1·246	1·220	1·315	1·185	1·290	1·144
0·4	1·186	1·205	1·211	1·192	1·306	1·177	1·279	1·136
0·5	1·159	1·179	1·178	1·165	1·303	1·174	1·275	1·133
0·6	1·133	1·153	1·145	1·140	1·307	1·178	1·280	1·136
0·7	1·108	1·128	1·114	1·116	1·322	1·192	1·299	1·150
0·8	1·085	1·103	1·084	1·094	1·353	1·225	1·338	1·181
0·9	1·065	1·081	1·059	1·074	1·417	1·297	1·419	1·243
1·0	1·053	1·068	1·046	1·064	1·596	1·502	1·601	1·392

The implications of this behaviour are far from clear, but it certainly illustrates the complexity of the dependence of E_π upon the various parameters of the relatively simple model whose main features are expressed by equation (5.1). Table 15 summarizes

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a selection from the calculations, in order to illustrate the trends for values of ρ_1, ρ_2 that are always possible. There can be no assurance that the sets of allocation parameters (i)–(iv) remain near to the optimal for different sets of correlations, and far more extensive calculations would be needed in order to examine the situation thoroughly; higher order terms in equation (8.7) are likely to assume great importance at extreme values of $\rho_{y\eta}$, but will perhaps scarcely affect the qualitative indications of Table 15.

From the calculations on which Table 15 is based, it is possible to evaluate reasonable upper limits to the consequences of selection. Since a first draft of this paper was prepared, Mr W. D. Furneaux has suggested to me that in some circumstances ρ_1 and ρ_2 might rise to 0·8 and 0·9 respectively, and they can scarcely reasonably be expected to be much higher. For such close correlations, $\rho_{y\eta}$ must lie between 0·46 and 0·98, and a value about 0·9 is suggested. With set (iii) of the allocation parameters in (8.8), E_π/σ takes the values 1·693 at $\rho_{y\eta} = 0{\cdot}5$, 1·624 at $\rho_{y\eta} = 0{\cdot}7$, and 1·591 at $\rho_{y\eta} = 0{\cdot}9$. Thus, under favourable conditions, a two-stage system of the kind under discussion, with $\pi = 0{\cdot}1$, might achieve $E_\tau/\sigma = 1{\cdot}60$, corresponding to the mean of the best fraction 0·137 of the population, but only if the correlation between scores could be kept low would it approach $E_\tau/\sigma = 1{\cdot}70$, corresponding to the mean of the best fraction 0·112. Other good sets of allocation parameters can scarcely be very different in result, and evidently even two stages of selection could approach closely to the ideal of $E_\pi/\sigma = 1{\cdot}75$.

9. General Comments

Earlier sections of this paper have elaborated a statistical approach to two-stage educational selection (or allocation!), and have illustrated this numerically by reference to the two stages of grammar school admission and university admission. I must again emphasize that what is being presented is an outline of a methodology for comparing alternative combinations of selective intensities, not a dogmatic assertion of the superiority of certain numerical values. Consideration of the theory and the numerical illustrations should help the reader to realize the need for clearer definition of the aims of selection in education, without which the search for an optimal selection programme cannot be begun, and the need for much greater knowledge of the magnitudes of intercorrelations between tests and other assessments at different ages. When these two needs have been met, the method described can be applied or adapted to the appropriate model.

Statistical analysis of this kind cannot tell how methods of education within schools or universities can be improved, nor is it concerned with either the need for such improvement or criteria for measuring improvement. The questions discussed presuppose that types of school and their effects on their pupils, diverse though both may be, remain unaltered under different systems of allocation.

Nothing I have said implies acceptance of any particular scheme of testing or examining pupils at either stage. The method can be applied by using any combination of marks and assessments at one stage as the score; evidently the quality of selection will benefit from defining the score so that it correlates as highly as possible with the ultimate criterion, but the problem of how to do this is not at present under discussion. Nor is there any implication that the first stage must fall at age 11, for the method would still apply were some other age chosen. The formulae shown and used in Section 8 do assume that two stages are needed and are sufficient for a good system, although the analysis could controvert the need for the first stage by showing the optimal to occur with $P_1 = 1{\cdot}0$.

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For the qualitative discussion of the problems that this paper presents, fairly rough approximations have seemed adequate, but modern techniques of high-speed computing would readily permit greater refinement if this were desired and if the population parameters were known with sufficient accuracy to make it worth while. Only large sample theory has been employed, leading to expectations of means after selection; in view of the large numbers of individuals involved, this is surely good enough, but of course within any relatively small sample of a few hundred the results of selection might differ appreciably from the expectations.

An evident need is for refinement in the concept of suitability for a university and in the definition of the criterion x. Put baldly, one cannot adequately discuss which members of a population ought to receive university education without definition of what ideally constitutes suitability, and one cannot define suitability without first agreeing on the purpose of universities; of course, no exclusion of multiplicity of purpose is implied. Certainly reliance upon performance in university examinations must denote both a failure to appreciate the statistical unreliability of examination marks and a complacency about purpose. What alternatives can be offered by those who have for long discussed the aims and ideals of universities?

Although Tables 5 to 8 are only approximate, they clearly show a disappointing performance of the two-stage selection: of those individuals who fall within the top fraction 0·1 or 0·04 in respect of the criterion x, more than half fail to secure university admission. As stated in Section 8, marginal cases will account for much of this trouble, but an immediate intuitive reaction to these tables is bound to be: "Increase π and take more of the population into the university, for it is better to ensure that more of the good students are admitted even at the cost of increasing the proportion of those who are unsuitable." This argument is in part fallacious, although it is related to a point mentioned at the end of Section 6 and again in Section 10. The logical approach seems to be first to define what level of the criterion separates the suitable from the unsuitable; the distribution of x is unlikely to have an antimode there, and consequently many members of the population will lie close on either side. Unless π, the proportion of the population admitted, is equated with the proportion having x greater than the ideal point of division, the total errors of allocation will be greater than they need be. However, if errors on marginal candidates (in either direction) are regarded as less serious than errors on those whose x is very large or very small, some form of weighting will be appropriate. The linear weighting involved in E_π seems a fair allowance to make, but an alternative could be tried; the chief difficulty would lie in securing agreement on the alternative!

Burt (1962) has written, "In a country like the United Kingdom, with a population of about 50 million, there must be at least 12,000 geniuses, in the sense defined by Galton. I estimate that the talents of more than half of them are virtually wasted—largely owing to the inadequacy of our methods of selection and training." This statement would be comparable with an analysis on the present lines with $\pi = 0{\cdot}00025$; probably tables analogous to Tables 5 to 9 at this extreme would be even more discouraging, but ability of a very high order is perhaps more likely to be detected than would be predicted from correlations such as those of Section 7. Even for the more modest selection intensities envisaged as appropriate to universities, improvement in methods of selection and training should be sought as strongly as an increase in π. Though the latter will reduce the errors of wrong rejection, it must also dilute the quality within the university and will possibly affect adversely the conditions experienced by the best.

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In Section 8 and elsewhere, a fixed value of π has been implicit in the argument, optimal values of P_1, P_2, P_2^* being sought subject to the constraint of a fixed π in equation (5.1). Any consideration of an optimal value for π must introduce entirely new factors, not least of which must be assessments of economic value to the community. An analogous but intrinsically easier problem in the context of plant breeding has been attacked elsewhere (Finney, 1960).

The propriety of assuming the second-stage score to have the same nature in the two sub-populations created at the first stage of selection is open to question. To apply exactly the same tests for assessing the standards of would-be university students whether or not they come from the grammar school stream is not essential and may not be desirable. In the absence of any information about appropriate scores, their distributions, and their correlations, however, a provisional assumption that y_2 for the two streams will have the same basic statistical parameters, without necessarily being identical in origin, seems reasonable.

10. Criticisms and Modified Models

The careful reader will have noticed neglect of one possibility of improving the two-stage selection in Section 8. At the first stage, only the first score (y_1) is available, and selection *must* be based upon it. At the second stage, not only is the second score (y_2) known for each individual but also y_1 is still known from past records; why should it not be used, so that the child is judged on the combination of the two? A statistical technique involving this could be devised, but perhaps the model would thereby lose in educational realism: the possibility of permitting a child to gain admission to a university because his excellent results at 11+ compensated for only indifferent success in G.C.E. will not appeal to many!

This leads naturally to another important criticism of the methodology of my paper. I have implicitly assumed that inherent qualities of the individual determine his university suitability and success without his being affected by schooling; the method does not require schools to be any more than convenient boxes for separating the bright students from the less bright and maintaining a provisional classification made at 11 until the child is old enough for universities to operate the second stage of selection. This could be true, but would indeed be a severe indictment of the effects of education! In reality one would hope that exposure of a child to the educational experience of a grammar school will tend to increase his suitability for a university. A simple modification to the model would be to suppose that each member of the fraction P_1 selected at the first stage has his x increased to $(x+u)$ because of his subsequent enjoyment of a grammar school environment. Possibilities here are:

(i) u is constant;
(ii) u is a function of x, perhaps $u = a+bx$;
(iii) u has a frequency distribution.

Doubtless the aim should be to have P_1 sufficiently large to keep negligibly small the danger that a child fails to enter a university because he was earlier denied the possibility of gaining u.

The optimal allocation parameters, P_1, P_2, P_2^*, and the optimal division of university entrants between the grammar school and non-grammar school stream are very sensitive to u. The consequences of the simplest case, a constant u, are easily seen. Evidently E_f will be increased by u and $E_{\bar{f}}$ unaltered, so that E_π is increased by

$$uP_1P_2/\pi.$$

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Table 16 shows the behaviour of this function for $\pi = 0{\cdot}1$, and thus for Tables 1, 2, 10, 11. Whatever the value of u, the addition to E_π will tend to move the maximum towards higher values of P_1 and lower values of P_2^*. For example, if $u = 0{\cdot}2\sigma$, the maximum of Table 1 moves from $P_1 = 0{\cdot}08$, $P_2 = 0{\cdot}675$, $P_2^* = 0{\cdot}05$ to approximately $P_1 = 0{\cdot}125$, $P_2 = 0{\cdot}660$, $P_2^* = 0{\cdot}02$; $u = 0{\cdot}5\sigma$ would take the maximum to $P_2^* = 0$ and approximately $P_1 = 0{\cdot}2$, $P_2 = 0{\cdot}5$. Qualitatively this is what would be intuitively expected for any reasonable form of u: if the child benefits from the grammar school by improvement in university suitability, then to increase the proportion of children exposed to the grammar school and to reduce or even to eliminate P_2^* will be advantageous. Quantitatively, it makes clear that, once the existence of an influence of type of secondary school on individual suitability for university education is conceded, information on the magnitude of this influence is essential to any objective study of optimal selection. Although the desirability of giving prospective university students a more academic pattern of secondary schooling than their less able contemporaries receive has long been asserted, what quantitative knowledge is there of the benefit from this? By how much are the bright child's chances reduced if ill luck in a test takes him to the wrong type of school?

TABLE 16

Values of $P_1 P_2/\pi$ *when* $\pi = 0{\cdot}1$

P_2^* \ P_1	0·08	0·1	0·125	0·2	0·25	0·4	0·5	0·8	1·0
0·00	—	1·000	1·000	1·000	1·000	1·000	1·000	1·000	1·000
0·01	—	0·910	0·912	0·920	0·925	0·940	0·950	0·980	1·000
0·02	—	0·820	0·825	0·840	0·850	0·880	0·900	0·960	1·000
0·05	0·540	0·550	0·562	0·600	0·625	0·700	0·750	0·900	1·000
0·10	0·080	0·100	0·125	0·200	0·250	0·400	0·500	0·800	1·000

Of course, y_2 is also likely to be affected by the type of secondary school experienced. Simple addition of a constant to y_2 would not modify the present argument, since exactly the same individuals would be in the best fraction P_2, but any more complicated increment in y_2 might have a greater differential effect. If the increment in y_2 is stochastic rather than deterministic, the whole problem will become much more difficult because of the different possible patterns of correlation between y_2, x, and u. This complex of questions seems fundamental to any real understanding of the relative merits of different systems of secondary education, yet I doubt whether it is adequately recognized by those who determine policy or whether real attempts have been made to provide quantitative answers for even the simpler cases.

This paper has been written around a model using two stages of selection. Such a model may indeed be inadequate to describe either what happens today or what ought to happen, but it can easily be extended to include more stages. For example, a three-stage model could easily be formed in an obvious manner with

$$\pi = \{P_1 P_2 + (1-P_1) P_2^*\} P_3 + P_1 (1-P_2) P_3^* \qquad (10.1)$$

as its basis. Examination of this would involve the same kind of calculations, though they would be vastly more laborious, especially as the consequences of non-normality

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might be more serious. For a fixed π, the value of $E(x)$ in the final upper selection will increase as the number of stages is increased, though the gain by going beyond three stages is likely to be small.

Perhaps of even greater interest would be a model appropriate to a system of large comprehensive secondary schools, into which all children go without first-stage selection. This is quite distinct from the simple one-stage system obtained by putting $P_1 = 1{\cdot}0$ in equation (5.1). The essential feature is that, within a school, streams of different ability are maintained, so that the upper streams either make more rapid progress or follow an enriched curriculum. Annually, or even more often, the position of each child is reviewed in terms of some score representing his current performance, and he may then be moved up or down to the next stream. A "simple" model would be one with only two streams, for proportions π and $(1-\pi)$; at each review time, a small fraction, p, of the whole population would be moved up from the lower stream and an equal fraction moved down from the upper stream. Generalizations to values of π and p that change during the school life of a cohort and to several streams are easily stated, but even the simple case is probably too complicated for mathematical analysis. To examine such models may become important, for comparison of their operating characteristics with those of the two-stage process in Section 5 might begin to bring some objectivity into discussion of the merits of comprehensive schools, and perhaps recourse must be had to computer simulations. One may guess that a nearly continuous sifting process of this kind could give values of $E(x)$ for the final upper selection appreciably higher than obtainable by simple two- or three-stage systems, but of course many non-statistical and purely educational factors would also have to be considered.

Some readers may be reluctant to accept the assumption that selection or allocation at any stage is based upon a single score for the individual, instead of simultaneously employing measurements of several qualities. However, the oversimplified model discussed provides only two alternative decisions at each stage; for this situation, even though many different measurements may be available (Section 3), any reasonable rule of allocation must be equivalent to the formation of a function of these as a composite score followed by a dichotomy in the right proportions so that high values go to one alternative and low to the other. Only when individuals are to be allocated between three or more categories can methods for achieving a division optimal in respect of scores for two or more dimensions of ability be distinguished from purely hierarchical selection—that is to say, selection that divides the population between categories solely on the basis of the interval within which a composite score falls. For example, even if the proportionate allocation between grammar, technical, and modern schools were exactly specified by external factors, the ideal manner in which these proportions would be taken from the population would still need definition; merely to propose placing the ablest 20 per cent. (judged on a single score) in the grammar schools, the next ablest 25 per cent. in the technical, and the remaining 55 per cent. in the modern would not suffice. Some combinations of abilities will obviously indicate the desirability of one type of school in preference to either of the others, but inevitably others will be marginal in respect of two or of all three.

A simple illustration will show how difficulties increase when such generalizations are discussed; both aims and criteria are difficult to define and the mathematical and computational complexities are much greater. Suppose that in a one-stage system children are to be allocated between three categories, and the suitability of any one child is represented by quantities x_1, x_2 (instead of simply x). Then a plane in which

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(x_1, x_2) are two coordinates needs to be divided into regions so that each region contains combinations of (x_1, x_2) regarded as ideal for allocations to one of the available alternatives. This division needs to put assigned proportions $\pi_1, \pi_2, \pi_3 (\Sigma\pi = 1)$ in the three categories, and the method of drawing the boundaries ought to be based upon a rule that specifies the ideal allocations whatever arbitrary values are assigned to the π_i. Only then is the specification advanced as far as in Sections 3 and 4. Next must come agreement on what criterion of good allocation is to be adopted; is this to lead to maximization of the total proportion of correct allocations, or is some weight to be attached to each (x_1, x_2) after which a weighted total is maximized (Section 6)? Presumably at least two scores will be available for the one stage of allocation, and all inter-correlations of these and x_1, x_2 are needed (Section 7). Computations analogous to those of Section 8 can then begin. Obviously similar problems arise in any attempt to separate educational provisions into sub-categories, for example by recognizing that different university subjects have different requirements and success in them may be differently related to school scores. The analysis of Sections 3 to 8 is only a superficial, though I believe a necessary, first step to the whole question of guiding each child into his ideal educational channel.

Although this geometrical and mathematical representation might seem an unreal abstraction to the educationalist, I suggest that any policy of allocating children between three or more types of school (or between universities, technical colleges, and no higher education), in such a way that each child is intended to receive the treatment best suited to him, must involve something of this kind. The essential problems are the same whether the answers given are based on the intuition of the wise educationalist or on the type of numerical analysis outlined here, and both approaches to solving them have their part to play. Certainly I have no intention of belittling the first, but I suggest that more intensive study of the second could bring objectivity into arguments that today are often left largely to the emotions. Recognition of the logical relations between aims, criteria, and operational success of an allocation scheme and discussion of their inherent statistical complexity does not mean rigid adherence to a policy that neglects the human values of particular cases; however imperfect the statistical analysis, it should at least be corrective of loose thinking about aims and the possibilities of their fulfilment.

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I must also thank my colleague, Mr G. J. Davies, and my computing staff for the extensive computations that underlie the published tables.

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Discussion on Dr Finney's Paper

Mr T. LEWIS (in proposing the vote of thanks): The importance of the topic of Dr Finney's paper needs no underlining. Thinking only of academic education—the context in which Dr Finney places his argument—we have, for example, the Ministry of Education's recent estimate (Ministry of Education, 1962) that by the end of the next decade there will be three times as many children staying on at school until 17 or 18 as there are at present. But the implications of the paper are much wider—including the best choice of people for the various professions, for work at many different levels in industry, for Parliament, and so on.

Dr Finney refers to his paper as a study in methodology, and says in Section 9 that his statistical analysis is not concerned with the need for improvements in methods of education or with criteria for measuring improvement, and that the problem of defining scores is not under discussion. I suggest, however, that these vital topics are not outside our area of discussion today. A year ago Dr P. V. Sukhatme read to this Society a paper (Sukhatme, 1961) on world hunger and problems of future food needs. I should like to refer to Dr Finney's written contribution (Finney, 1961) to the discussion which followed, and to pay tribute, on behalf of myself and others, to his wise intervention on that occasion. "On such an issue", he wrote in his final paragraph, "it is easy to obscure objective statistical thought by emotions, but I do not think that the founders of our Society would have shared our apparent reluctance to permit entry of human values into our discussions. . . . [We should] refuse to regard [Dr Sukhatme's] paper as merely an exercise in statistical methodology, a character properly possessed by most of the papers we receive." These remarks form, *mutatis mutandis*, a text appropriate to Dr Finney's own paper, which is in its implications—for all that he describes it as a study in methodology—comparable with Sukhatme's in the social importance of the issues it raises.

I certainly think the non-statistical issues are the vital ones for discussion here, and they are in any case implied by Dr Finney's mathematics. Whatever the approximations involved in the calculations, these show unmistakably that, for efficient allocation, the criteria used for prediction must be very highly correlated with the characteristic for which one is selecting. The 70 per cent. and 75 per cent. correlations of the numerical examples, high as they appear, are not good enough. In other words, much of the effort spent on

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examining may be misdirected. This is perhaps a platitude, but one whose implications we are not on the whole very keen to face. The question *What are examinations examining?* urgently needs to be examined.

What, for example, is the relation between the examination results of medical students and their subsequent effectiveness as doctors? I am told that, perhaps not surprisingly, there has been no follow-up study of this question.

It follows from all this that, over and above any question of optimal choice of P_1, P_2 and P_2^*, and so on, important as this all is, there is an overriding problem of defining predictive criteria which reflect really closely the characteristics desired for the selected group. The search for such a criterion may, even if successful, be long and difficult, and the touchstone, when found, may turn out to be surprising, perhaps even a little disquieting, for it reveals the true nature of one's selection values. I recall a case related by Mr V. D. Barnett. Apprentices taken on by a certain firm were carefully chosen by means of an array of "y-scores"—interviews, tests, previous examination results, and so on, yet their subsequent performance on the job, as evidenced by their superiors' reports, was not always satisfactory. How could the admission procedure be improved so as to select more consistently individuals who would get good x-scores, i.e. good reports from their foremen? After an extensive investigation, in which nearly all the predictive variables turned out to be unrelated to the x-data, one thing only was discovered which gave a good correlation—the apprentice's time-keeping record.

I am sure Dr Finney is right to stress the importance of a study of the operating characteristics of his models, and we shall eagerly await the detailed results on operating characteristics when he gets his computer. In the meantime it might be worth pointing out that the operating characteristic (O.C.) can be calculated quite easily in one at least of the cases Dr Finney has considered, viz. when $\rho_y = \rho_1 \rho_2$ (cf. his equations (7.4) and Tables 2, 4, 6, 8). To do this one works in terms, not of P_1, P_2, P_2^*, but (as suggested by Dr Finney in Section 8) of the equivalent parameters a, b, c, a being the truncation value of y_1/ω_1 at the first selection stage, and b, c those of y_2/ω_2 for $y_1/\omega_1 > a$, $y_1/\omega_1 < a$ respectively at the second stage. Thus, if G, L are the upper univariate and bivariate normal probability integrals,

$$G(a) = P_1, \quad L(a, b, \rho_y) = P_1 P_2, \quad L(-a, c, -\rho_y) = Q_1 P_2^*.$$

The O.C. comes out in terms of G only, as

$$\text{O.C.} = G\left(\frac{b-\rho_2 x/\sigma}{\sqrt{(1-\rho_2^2)}}\right) G\left(\frac{a-\rho_1 x/\sigma}{\sqrt{(1-\rho_1^2)}}\right) + G\left(\frac{c-\rho_2 x/\sigma}{\sqrt{(1-\rho_2^2)}}\right) G\left(\frac{-a+\rho_1 x/\sigma}{\sqrt{(1-\rho_1^2)}}\right). \tag{A}$$

For instance, taking ρ_1, ρ_2, ρ_y as in (7.4), and choosing tabular values (National Bureau of Standards, 1959) of a, b, c so as to reproduce as nearly as possible the set of values

$$P_1 = 0{\cdot}2, \quad P_2 = 0{\cdot}42, \quad P_2^* = 0{\cdot}02, \quad \pi = 0{\cdot}1 \tag{B}$$

discussed as an example by Dr Finney in Section 8, we have:

$$a = 0{\cdot}8, \quad b = 0{\cdot}6, \quad c = 1{\cdot}9;$$

$$P_1 = 0{\cdot}212, \quad P_2 = 0{\cdot}430, \quad P_2^* = 0{\cdot}0194, \quad \pi = 0{\cdot}106;$$

and the operating characteristic of the scheme comes out from equation (A) as follows:

x/σ	$-1{\cdot}2$	$-0{\cdot}4$	$+0{\cdot}4$	$+1{\cdot}2$	$+2{\cdot}0$	$+2{\cdot}8$	$+3{\cdot}6$	$+4{\cdot}4$
Proportion selected	·002	·020	·094	·282	·564	·818	·947	·990

There is 5 per cent. chance of selection when $x/\sigma = 0{\cdot}0$, and 95 per cent. chance when $x/\sigma = 3{\cdot}6$. When $x/\sigma = 3{\cdot}481$, the value which corresponds to the least brilliant of Burt's 12,000 geniuses, the chance is 93·4 per cent., the integrated loss amounting to some 550 geniuses. It is also worth noting that $T(\pi) = 1{\cdot}25$ and that the O.C. ordinate has, as one would expect when one comes to think about it, a low value—30 per cent.—for this value of x/σ.

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While one is about it, one can multiply the O.C. function by $(1/\pi)$ times the standardized normal density function, to get the distribution of x/σ in the selected sub-population $(f+f^*)$ and see how near to normal it is. The result is very satisfactory; the skewness and flatness coefficients γ_1 and γ_2 turn out to be $+0{\cdot}07$, $+0{\cdot}02$ respectively, and one could hardly want anything more normal than that.

Something more can be done to sketch in without much computing effort the outline of the O.C. pattern, since the O.C. again comes out quite simply at the further boundary of the parameter space where

$$\begin{vmatrix} 1 & \rho_v & \rho_1 \\ \rho_v & 1 & \rho_2 \\ \rho_1 & \rho_2 & 1 \end{vmatrix} = 0,$$

i.e. in the degenerate case when x is an exact linear function of y_1 and y_2:

$$x/\sigma = \{(\rho_1 - \rho_v\,\rho_2)\,(y_1/\omega_1) + (\rho_2 - \rho_v\,\rho_1)\,(y_2/\omega_2)\}/(1-\rho_v^2)$$
$$\equiv \xi(y_1/\omega_1, y_2/\omega_2), \quad \text{say.}$$

In this case the O.C. function

$$= G\left(\frac{b-\rho_2\,x/\sigma}{\sqrt{(1-\rho_2^2)}}\right) \quad \text{for} \quad x/\sigma > \xi(a,c),$$

$$= G\left(\frac{b-\rho_2\,x/\sigma}{\sqrt{(1-\rho_2^2)}}\right) - G\left(\frac{-a+\rho_1\,x/\sigma}{\sqrt{(1-\rho_1^2)}}\right) + G\left(\frac{c-\rho_2\,x/\sigma}{\sqrt{(1-\rho_2^2)}}\right) \quad \text{for} \quad \xi(a,c) > x/\sigma > \xi(a,b),$$

and $$= G\left(\frac{c-\rho_2\,x/\sigma}{\sqrt{(1-\rho_2^2)}}\right) \quad \text{for} \quad \xi(a,b) > x/\sigma.$$

For example, suppose (cf. equations (7.2)) that

$$\rho_1 = 0{\cdot}9, \quad \rho_2 = 0{\cdot}941, \quad \rho_v = 0{\cdot}7; \tag{C}$$

taking $a = 0{\cdot}8$, $b = 1{\cdot}1$, $c = 1{\cdot}5$ we get P_1, P_2, P_2^*, $\pi = 0{\cdot}212$, $0{\cdot}422$, $0{\cdot}0198$, $0{\cdot}105$ respectively, reproducing fairly closely the values (B); then there is 5 per cent. chance of selection when $x/\sigma = 1{\cdot}00$ and 95 per cent. chance when $x/\sigma = 1{\cdot}76$. If we interchange the values of ρ_1, ρ_2 in (C), the corresponding values of x/σ are $0{\cdot}87$ and $2{\cdot}02$.

While I do not think that there is much to worry about in regard to the assumptions of approximate normality of scores in the various curtailed sub-populations, granted that the underlying joint distribution of x, y_1, y_2 is trivariate normal, this latter assumption does call for comment. As Dr Finney explains, his criteria, E_π/σ and the proportion of incorrect allocations, do not tell the whole story; it is not the marginal cases that really matter, but the incidence of severe misallocation—the unsuitable individuals who get selected, and the first-rate ones who fail to. In other words, the lower tail of the distribution of selected scores and the upper tail of the complementary distribution are what matter. Although one might expect a location measure such as E_π/σ to be fairly insensitive to the form of the underlying multivariate distribution of scores, the same is unlikely to be true for the tails, and if this underlying distribution were not in fact multinormal then the validity of some of the conclusions in the paper might be affected.

If I am not mistaken, Dr Finney does not anywhere in his paper adduce any evidence in support of his assumption of a jointly normal distribution of x, y_1 and y_2. However, I am sure it is a reasonable one. In this connection I would mention the investigation (Petch, 1961) carried out by Mr J. A. Petch on the G.C.E. A-level results and subsequent degree examination results for 2,228 candidates who sat the Northern Universities Joint Matriculation Board's A-level examination in a particular year, 1956. There was, in effect, a sample of size 2,228 from a conditional bivariate distribution of x and y_2 in $(f+f^*)$. This large sample proved to be well fitted by a normal bivariate model, with a correlation—$\rho(x, y_2)$ in Dr Finney's notation—of about 40 per cent. One may, I think, regard

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this as supporting evidence for the joint normality of y_1, y_2 and x, as it is reasonable to suppose that y_1 has a marginal distribution of the same form as those for y_2 and x. It often happens that in trying to settle one problem you raise others, and in this instance I came to realize, on taking a hand with the data at one stage, that the score y_2 had quite a heterogeneous structure. The 2,200-odd candidates could be split into some 1,300 who were attempting the A-level examination for the first time in the year of observation, and about 900 who were sitting it for the second or third time; the pattern of the distributions was markedly different for the two groups. The "second-shotters" included, without doubt, some who had done badly at their first attempt but improved through experience at their second, and again others who had done well the first time, had sat again without any particular need to do so and had then performed indifferently through lack of interest. This is just one of the many complexities which need to be considered in the extensive further work which will, I am sure, be set going by Dr Finney's paper.

Indeed, if sufficient effort is put into following up the lines of advance and opportunities for basic re-thinking which this important paper opens up, there is a real chance of a significantly better future for the coming generation of schoolchildren and students. So perhaps I may be permitted to speak, hopefully, in their name as well as on behalf of us here today when I propose, as I do now, a vote of very appreciative thanks to Dr Finney for his paper.

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Dr W. D. Wall: I feel rather like an interior decorator in a meeting of expert cooks. I am not competent to criticize the meal so I am going to talk about the decorations. May I point out that any interest shown by anybody in education is welcomed, particularly when it comes from somebody who has something of value to say from the standpoint of his own science. Most pundits work on the assumption that, having been educated themselves, they are therefore fully competent to talk about education, even about that part of education which they themselves have fortunately escaped; indeed, the greater the area of education they have escaped, the more likely they are to pontificate about it.

May I join Mr. Lewis in saying that, in a sense, the non-statistical issues raised by this paper are the vital ones; I welcome the paper because it draws attention in a rather vigorous way to these non-statistical issues and I would hope that more Fellows of the Society will give their attention to the problems raised by this model and perhaps give us professional help with some of the problems about which it has led me to think.

My first point is, that while the model is applicable to any type of two-stage selection, in the reality of the schools, although selection may take place only at two stages openly, it is in fact taking place all the time. This continuous selection affects the individual and any prophecies we make about him, because, in selecting him for a particular kind of environment, and in acting on our prophecy, we deeply influence him by the very environment into which we put him. In fact, we tend to make our prophecy come true. Therefore, the two-stage model proposed enables us to define a criticism of an overt two-stage process of selection—particularly by exposing the bases of our selection processes in education.

As Dr Finney himself points out, a major problem is presented by the factor of the effects of the "treatment" decided upon as a result of selection. Now if you are attempting

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to do anything in school, it is to change people; if you are not changing them, then you are wasting your time. The purpose of education is to maximize certain aspects of the individual and perhaps to create others. What, then, psychologically is this x and what does education do to it? What are the differential effects of different decisions about allocation? This x is likely to have a normal distribution, I would suggest, because it is a bit of a hotch-potch of all sorts of things combined in more or less random but different ways in different people. For example, there is likely to be in it something of an innate intellectual endowment, there is likely to be in it the accumulated effects of education, there will probably be the attitudes which the child has acquired from his teachers, there will be modifications of his value systems; in fact, all manner of things about which we just do not know much.

The problem here then is that, if we assume that the quality or ability with which we are concerned is more or less unitary and can therefore be measured by fairly simple examinations or tests, we may induce it to behave in the way which we expect. We confirm our convictions by acting upon them. This seems to me to be what happens in a good deal of grammar school selection. Pupils are allocated as suitable to a grammar school and, because the school does its job, the child becomes suitable. Within certain limits the borderline of suitability for an academic type of education can be moved up or down in terms of how far a particular grammar school is effective in educating the children it receives.

This vitally affects the second stage, that of university selection. Universities in general behave as though they have no broad educational responsibility and as though the onus were entirely on the student. Present means of selection of students could be improved so that more perhaps would be selected who could survive and even profit from what they have to offer. If, however, universities were to change their policy and to accept the fully educational role that some would have them assume then predictions based on present circumstances would probably go astray. In terms of current preoccupations, the "pool of ability" depends upon how far academic schools and universities can and do change their students by the education they give. The danger of our current methods of selection or allocation—which are essentially based upon the prediction of future performance in a more or less unchanging situation—is that (a) they are self-justifying and therefore to some extent circular and (b) they tend to an increasingly inflexible set of attitudes and behaviours in the receiving institutions.

If, on the other hand, it becomes accepted that the purpose of education is to change and if necessary to change radically, then equally the defeating of at least adverse predictions is a goal. If, for example, a child is mentally subnormal we tend to put him into a school for the mentally subnormal; the purpose of the school is to make him better than he would be if he were left out. If the school is very highly successful, the child will operate perhaps even above the level of subnormality and all the school's efforts are directed to that purpose—one wonders whether sometimes university efforts are not directed to the reverse!

Some of the difficulty over allocation and selection arises because of misunderstanding and deliberately aroused political passion. We have been concerned with the simple single problem of selecting children suitable for a limited supply of a scarce commodity, not with finding all those who might profit from a particular kind of schooling or higher education. Basically, however, the educationist is not concerned with selection. He is concerned with guidance. He is concerned with finding out about a child or adolescent in detail as a step towards maximizing any strengths and remedying any deficiencies—in knowledge or what you will.

If, then, Dr Finney's model, or anything like it, can be pushed this much farther, if it can be constructed to deal, on the one hand, with a probably complex series of measurements which describe an individual as at present and, on the other, with a complex series of criteria towards the attainment of which you aim to lead him, then we should all rejoice. Such a model would make the educationist define his aims in terms of measures of a much wider variety and a more precise kind than at present. It might also direct our attention to the fact that a variety of different combinations of qualities in the individual may in fact

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produce the same result, For example, a not very bright child who is highly motivated may do much better than a much brighter child who is not well motivated. If we look at our problem in this way we concentrate our attention on guidance; and the psychometrist and statistician are challenged, not only to select those whose chances of success are high, but to provide the grammar or any other kind of school with a description of the present status of the pupils they are receiving, with a clear indication of what they should do, and with an estimate of the probability of success in their efforts.

Finally, there are one or two small points. I do not think we should confine ourselves to predictions from one set of scores obtained at one time. We should, in fact, attempt to use dynamic patterns. There is evidence that the rates and directions of growth in children differ and that if we take measures at more than one point in time, we find different patterns for different children who might at any one point seem to have the same problem. Some children may be going up, some down. And the direction of growth is possibly as important in prediction as is status at any given time. Hence the differences within scores for a child from one time to another may ultimately be as important in allocation, treatment or guidance as anything else.

The other point I want to stress, and am coming back to, is that we should beware of the convention of normality in our distributions and what it tends to force us to do. Most of our tests are based on the assumption that what we are measuring is more or less normally distributed and most of our statistical predictions assume the same thing. However, many of the psychological factors we are concerned with may not be really normally distributed. I am therefore wondering how far—and I ask for guidance from my statistical colleagues—our tests should continue to be constructed and used on the assumption of a basically normal distribution of scores.

I should like to close by strongly seconding the vote of thanks to Dr Finney.

The vote of thanks was put to the meeting and carried unanimously.

Dr Yates: The first point I want to make is that there is a real difference between the mean x and the percentage correctly allocated. Dr Finney, it struck me, rather leaned over backwards to say that there was little to choose between the two measures. I feel strongly that statisticians should give a clear lead in indicating which of the many measures that are available is, in their opinion, the right one to use.

It is frequently necessary in statistical analysis to group quantitative data into classes, the dividing lines of which are largely arbitrary. One must beware, however, of subsequently attaching an importance to these subdivisions which they do not really possess. If a child has an x which is very close to the dividing value then it is of little consequence in which of the two classes he is placed. What is important, as Dr Finney has indicated, is that we should get the people who are likely to benefit greatly from university education to the universities and that we should avoid committing to universities those who are definitely unsuitable.

Dr Finney expressed doubt, or repeated the doubts of the university authorities, that the x value should be identified with the examination result. The examination result is certainly an imperfect measure of what we are really looking for. If a person does badly in his final university examination he is handicapped at the start of his career but he can frequently rectify this in the first few years of his work. It is clear that much further research is needed into methods of testing of all kinds, including the use of interviews. The main difficulty here is to find out and assess the performance of an individual in his future life after he has left the university.

My general impression from Dr Finney's tables, Table 1 and Table 4 in particular, is that it does not very much matter what proportions of selection are used at different levels. If this is so then it gives us a much freer hand to vary the proportion selected according to other needs. It is clear that in any reasonable educational policy more educational effort will be devoted to the brighter individuals, and the determination of the optimal proportions

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of selection at each stage will in part be governed by the amount of extra educational effort which is considered appropriate for the brighter individuals.

One point which strikes me as most important in considering the selection of pupils for different types of education is that of directing them into subjects in which they will do well and in which they will be interested. There is the further problem of maintaining their interest. This is particularly acute in adolescence and most of us must have encountered cases where children have desired to change their subject to one which has caught their imagination but for which they seem to be decidedly less well suited.

Dr Wall said that one of the main objects of education was to recognize deficiencies in pupils and set them right. I would respectfully put forward a rather different thesis. Certainly one must recognize the deficiencies but in many cases I think it is reasonable to accept some of these and not attempt to remedy them, concentrating rather on the things in which the child shows ability.

Finally, I would say that the general moral of this paper is that there is urgent need for much greater research into educational methods. In my own particular subject of mathematics, for example, I find that mathematics teaching is very similar now to what it was when I was at school. Many pupils seem to react most unfavourably to it and develop a thorough dislike of the subject. It is by no means apparent that this is in all cases due to lack of ability. Only by the type of research which has become customary in the natural sciences, namely by testing experimentally different methods of teaching, will we begin to find out how to improve teaching methods and what are the most appropriate methods for the really able, for the intermediate and for the weaker pupils. Relative to the amount of money that is spent on education the resources needed for a really vigorous research programme are quite trivial.

I believe that if we can devote serious efforts to this type of work we shall very greatly improve the final products of our educational system and at the same time I personally feel hopeful that we shall make education a more pleasant process than it is for many pupils today.

Mrs J. Floud: I should like to comment on the status and distributional properties of Dr Finney's postulated quantity x. For each individual, x represents his innate suitability for a university education; and x is taken to be normally distributed in the whole population of an age group.

Of course, x is an inaccessible quantity; for all practical purposes it must be equated with performance in university examinations, or at any rate with some comparable though possibly more relevant or perfect criterion, if and when one is devised.

This fact, that x cannot be expressed otherwise than operationally as a prediction about an individual's performance in some test or examination, seems to me to oblige us to scrutinize the status of x in our discussion. Is it indispensable for the formulation and solution of our problem? If it is not directly accessible—if one cannot select directly on x—has it any compensating explanatory value which might deter us from abandoning it on grounds of intellectual economy? If we retain it, as in one of Dr Finney's models, are we making the best guess as to its distributional properties?

The status of x, and our guess as to its distributional properties, depend on the notion we have of educational selection. A serious difficulty with Dr Finney's methodology is his underlying assumption that the inherent qualities of the individual determine his university suitability. This criticism he acknowledges, in Section 10 of his paper, and he introduces the quantity μ to represent the increment of improvement in an individual's suitability for a university education brought about by selective secondary (grammar) schooling. He enumerates the possible ways in which the increment might relate to x; in fact we know a little (from studies of "early leaving" from grammar schools) about the differential distribution of the μ increment by social class—it accrues more readily to the children of parents in non-manual occupations. But this is not the important point here.

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The important point is that the value of x is in a state of continual modification, at least from the moment a child sets foot in the infant school; and I do not see that we can afford to ignore this fact in devising allocation models.

All schools are selective institutions. The allocation at 11+ merely institutionalizes, consolidates and reinforces a continuous long-term process of social-cum-academic selection (for which Dr Finney's simple model of a two-stream comprehensive school will serve only as the crudest illustration). Many factors enter into the process, of course: random accidents of individual life history; the organizational characteristics of schools (e.g. whether or not pupils are "streamed"), and their characteristics as social systems—as well as systematic and cumulative social influences on the individual's life-chances, such as those of geographical region, ethnic origin, religious affiliation and social class.

But the upshot is that it can virtually never be reasonable at any point in the educational process to take x to be normally distributed—and certainly not in an age group of 11-year-olds facing selection for secondary schooling.

This enormously complicates the problem of calculating allocation parameters, for, as Dr Finney points out, these are very sensitive to modifications of x. If selection is continuous, the calculation of allocation parameters at 11+ and 18+ must allow for the cumulative impact upon x of a whole hypothetical series of anterior, more or less informal, selection stages, as well as of the special reinforcing effect of the formal procedure at 11+. And since the mathematical complexities of even a three-stage process are very great, it seems essential to explore, as Dr Finney suggests, the possibilities of computer simulations. It would be a fascinating task to translate existing research findings into population parameters for this purpose.

This, however, is still to retain x; and I find this difficult to justify. Without x, as I see it, the formulation of the problem gains enormously in economy and clarity. We see immediately the tasks confronting us, and the kinds of sociological investigation and statistical exercise which are needed to cope with them. I have in mind close definitions of the multiple purposes of higher education and the related search for criteria and predictors; the forecasting of effective demand for higher education (here again a case for computer simulations); and the calculation of the optimal provision of university places, on various social, educational, political and economic assumptions.

Dr R. N. Curnow: I should like to congratulate Dr Finney on this most interesting and stimulating paper. In many ways the problems considered are not the most important facing educational policymakers today. The main problems are surely those of introducing more flexibility into the schools system and of improving the reputation of the alternatives to the grammar school and the university. This is not to say, of course, that the problems considered in this paper are unimportant. Dr Finney has shown that it is possible to obtain results of great practical interest from a model of educational selection that is not all that unrealistic. Perhaps one of the most important effects of this paper will be a greater interest in formulating aims and in obtaining numerical values for the variances and correlation coefficients of test scores and university performance.

I am rather surprised that Dr Finney has made no mention of the selection of pupils to receive sixth-form education. Selection at this stage is probably not as intense as at 11+ and university admission, but it does make university selection three-stage rather than two-stage.

Dr Finney dismisses the possibility of using 11+ scores to improve the accuracy of the second-stage selection based on university admission scores. On p. 541, the optimal two-stage schemes are shown to be equivalent to one-stage schemes in which the correlation coefficients between university performance and test score are about 0·78 and 0·68 for correlation sets (7.2) and (7.4) respectively. The corresponding correlation coefficients for a scheme in which no selection is applied at 11+, but in which optimal use is made of the 11+ scores at the time of university admission, are the multiple correlation coefficients of x with y_1 and y_2, $R = 0{\cdot}79$ and $R = 0{\cdot}71$. These values are slightly higher than those

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obtained by Dr Finney for his optimal schemes even though they refer to schemes in which there is no selection at 11+. These strange results are due partly to the inadequacy of the model and, perhaps more importantly, to the rather high partial correlation coefficients between the 11+ score and university performance for students having the same university admission score, $\rho = 0{\cdot}37$ and $\rho = 0{\cdot}47$ for correlation sets (7.2) and (7.4) respectively. Both the sets of correlation coefficients considered by Dr Finney do have some rather strange implications and I should like to suggest that other sets may give results differing substantially from those obtained in this paper.

Many people would object, rightly or wrongly, to using 11+ scores at the time of university admission. However, more use could perhaps be made of school examination results in the years immediately before university admission examinations. Ordinary level G.C.E. results are reasonably well standardized and could perhaps be taken into account. Obviously, there are other less statistical advantages in reducing the present complete reliance on Advanced level G.C.E. results. Unfortunately, the Ordinary level G.C.E. examination may not be sufficiently distant in time from university admission to provide as much additional information as does the 11+ examination.

The reasons given for maximizing the proportion of students correctly allocated depend on a rather severe dichotomy of ability. The arguments for maximizing the average ability of selected students seem much stronger.

One further small point. On p. 547 Dr Finney says that the value of $E(x)$ will increase as the number of stages is increased. This is obviously true if optimal schemes are used but the gains from a non-optimal three-stage scheme may well be much less than those from an optimal two-stage scheme.

Dr P. M. Levy: Many educationists will welcome the interest shown by Dr Finney in this sort of problem and will encourage other contributions by statisticians. But I do not wish to comment on the educational aspects of this paper: Dr Finney has, I believe, countered many possible criticisms on these grounds in his statement of aims and in the more qualitative sections of his paper.

The point at which my comments begin has already been referred to briefly by previous speakers. In the first paragraph of Section 10 a criticism is dismissed which I believe indicates a most serious omission in the model presented. Dr Finney says, of using y_1 and y_2 jointly to predict university suitability, that: "the possibility of permitting a child to gain admission to a university because his excellent results at 11+ compensated for only indifferent success in G.C.E. will not appeal to many!"

Surely a correlation does not disappear because we find it distasteful. Even if we do reject the use of score y_1 seven or eight years later we can surely measure the underlying abilities again with advantage.

More generally though, and stemming from this criticism, I regret that Dr Finney's advisers do not appear to have referred him to an important text: Cronbach and Gleser (1957). In their chapter on two-stage sequential selection they compare a number of selection strategies, among which are the following: (a) a non-sequential battery, that is, y_1 and y_2 used jointly to predict x; (b) a fully sequential strategy, that is, on the basis of y_1 the group is divided into three sub-groups—those rejected, those accepted and those awaiting decision until both scores are known; (c) a pre-reject strategy, in which some are rejected on the basis of y_1 and the remainder (P_1) sorted out on the basis of both scores; (d) a pre-accept strategy, which means accept some on the basis of y_1 and then select some of the remainder (Q_1) on the basis of both scores.

Dr Finney's model corresponds to none of these; he has used what psychologists call a "multiple cut-off" method which is most suited to the situation where y_1 does *not* have a linear relationship with the criterion x and generally where there is some hindrance to using y_1 and y_2 jointly. It should also be noted that the 100π per cent. of people selected for university are to some extent different people from one strategy to the next, depending upon the relationships of (7.2) and P_1.

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Secondly, of all these strategies, the non-sequential battery—or simple joint use of y_1 and y_2—is always optimal under Dr Finney's assumptions of linearity of relationships and for his criterion of maximizing the expected mean university suitability score for a fixed π. Using these same assumptions Cronbach and Gleser have to impose an additional restraint, concerning the cost of obtaining y_2, before they can discuss any other strategy as optimal.

Thirdly, and this is the crux of my criticism, if Dr Finney took account of the joint prediction of y_1 and y_2 at the second stage, the optimal value for P_1 is always unity for all values of π. With this strategy, $\pi = 0{\cdot}1$, $0{\cdot}04$ and a multiple correlation of $0{\cdot}789$ obtained from the data in (7.2), the values of $E(x)_\pi$ would be $1{\cdot}384$ and $1{\cdot}700$ respectively. These values may be compared with lines 2 and 6 of Table 14. Certainly the differences are small as far as the university population is concerned, but there is a world of difference educationally between Dr Finney's optimal values of P_1 (Table 14), all of which are less than 13 per cent., and the true optimal value I have demonstrated of 100 per cent.

Thus the existence of other optimal values of P_1 in the paper before us does depend upon the non-optimal strategy of ignoring y_1 at the second stage, and I suggest that the present model is not adequate to decide what is the optimal value of P_1. Only if additional restraints were imposed, such as the extra cost per child of grammar school education, could optimal values be discovered which are other than the statistically trivial but educationally important one of $P_1 = 1$.

Reference Quoted by Dr Levy

Cronbach, L. J. and Gleser, G. C. (1957), *Psychological Tests and Personnel Decisions*. Urbana: University of Illinois Press.

The following written contributions were received after the meeting:

Mr I. D. Hill: In the paragraph in Section 10, already quoted by Dr Levy, Dr Finney seems to be taking it for granted that if 11+ results were to be taken into account at all they should be used with a positive sign. This is true if the criterion x is considered fixed, but if x can be taken as varying it may be that a negative sign should be used. This would mean that, of two candidates for a university who were equally placed by the score y_2, the one with the worse value of y_1 should be accepted, on the argument that he is developing more rapidly than the other.

Heaven help anyone who tried to introduce such a system and explain it to the parents! I am not suggesting that it should be used, since it lacks "educational realism" to an even greater extent than Dr Finney's suggestion, yet it makes sense from a mathematical point of view. I suggest that this means that the mathematical model is inadequate.

Mr F. Sandon: Dr Finney has dealt with some of the aspects of educational allocation and selection but the practical conclusions that he reaches are based largely on the values that he uses for measures such as y_1, y_2, x, ρ_1, ρ_2, ρ_v, P_1, and P_2. As a former headmaster of two grammar schools, one in a county borough and one in a county area, and as a former chief examiner (selection officer) of two local education authorities, one a county borough and one a county, I was interested to read that he found it "surprisingly difficult" to "discover reasonable values" for his measures. When it is remembered that each year teachers and examiners between them assign millions of marks to their pupils and candidates, this observation seems very unexpected. But to the practitioner it is surprising that any sound values whatever can be found. As I have at different times tried to estimate some of these values perhaps it is appropriate that I make a few comments.

The grammar school admission score is y_1. There is a different distribution for each L.E.A. Dr Finney assumes that each will be normally distributed and that a fraction P_1 with the higher value of y_1 is admitted to the grammar schools. But in nearly all cases the E.S.N. (educationally subnormal) children do not take the examination nor, of course, do those children who are weaker still. In fact, until recently, only those pupils whose parents wished it did enter the examination. The scores were therefore in many cases skewed,

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with the brighter children in the longer tail. Further considerations distorting the y_1 distribution and denying the justification for taking P_1 (the fraction with the highest values of y_1) as equivalent to the grammar schools entrants are (1) the fact that the function of y_1 has in the past been to "select" a P_1 of the order of ·15, so that the examiner has attempted to get the best discrimination possible at about his borderline and has not minded lumping all the "also-rans" more or less together at the bottom, (2) the fact that special scores, for pupils at the borderline only, are obtained to modify the original scores and (3) the fact that for some of the candidates with the higher scores the parents did not allow their children to accept the offer of a L.E.A. grammar school place.

The university admission score is y_2. Again, as Furneaux (1961) has pointed out (see also Sandon (1961b) in his review of this), this cannot be regarded as a single score. Dr Finney suggests that there is "the same system of scoring within each sub-population as would be true if it were based on G.C.E. results". This may be doubted. In spite of the efforts of the Ministry of Education and the nine examining bodies this can hardly be so when different candidates take different subjects. In addition, it must be remembered that the various examining bodies have somewhat different "clientèle" (e.g. the Oxford and Cambridge Joint is taken largely by "public" independent schools, the Oxford is taken by "private" independent schools, and the Northern Universities Joint Matriculation Board and the London by "maintained" (L.E.A.) schools). Although the G.C.E. in practice serves largely as y_2, yet again we have other measures that apply to some candidates only and we have again a number of pupils with high scores who do not use their G.C.E. results to seek university admission.

The criterion for university admission is x. Dr Finney admits that this is not capable of convenient measurement and takes university results as his measures. But just as the y_1 examiner has not attempted to make his discrimination at the lower end of the range, so it would be foolish, I suggest, to think that the x measured by degree marks is normally distributed over the whole of the age cohort.

When it comes to determining the values of ρ the difficulties that the enquiring teacher meets are very numerous. He knows that he should correct for homogeneity but he does not know the factors concerned nor, in many cases, can he assume that the fractions are sharply cut-off slices of any normal distribution with linear regressions over all the range. In a city with several grammar schools and with parents' choice of schools, the admissions for any one school come from various places above the admission line; in a rural area the one grammar school has probably so small an entry (of one, two or three forms, giving numbers often not exceeding one hundred from a field of a few hundreds only) that his conclusions would be masked by difficulties resulting from such small samples.

There has been a number of values given in the literature other than those of Nisbet and Buchan quoted by Dr Finney. Some of the better known values for the correlation between y_1 and scores obtained later in the grammar school, including those in the G.C.E., are quoted here. It may be remembered that the term-by-term correlation within any one form can be ·85 (Sandon, 1961a) and for term marks with G.C.E. it can be ·8 (Valentine, 1932), or ·37 to ·69 (International Examinations Inquiry (1936)). Emmett (1942), ·764; McClelland (1942), ·770; Peel and Rutter (1951), ·487 to ·555; Thomson (1943), ·67 to ·88.

There are thus very wide variations in the values given. Thomson (1943) has attempted to correct for homogeneity, to obtain values of ·91 to ·93.

As an illustration of some of the difficulties in correlating the grammar school admission examination conducted in an English city with the later G.C.E. (N.U.J.M.B.) the table on page 560 may be of interest.

It will be seen that practically half the P_1 pupils are not traceable as G.C.E. candidates five years later. Some may have refused the offer of a place in the L.E.A.-maintained school, some may have migrated to other L.E.A. areas, some may have died, some of them may have taken the G.C.E. after four years, some after six, or even seven, years, some may by-pass the G.C.E. in whole or part (the data relate to Ordinary level). Of those who take it after five years, some may take one subject only, some may take up to

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nine. We must bear these features in mind when we read the table, for the non-monotonous features of lines e, f, and g are otherwise inexplicable. How with data of this kind can one obtain a useful correlation between y_1 and y_2?

For the correlation between y_2 and x we have again the tremendous difficulty about the correction for homogeneity. We have also the difficulty of assessing the comparability of university degree performance between universities, faculties and years. And again not all the P_2 fraction will go into a university for reasons similar to those in the earlier case. The values quoted by Furneaux can be compared with those of the International Examinations Enquiry of ·51 to ·85.

Performance of candidates in a grammar school admission examination in the G.C.E. (ordinary level) five years later

a Score in grammar schools' admission examination	†580	600–	650–	700–	750–	800– 850–	900+	Total
b Frequency in this score array	150	370		143		35	7	705
c Frequency of b identified in grammar schools after 5 years	62	130	82	51	45	15 7	4	396
d Number of G.C.E. passes of the c pupils	155	375	298	222	212	60 26	16	1,364
e c/b (as %)	41	57		67		63	57	56
f d/c	2·5	2·9	3·6	4·4	4·7	4·0 3·7	4·0	3·4
g d/b	1·0	1·8		3·0		2·5	2·3	1·9

† Approximate score for P_1 (see text).

We are therefore forced to conclude that there is little sound foundation for any figures for the values of $\rho(xy_1)$, $\rho(xy_2)$ or $\rho(y_1\, y_2)$. It will be noted that most of the values quoted are from rather out-of-date literature. The position is that correlation analysis in this connection has gone out of fashion. This is largely, I suggest, because the various difficulties indicated above have led investigators to the decision that correlations are of little value when it is desired to reach conclusions of any generality. I should, however, like to comment on the values that Dr Finney takes for $\rho(xy_1)$ and $\rho(y_1\, y_2)$, or for ρ_1 and P_v when these are corrected for homogeneity. It is hardly likely that each within a pair can have the same value, that y_1 for the whole cohort is correlated as much with x, a criterion of the suitability of an individual some ten years or more later, as with y_2, a school measure obtained some five or seven years later. I think that ρ_1 is necessarily less than ρ_v, just as ρ_1 is necessarily less than ρ_2.

On an entirely different point, I do not follow in Tables 1–8 how, if $P_1=1$ (so that $Q_1=0$), there can be any value for P^*. Dr Finney in Section 6 agrees that it is indeterminate. But by taking specific values in these tables he obtains some quaint ends to the various rows of figures.

References Quoted by Mr Sandon

Emmett, W. G. (1942), *An Inquiry in the Prediction of Secondary-School Success.* University of London Press.

Furneaux, W. D. (1961), *The Chosen Few.* Oxford University Press.

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Dr FINNEY commented briefly on some of the points raised in the discussion. He subsequently replied in writing as follows: I must begin with a word on Mr Lewis's comment regarding the limited scope of the paper. I restricted my investigation to certain aspects of educational allocation to which I felt able to make a new quantitative approach; an already long paper would have become unbearably tedious if I had tried to discuss other aspects on which I had nothing new to contribute. Of course, I recognize that the definition of scores is very much a question for the statistician, that his advice is also often relevant in the devising of criteria, and that if he is to work in the field of education he needs a good understanding of any proposals for improvement in educational methods.

Mr Lewis has made an important step in the evaluation of operating characteristics, and I look forward to examining this more carefully. I suspect that $\rho_v = \rho_1 \rho_2$ is an unlikely state of affairs, but it perhaps has interest as a lower limit for ρ_v; to have some information on O.C. curves without excessive computation may prove very valuable. It is, of course, just because of the desirability of assessing the frequency of really bad allocations that I have stressed the need to examine O.C. curves. Taken in conjunction with the approximate normality that Mr Lewis finds remaining even after two stages of selection, there is perhaps reason to hope that his approach may lead to a first-order approximation to the O.C. even for a more general correlation structure. However, before undertaking much more computation, I would want to look further at the allowance to be made for the effect of school on subsequent performance and ability; my remarks on u are very crude, but comparison of O.C. curves will not tell us much until the model takes account of this in a not too unrealistic way. Can it be true that no empirical quantitative evidence exists?

I also find interesting Mr Lewis's information on normality of distribution and on the heterogeneity of the population. Without in any way decrying the importance of this heterogeneity, I would guess that in our understanding of the whole problem a more urgent need is to take account of heterogeneity of aims. How shall we amend my attempt to skim the cream and replace it by a search for a system of allocation that recognizes the need to achieve a balanced community in which the different professions and trades are filled optimally? The question is perhaps largely non-statistical, but statisticians ought to be concerned in giving numerical expression to the criterion; this criterion must involve constraints on numbers or proportions in different categories, representation on a single scale of the losses from imperfect allocation to these categories, and recognition of a non-linear situation in that the value that the community loses by a particular imperfect allocation may not be simply related to the sum of the losses by individuals.

Dr Wall made some remarks very pertinent to the nature of x; his insistence that education is concerned with remedying deficiencies as well as with recognizing innate talent is a timely reminder. Without disagreeing with his outlook, I want to emphasize my belief that we must be able to talk about x however much we may modify its definition. My simplification to a univariate model here is doubtless extreme, and perhaps we must develop the theory in relation to a multivariate x that represents different aspects of ability vectorially. Perhaps x for any child must correspond to the highest of which he will be capable under ideal educational conditions, leaving the statistical model to associate with various

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categories not only scores but measures of failure to bring x up to its full value. I do not see how this can be done without many parameters, but that is no excuse for evading the problem. Both educationists and statisticians, however, must insist that far more effort be put into the analysis of all records that may help us to estimate the parameters of the existing educational system: these are the only guide we have to the quantitative effect of changes.

I assure Dr Yates that I did not intend to give the impression that I saw no difference between use of the mean x and of the percentage correctly allocated. I believe that the interests of the community are better served by the former, but individual parents and perhaps some teachers may be more attracted by the percentage criterion. I therefore think it useful to investigate the alternative: if optimal conditions for it are similar to optimal conditions for x, both points of view can be accommodated without sacrifice of the statisticians' preference. Only by study of the operating characteristics can we see the consequences of different systems of weighting values of x to give a single index of the quality of the selection. Dr Yates stressed the importance of having in the universities those who are most likely to benefit greatly, but a form of loss function is essential in order to evaluate on one scale all the possible wrong allocations.

With much of what Mrs Floud said, I agree completely. She has rightly emphasized many weaknesses in my model and indicated other very important factors. I share her wish both to construct from past research reasonable numerical values of population parameters and to extend my type of calculation to more realistic and necessarily more complicated models. However, I cannot understand her wish to remove x from the discussion: with all its modifications, x remains simply the characteristic of an individual at any instant on which ideally selection would be based. I have in Section 3 answered Mrs Floud's objection to taking x to be normally distributed: her worry may arise from confusion of the statistical and colloquial uses of the word *normal*.

I suspect that Dr Curnow's tongue was in his cheek when he commented on selection for sixth forms. I am well aware that my model ought to be at least three-stage, and commend to his attention this formidable computational problem. I am not surprised that closer correlations are obtainable if all selection is deferred until university admission, but rather am pleased that my two-stage scheme approaches so closely to this. Implicit in my argument is the assumption that selection at 11+ is at present an administrative necessity (on whose educational merits and demerits I express no opinion), and I am comforted to find that E_π is not too adversely affected thereby. I think that Dr Curnow is wrong in suggesting that still higher correlation coefficients could be obtained by some selection at 11+; Dr Levy pointed out the optimality of the multiple correlation.

I am very grateful to Dr Levy for his valuable comments, which give me much to think about. I can assure him that I had no wish to imply that a distasteful correlation can be put equal to zero! I had thought it unlikely that, even if appreciable statistical advantages appeared to come from using y_1 as well as y_2 at the second stage of selection, this would be acceptable even for serious discussion at the present time; bearing in mind the oversimplification of my model and the fact that both x and y_2 are likely to be modified for the grammar school pupils, I therefore did not explore this line further. He is of course quite right in stating that "simple joint use of y_1 and y_2 is always optimal", but I was concerned to look for an optimal within the present framework of ignoring y_1 at the second stage. To do as Dr Levy proposes would involve occasionally telling a boy that, despite good performance in an examination at age 18, he could not be admitted to a university because of his poor test record seven years earlier. I do not deny that this *may be* the right policy, but my first reaction on being faced with such a situation would be to suspect a flaw in the model rather than the existence of a small proportion of inherently awkward and unfortunate individuals. Dr Wall's remarks about the need to use dynamic patterns strike me as relevant here. The small numerical difference between using y_2 alone and using y_1+y_2 at the second stage is comforting, and the considerations summarized in what I termed u will reduce it further; I remain convinced that to use y_2 alone is the more

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realistic but am very well aware that the real need is to take account of rates and directions of change as well as instantaneous scores. Mr Hill seems to have a similar idea.

Before this meeting, I had not heard of Cronbach and Gleser's work, and one personally satisfying outcome of my paper is that subsequently I have not only seen their book but have also talked with Dr Cronbach. There is evidently much in common between their approach and mine, and their classification of selection strategies is important. Nevertheless, I think that they have been largely concerned with a different class of problems, those in which two or more stages of selection are to be applied in quick succession. To hold some individuals in a marginal class pending further tests may then be practicable. If several years and an educational programme that modifies abilities come between one stage and the next, some allocation must take place after each stage. For example, the fully sequential strategy might be very appropriate if the 11+ selection itself were broken down into two stages; its application to the system I discussed would mean a three-fold classification at 11 into those who were assured of eventual admission to a university whatever happened during the remainder of their school days, those who were thereafter denied all hope of university admission, and the intermediate class of those for whom no decision could be made until 18. I question whether tests yet available have sufficiently high validity for any substantial numbers to be put (at age 11) into the first class. In fact a simple multiple cut-off method can be highly efficient, as is well known to animal breeders who use it under the name of "independent culling" (Young, 1961; Finney, 1962). My equation (5.1) is more complex on account of its second term, and, under the modified conditions that I suggest in Section 10, I would expect it to be little inferior to a complete joint use of y_1 and y_2. I must agree with Dr Levy that the various Cronbach and Gleser strategies need closer examination from my point of view, remembering that a balance must be maintained between realism of model, practicability of strategy, and statistical efficiency. Some of the difficulties would disappear for a system based upon comprehensive secondary schools; although this is scarcely in itself sufficient reason for advocating such a system, small-scale empirical sampling investigations that I have recently begun make it seem increasingly desirable because of the magnitude of individual discrepancies that occur between values of x actually selected and the highest values in the population.

On this subject, any comments from Mr Sandon are sure to be wise and penetrating. He has most helpfully stated some of the disturbances to which allocation is subject in practice. I hope that his mention of additional evidence on correlations may encourage someone to re-examine and bring up to date our knowledge of this important field: the records of various examining bodies must include vast stores of information, but until the advent of modern high-speed computers adequate study of such information was impracticable. Although I appreciate Mr Sandon's fears of the complexity of the correlation structure, I hope he does not imply that he thinks all questions about the correlation of y_1 and y_2 useless. My simple assumptions of normality and linearity of regressions may be seriously inadequate, but I think they may need to be retained as a first approximation while active pursuit of a more realistic but still tractable model continues. Despite all the difficulties that Mr Sandon mentioned, and despite his very fair criticisms of my numerical values, I suggest that progress will come by better understanding of the correlations rather than by rejection of this approach.

In answer to Mr Sandon's point about Tables 1–8 with $P_1=1$, I may add that the indeterminacy of P_2^* results in the numerical consequences of selection being independent of P_2^*. For example, the formulae readily reduce to

$$E_\pi = \sigma\rho_2\,\nu(\pi),$$

and it is this quantity that appears in the final column of Tables 1–4. Mr Sandon found the numerical values "quaint", but they are in fact the limits approached as $P_1\to1$ on any row of the tables. Perhaps the quaintness arises from the form of the approach to the limit; for any fixed P_2^*, $\partial E_\pi/\partial P_1$ appears to tend to an infinite limit as $P_1\to1$.

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YOUNG, S. S. Y. (1961), "A further examination of the relative efficiency of three methods of selection for genetic gains under less-restricted conditions", *Genetical Research*, **2**, 106–121.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

ABRAHAMS, Stanley Bryan, B.Sc.
ASO, Yasuhiro, M. Econ. (Tokyo)
BERNSTEIN, Abram B., M.S., B.S.
BEYNON, Ernest Geoffrey, B.Sc.
BIRCH, Martin William, B.A., Dip. Math. Stats.
BRANTON, James John, B.Sc.
CAMPBELL, Alexander, B.Sc.
CHANDLER, John
DAVEY, Anthony John Paul
ELLENA, Emilio, M.S.
FREEMAN, Peter Ronald, B.A., Dip. Math. Stats.
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A Remark on the Likelihood Principle

By CHARLES STEIN

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THIS note is an amplification of one of the remarks I made in the discussion on Barnard *et al.* (1962). I shall give an example of a problem where the likelihood principle would strongly suggest a type of inference which, judged by its consequences, is clearly wrong. Rather than try to paraphrase Professor Barnard's verbal reply, I think it better to allow him to answer this note as he sees fit.

Let us first look at the simple problem in which we observe X normally distributed with unknown mean θ and known variance σ^2. The density of X is

$$p_\theta(x) = \frac{1}{\sigma\sqrt{(2\pi)}} \exp\left\{-\frac{1}{2\sigma^2}(x-\theta)^2\right\}. \tag{1}$$

Most statisticians would agree that, in one way or another, one is justified in concluding that θ is close to X, say within $k\sigma$ with k about 3. To my mind, the most satisfying form of this inference is the confidence statement

$$p_\theta(X-k_\sigma<\theta<X+k_\sigma) = \frac{1}{\sqrt{(2\pi)}}\int_{-k}^{k} e^{-\frac{1}{2}x^2}\,dx. \tag{2}$$

It seems to me to be misleading to say that a probability statement is a frequency statement, but, as Tukey pointed out in his Wald lectures in 1959 (still unpublished), (2) implies many frequency statements, provided one is willing to accept the qualification "with very high probability". For example if $\theta_1, \ldots, \theta_n$ are chosen in any way (with n large), and $X_1, \ldots, X_n$ are independently normally distributed with means $\theta_1, \ldots, \theta_n$ and variances $\sigma_1^2, \ldots, \sigma_n^2$, then the proportion of the statements

$$X_i-k_{\sigma_i}<\theta_i<X_i+k_\sigma \tag{3}$$

that will be true is very nearly

$$\alpha = \frac{1}{\sqrt{(2\pi)}}\int_{-k}^{k} e^{-\frac{1}{2}x^2}\,dx. \tag{4}$$

The advocate of the likelihood principle would either deny any justification for believing (3) to be true or would make a similar inference whenever the likelihood function,

$$\theta \to p_\theta(X), \tag{5}$$

is the same, to within a multiplicative constant, as the function given by (1). This is to be looked at for fixed X, the value observed.

Now let us look at another problem with a very similar likelihood function. Let θ be an unknown positive real parameter and suppose we observe a random variable X

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with density

$$p_\theta(x) = \begin{cases} ax^{-1}\exp\left\{-\frac{c^2}{2}\left(1-\frac{\theta}{x}\right)^2\right\} & \text{if } 0<x<b\theta, \\ 0 \quad \text{otherwise.} \end{cases} \tag{6}$$

Here a is chosen so as to make

$$\int p_\theta(x)\,dx = 1, \tag{7}$$

and it is clear that a does not depend on θ. Although this density and all the precise statements made below are meaningful for any positive choice of b and c, the phenomenon we want to illustrate is best brought out if we think of c as large (e.g. 5) and b enormous (e.g. $10^{10^{10}}$). Again we can easily obtain confidence sets for θ since this is a scale parameter family:

$$p_\theta(\alpha X<\theta<\beta X) = p_\theta\left(\frac{1}{\beta}<\frac{X}{\theta}<\frac{1}{\alpha}\right) = a\int_{\beta^{-1}}^{\alpha^{-1}} x^{-1}\exp\left\{-\frac{c^2}{2}\left(1-\frac{1}{x}\right)^2\right\}dx. \tag{8}$$

We suppose $1/\alpha \leqslant b$. It is convenient to look at the one-sided confidence sets

$$p_\theta(\theta<\beta X) = a\int_{\beta^{-1}}^{b} x^{-1}\exp\left\{-\frac{c^2}{2}\left(1-\frac{1}{x}\right)^2\right\}dx. \tag{9}$$

We see that even with β fairly small, e.g. $\beta = \frac{1}{2}$, this probability is close to 1 since the integrand is integrable on any compact set but not at ∞. Thus, in the present case we are justified in concluding that θ is substantially smaller than X although we cannot say very precisely how much smaller; that is, in order to make (8) close to 1 or even appreciably different from 0 we must have $\beta \gg \alpha$.

Now let us look at the likelihood principle. As we have mentioned briefly around (5), the equivalence class of functions

$$\theta \to p_\theta(X) \quad (X \text{ fixed}), \tag{10}$$

two such functions being considered equivalent if they differ by a positive constant multiple, possibly depending on X (since we are looking at fixed X), is called the likelihood function. The likelihood principle is the assertion that a statistical inference must depend only on the likelihood function actually observed, not, for example, on the distribution of possible likelihood functions. We observe that in the first problem the logarithm of the likelihood function is (ignoring an additive constant as we always may with log likelihoods)

$$-\frac{1}{2\sigma^2}(\theta-X)^2 \tag{11}$$

and in the second it is

$$\left.\begin{aligned} -\frac{c^2}{2}\left(1-\frac{\theta}{Y}\right)^2 = -\frac{c^2}{2Y^2}(\theta-Y)^2 \quad \text{if} \quad \theta > Y/b \\ 0 \quad \text{otherwise} \end{aligned}\right\} \tag{12}$$

(where we have written Y instead of X in order to avoid confusion in speaking of (11) and (12) together).

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Both of these functions are quadratic in θ, the latter being cut off way out in the tail. Thus, according to the likelihood principle, except for this insignificant cut-off, the inference (3), in some form or other, should apply to the second example (6) as well, provided we replace σ by Y/c. Then (3) becomes

$$Y-k\frac{Y}{c}<\theta<Y+k\frac{Y}{c}. \tag{13}$$

We have seen that this is not true in a confidence sense, since, for example, with $k=3$, $c=10$ and b very large,

$$P_\theta\left(Y-k\frac{Y}{c}<\theta<Y+k\frac{Y}{c}\right)<P_\theta\left(Y-k\frac{Y}{c}<\theta\right) \tag{14}$$

is very small. It seems fairly clear from (14) that no one could consider the inference (13), or anything like it, to be justified, since it will simply be wrong in an overwhelming proportion of the cases in which it may be applied, no matter how the θ's vary from one trial to the other.

It remains to indicate the way this example arose, and also to give what I believe would be the Bayesian reply to it. Suppose we observe $X_1, X_2, \ldots$ independently normally distributed with common unknown mean θ and variance 1. We take N observations, where N is the smallest n for which

$$\left|\sum_1^n X_i\right|>c\sqrt{n}, \tag{15}$$

where c is large. The likelihood function is then

$$\sqrt{\left(\frac{N}{2\pi}\right)}\exp\left\{-\frac{1}{2N}\left(\sum_1^n X_i-N\theta\right)^2\right\}. \tag{16}$$

If $c-|\theta|$ is much greater than 1, then (15) with n replaced by N will with high probability be nearly an equality, so that, if we put

$$Z=\operatorname{sgn}\sum_1^n X_i, \tag{17}$$

the likelihood function is approximately

$$\sqrt{\left(\frac{N}{2\pi}\right)}\exp\left\{\frac{1}{2N}(Zc\sqrt{N}-N\theta)^2\right\}=\sqrt{\left(\frac{N}{2\pi}\right)}\exp\left\{-\frac{c^2}{2}\left(Z-\frac{\sqrt{N}}{c}\theta\right)^2\right\}. \tag{18}$$

If we identify Y with $c/\sqrt{N}$, then for $Z=1$, this is essentially the same as the likelihood function whose logarithm is given in (12) except for the portion cut off the tail in (12). For $Z=-1$ it is similar.

Reverting to the exact formulae, I suspect that the conclusion

$$\frac{\sum_1^N X_i}{N}-\frac{k}{\sqrt{N}}<\theta<\frac{\sum_1^N X_i}{N}+\frac{k}{\sqrt{N}}, \tag{19}$$

which, according to the likelihood principle, is neither more nor less justified than (3), will also turn out to be wrong with high probability, at least for a substantial range

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of θ. One interesting point about this example is that the parameter point θ is the mean of the normally distributed random variables $X_1, X_2, \ldots$, so that it could have exactly the same meaning in applications as θ in the example discussed around (1)–(4). Thus, if there is to be any difference in the prior distributions used by a Bayesian it can hardly be on the basis of the meaning of the parameter. It could be argued that the experiment described around (15) is not a very reasonable one but this cannot affect the analysis of the experiment.

Just as an amusing exercise, let me try to discuss this example from a Bayesian point of view. The Bayesian is, like all of us, a bit lazy. The first problem discussed is a location-parameter problem, the second a scale-parameter problem. But it seems likely that in 1-parameter problems (and possibly others when the information is large) one will approximate the prior distribution adequately for most purposes by using $d\theta$ as the element of probability in the case of a location parameter and $d\theta/\theta$ in the case of a scale parameter. This is, for the Bayesian, purely a matter of laziness, and he may contend that, if it were not for this laziness, he would choose a single prior distribution to work for both cases, if he knew the meaning of the parameter θ. However, we observe that this means that in practice, allowing for laziness, the prior distribution used does depend on accidental features of the decision problem.

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BARNARD, G. A., JENKINS, G. M. and WINSTEN, C. B. (1962), "Likelihood inference and time series", *J. R. statist. Soc.* A, **125**, 321–372.

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Comments on Stein's "A Remark on the Likelihood Principle"

By G. A. Barnard

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Professor Stein's example (Stein, 1962), as is usual with him, brings out the issues involved in the likelihood principle very sharply, and it should help greatly towards their clarification. In our comments we follow his notation, and the numbering of his formulae.

In the penultimate paragraph of Section 4 of their paper, Barnard *et al.* (1962) said, "If S (the sample space) and Ω (the parameter space) are provided with related ordering structures, or group structures, or perhaps other features, it may be reasonable to apply a form of argument which would not apply if these special features were not present." Now with Stein's density (1), S and Ω are the whole real axis, which has group structure under addition, while the density or kernel function $P_\theta(x)$ is a function $f(x-\theta)$ of the difference $x-\theta$ only, and thus remains invariant when the same element of the additive group, a, say, is applied to both x and θ: i.e.

$$f\{(x+a)-(\theta+a)\} = f(x-\theta).$$

On the other hand, with Stein's density (6), S and Ω are both the positive real axis, which has group structure under multiplication, while the kernel function may be taken to be

$$g(x/\theta)\begin{cases} = a\exp\left\{-\frac{c^2}{2}\left(1-\frac{\theta}{x}\right)^2\right\} & \text{if } 0<\left(\frac{x}{\theta}\right)<b, \\ = 0 \quad \text{otherwise} \end{cases} \tag{20}$$

(provided that, in forming the probability measure on S, we integrate g with respect to dx/x, the group-invariant element of measure) and $g(x/\theta)$ is invariant when both x and θ are multiplied by a, i.e. $g(ax/a\theta) = g(x/\theta)$. Thus underlying Stein's example (1) we have the translation group, while underlying (6) we have the multiplication group. It is this difference in group structure which accounts for the contrasted inferences.

We convert the multiplicative group of positive real numbers into the additive group of all real numbers by taking logarithms, which suggests in (6) setting

$$y = \log x, \quad \lambda = \log\theta. \tag{21}$$

The two examples now have the same group structure, while the density or kernel corresponding to (6) is now

$$h(y-\lambda) = \phi(y-\lambda) \tag{22}$$

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where
$$\phi(u)\begin{cases} = a\exp\{-(c^2/2)(1-e^{-u^2})\} & \text{for } u \leqslant \log b, \\ = 0 & \text{for } u > \log b. \end{cases} \tag{23}$$

The graph of $\phi(u)$ is sketched in Fig. 1, with the values $b = 10^{10^{10}}$ and $c = 5$ used to derive roughly the numerical values inserted. The sketch is, of course, far from being to scale, while only relative heights are shown in the vertical direction.

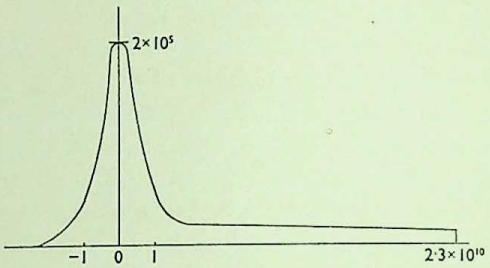

FIG. 1. Graph of $\phi(u)$.

We thus obtain a fair idea of $\phi(u)$ by thinking of it as the density function for a mixture of two populations, one uniformly distributed between 0 and $2{\cdot}3 \times 10^{10}$, the other normally distributed with mean 0 and variance near to 1; the rectangular component outnumbers the normal component in the mixture by about 10^5 to 1.

Given y, the likelihood function of λ is $\phi(y-\lambda)$, and it thus has the shape sketched in Fig. 2. All values of λ from $y-2{\cdot}3\times10^{10}$ to a little less than y itself are almost equally plausible, but any value very near to y is much more plausible than any value decidedly less than y. Values of λ appreciably bigger than y are possible, but very unlikely indeed.

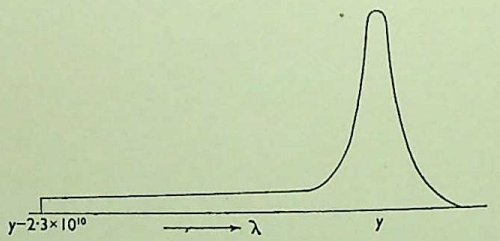

FIG. 2. Likelihood of λ, given y.

Now let us follow the procedure leading from the likelihood (1) to the interval statement (2) (without for the present discussing the appropriateness of (2)). We begin at the maximum-likelihood value y of λ, and take in less and less likely values until we have arrived at the required level of "probability" (say α). That is, we find a value L such that

$$\int_{\phi(u)>L} \phi(u)\,du = \alpha \tag{24}$$

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and we then make the "probability" statement

$$\Pr\{(y-\lambda) \text{ belongs to the set of } u \text{ for which } \phi(u) > L\} = \alpha. \tag{25}$$

In the present case the set of u values runs from just above $-\log_e 2$ up to about $2{\cdot}3\alpha \times 10^{10}$. The "probability" statement for λ is then, very nearly,

$$\Pr\{y - 2{\cdot}3\alpha \times 10^{10} < \lambda < y + \log_e 2\} = \alpha \tag{26}$$

and, in terms of x and θ, this is

$$\Pr\{(10^{10^{10}})^{-\alpha} x < \theta < 2x\} = \alpha. \tag{27}$$

Thus the statement (27) is what corresponds, in example (6), with the statement (2) derived from example (1).

The probability statement (27) appears to contradict Stein's statement that "we are justified in concluding that θ is substantially smaller than x", since the interval in (27) included values which are substantially larger than x.

The difference between Stein's probability statement and ours arises from the fact that instead of taking the condition (24) as his starting point, Stein found δ such that

$$\int_{\delta}^{\infty} \phi(u)\, du = \alpha, \tag{28}$$

which can be done in the present case by taking δ to be very nearly $2{\cdot}3(1-\alpha) \times 10^{10}$. Since we then have

$$\Pr\{y - \lambda > \delta\} = \alpha$$

we find for λ

$$\Pr\{\lambda < y - \delta\} = \alpha \tag{29}$$

and for θ

$$\Pr\{\theta < x/10^{(1-\alpha)10^{10}}\} = \alpha. \tag{30}$$

For $\alpha = 0{\cdot}999$ the proposition inside the braces is

$$\theta < x/10^{10^7}$$

which certainly implies, as Stein says, that θ is considerably less than x.

The fact that we are able to "infer", with high probability, both that θ is much less than x and that θ is in an interval containing x, is characteristic of the simple theory of confidence intervals, as was pointed out long ago by Fisher. Properly interpreted, of course, the two probability statements are not contradictory, but it is worth considering whether one of them can be considered as in any sense more reasonable than the other, or whether some unification is possible. The confidence limits for λ obtained from (24) are shorter than those obtained from (28), since the values of $\phi(u)$ used are higher; it follows that the interval (27) for θ is logarithmically shortest. On the other hand, in the simple sense, (30) is shorter than (27).

The present author is inclined, in the absence of indications to the contrary, to reject less plausible values before rejecting more plausible values, and so he prefers

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(27) to (30); but decision considerations could well upset such a preference. The real truth is that neither (27) nor (30) can be said to give by itself a wholly satisfactory inference from the data. The inference is much better made in terms of the likelihood function sketched in Fig. 2 which, because of the group structure in the problem, may be integrated to give a "confidence distribution", from which the confidence statements implied by (27) and (30), and many others, can be derived. An exactly equivalent "confidence distribution" can be obtained from the likelihood function (20), given x, provided the integration is done with respect to the scale-invariant element $d\theta/\theta$.

It may be objected that in all this we are resting very heavily on the group structure, and this is easily upset by what may seem to be trivial changes. For example, in the problem discussed by Stein in his pages 567 and 568, the sample space of N is discrete, and does not form a group under multiplication. (Strictly, the sample space here is best thought of as 3-dimensional, a point in it being specified by the triplet $(N, \operatorname{sgn}\Sigma X_i, \Sigma X_i - c/\sqrt{N})$; but we ignore these complications.) However, the considerations of Stein's pages 567 and 568 apply only when N is large and θ small, and in this case the problem has a structure only slightly weaker than group structure. Just how much structure is required in a problem to justify inferences like that made above is an open question as yet. One of the difficulties lying in this direction can be seen if we consider the situation arising in Stein's example (1) if we have, in addition, the datum $\theta > 0$; the set Ω in this case ceases to be a group under addition, though it can, of course, be embedded in one. To obtain the "confidence distribution" here one must imagine the likelihood function extended over the whole group, and interpret any confidence intervals as intersections with the actual sample space.

The reader may feel at this point that we are entirely accepting Stein's criticism, and are throwing overboard the whole likelihood principle. That this is not so can perhaps be seen from the fact that nothing we have said conflicts with the thesis that, so long as we are concerned only with the pairwise comparisons between simple hypotheses, Stein's two examples (1) and (6) must be treated exactly alike, when the observations correspond. Thus if with (1) we have $\sigma^2 = 1$ and observe $x = 3$, while in (6) we have $c = 2$ and observe $x = 2$, the log-likelihood is in case (1)

$$-\tfrac{1}{2}(\theta-3)^2 \quad -\infty < \theta < \infty$$

and in case (2)

$$-\tfrac{1}{2}(\theta-2)^2 \quad 0 < \theta < \infty,$$

and we can say that from the first observation, the value $\theta = 3$ is $\sqrt{e} = 1{\cdot}65$ times as likely as the value $\theta = 2$, while from the second, the value $\theta = 2$ is $1{\cdot}65$ times as likely as the value $\theta = 3$. The equality of these two relative likelihoods has a precise meaning, in that, for example, it implies that if both observations were made, independently, and both related to the same quantity, then the combined data would provide us with no grounds for choosing between $\theta = 2$ and $\theta = 3$. (Of course we might have other grounds, apart from the data, for such a choice.)

The likelihood function enables us to make any pairwise comparison we like of one *point* in the parameter space with another. What we cannot in general do is to compare one set of points in the parameter space with another set; we can do this only when special conditions are satisfied. Just as, for example, if we are given a distance function $\delta(x_1, x_2)$ between points in a space X we cannot extend this notion of

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distance to cover distances between arbitrary sets of points (while preserving the standard axioms for distance); only if, for example, the sets are spheres, can this be done (for then we can use the distance between centres of spheres).

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Wheat Yields in England, 1815–59

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The Society's library contains two notebooks, one a copy of the other, containing information on the yield of wheat in England during the first half of the nineteenth century. Their provenance is unknown and they are not catalogued, but they can be identified as containing the record described by Thomas Tooke in his *History of Prices*, Volume V (Tooke and Newmarch, 1857, pp. 53–54, 118–133). This record, based on actual crop-cutting data, was started by the Liverpool firm of Cropper Benson & Co. in 1809, and when this firm discontinued its interest in the wheat trade in 1836 was continued by Joseph Sandars, a Liverpool business man. The last entries in the books refer to 1859; Tooke died in 1858 and Sandars in 1860. One book, which carries a Liverpool label, may have been Sandars's original, while the other, whose paper is watermarked 1853, may have been a copy made for Tooke when he was preparing his history. This latter copy contains copies of memoranda in which the remarkably modern technique of yield estimation is described. There is unfortunately no record of the writer of these memoranda, but he may have been J. Danson, a Fellow of the Society, a letter from whom accompanies them.

The first memorandum reads as follows:

Monday 6th June, 1854.

Mr. Sandars, Corn Surveys

Today I dined at Mr. Tooke's and by appointment met Mr. Sandars of Liverpool, a gentleman largely engaged in the Corn Trade there and who has made in that trade a large fortune. Mr. Sandars gave evidence before the Committee (Commons) on Agriculture of 1836—and has taken a somewhat leading part in most discussions on the Corn Laws during the last twenty years.

It was part of Mr. S's plan of operations in his business to institute Surveys of the Crops in England, a few weeks before each harvest, so as to enable him to judge of the probable result of the harvest.

One of the purposes of the meeting of today was to ascertain from Mr. Sandars the precise mode in which these surveys were conducted.

He said that the plan was to send out from his Counting house at Liverpool at the proper time three or four or more persons of intelligence, each of whom took a prescribed circuit and was provided with introductions to a certain number of farmers on that circuit. The inspector was directed to go into a wheat field on each farm, and to use a small mechanical piece of frame work with which he was provided for the purpose of enclosing within the frame a square yard of the growing corn. He was then to count the number of ears within the frame—and stooping down he was to cut off within six inches of the ground the first ten stalks that came to his hand. These stalks he was to tie up and label and forward at once to his employers at Liverpool.

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FIG. 1. Routes taken by the surveyors.
——— Midland circuit
—·—·— Southern circuit

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When the stalks were received there they were carefully examined, the number of grains in each ear counted and accurately weighed, so as to give a fair average of the whole, and the results tabulated. The effect therefore of each such inspection was to give as regards the field inspected

(1) The number of ears to the square yard
(2) The average number of grains in each of those ears
(3) The average weight of each grain
(4) And the collected stalks served as a sample of the crops

From these data deductions were worked out as to the yield per acre; and the general results were recorded in a form carefully kept up from year to year.

Mr. Sandars said that in many cases the persons employed knew nothing of the subsequent stages of the enquiry.

He also said that an experience of nearly thirty years had satisfied him that the results obtained from these surveys were very rarely far from the truth.

As will be seen below, the recorded yields are remarkably high, and the second memorandum reports Mr Sandars's comments on this point. The recorded yield in bushels of 60 lb/acre is found by grossing up the measured yield of one square yard. However, the estimate of the acreage under wheat always included paths, headlands, hedges, etc. and as a result it is suggested that the figures should be reduced in the ratio 50 : 72. From a present-day standpoint it would seem more reasonable to leave the yield figure and to adjust the acreage; no adjustment has in fact been made in the results reproduced below.

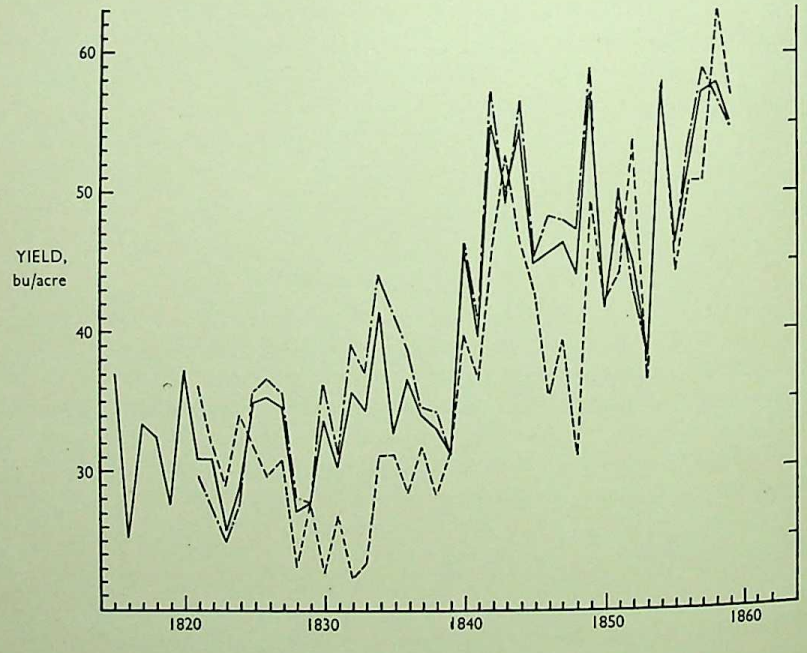

FIG. 2. Average wheat yields, 1815–1859.
— — — — Midland circuit
— · — · — Southern circuit
———— General average

What is completely lacking is any description of the method of choosing the field to be sampled and of locating the sample in the field. From some remarks quoted by

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Tooke, it is plain that the importance of choosing a "typical" site and not picking the best of the crop was fully appreciated, but it seems likely that the surveyors visited the more cooperative farms, and that on these the yield tended to be higher than average. It is also well known today from work in India that small crop samples of this kind are liable to substantial positive bias, mainly due to edge effects. For these reasons, the absolute yields may well be too high by an unknown amount. However, the yearly changes are probably quite accurately determined, and the yield components, weight per grain and number of grains per ear, should be free from any substantial bias.

Yield figures are given in the books from 1821 to 1859. Yield figures for individual centres start in 1830 and yield components in 1837. Figures of average yield are taken back to 1815 by Mr David Hodgson's account of the Surveys in his evidence to the Select Committee on Agriculture in 1821 (see Tooke and Newmarch, 1857, p. 121). The two "circuits" are shown in Fig. 1; average figures for these circuits are given in the table overleaf. The average annual yields in bushels per acre of the two circuits, together with the national average for the years 1815–20, are shown in Fig. 2. It will be seen that there was a substantial increase in yield over the period covered. From the component figures for 1821–59 it appears that this increase reflects increases in both ears per square yard and number of grains per ear, whereas the weight per grain remained relatively constant. For comparison, typical modern figures might be 320 ears per square yard, 25 grains per ear and 1·4 oz. per 1,000 grains, giving a yield of 56 60-lb bushels per acre.

The main historical value in these figures must lie in their objectivity and wide coverage. They overlap usefully with the series of yield figures from Broadbalk field, Rothamsted. It is surprising that the earlier series, which was quoted for the period 1815–55 by Tooke (Tooke and Newmarch, 1857, pp. 127–128), has been not more widely used. Unfortunately, without more and better figures of the acreage of wheat in England than are available, the yield figures cannot provide estimates of the total annual production of wheat. Nevertheless, they show a dramatic rise to a new level from 1840 (see Fig. 2). In this, they confirm in a quite remarkable way Lord Ernle's insistence that the conditions of English agriculture swung rapidly from depression to prosperity after 1837 (Ernle, 1961, pp. 349–376). Ernle's account of the suddenness and scale of this transition may seem to be exaggerated, but the figures of wheat yields are consistently much higher in the 1840s and '50s than in the 1820s and '30s. Weather conditions and other short-run variables undoubtedly account for the marked year-to-year differences in average yields in both periods, but the great discrepancy between the two periods as a whole must as certainly reflect the huge increase in agricultural inputs with the rise in farm product prices after 1837. The most important of these inputs so far as grain yields are concerned, the expansion in the application of artificial fertilizers and of dung from the increased store feeding of cattle on oil-cake and other purchased feeding-stuffs together with the extension of field-drainage, are mentioned by Ernle. Their effects usually persisted beyond the one season and were able to hold wheat yields at a high level during short periods of severe depression, such as 1849–51, when inputs were curtailed.

ACKNOWLEDGEMENT

We are most grateful to Mr J. A. King, the Society's librarian, for showing us the books and for directing us to the references to them in Tooke and Newmarch.

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TABLE 1

Wheat yields, 1815–59

	Midland circuit				*Southern circuit*				
	Ears per sq. yd.	*Grains per ear*	1,000 *corn wt. (oz.)*	*Yield (bu/acre)*	*Ears per sq. yd.*	*Grains per ear*	1,000 *corn wt. (oz.)*	*Yield (bu/acre)*	*General Mean yield (bu/acre)*
1815									37·0
1816									25·3
1817									33·4
1818									32·6
1819									27·7
1820									37·3
1821				36·2				29·6	30·9
1822				32·0				30·6	30·9
1823				29·0				25·0	25·8
1824				34·1				27·7	29·0
1825				31·7				35·7	34·9
1826				29·7				36·7	35·3
1827				30·7				35·7	34·7
1828				23·1				28·1	27·1
1829				27·7				27·7	27·7
1830				22·7				36·3	33·6
1831				26·8				31·2	30·3
1832				22·3				39·1	35·7
1833				23·4				37·1	34·3
1834				31·1				44·1	41·5
1835				31·2				33·2	32·8
1836				28·4				38·8	36·7
1837	175·1	23·3	1·563	31·8	169·8	25·0	1·626	34·6	34·0
1838	166·7	21·8	1·546	28·2	157·7	27·7	1·566	34·3	33·1
1839	174·3	25·3	1·415	31·3	161·9	26·5	1·394	31·2	31·2
1840	178·7	28·8	1·536	39·8	192·8	30·5	1·541	46·4	45·1
1841	176·0	27·0	1·535	36·6	190·1	27·9	1·519	40·5	39·7
1842	177·6	30·8	1·661	45·8	205·7	32·8	1·681	57·0	54·8
1843	207·1	33·1	1·526	52·6	210·5	31·2	1·482	49·3	50·0
1844	186·3	30·4	1·626	46·3	215·3	32·7	1·593	56·6	54·5
1845	191·1	30·9	1·434	42·7	199.8	29·5	1·526	45·4	44·9
1846	158·6	28·5	1·559	35·2	199·0	29·9	1·605	48·3	45·7
1847	167·5	29·1	1·598	39·4	220·6	29·7	1·466	48·1	46·4
1848	163·0	23·9	1·590	31·1	209·7	29·0	1·535	47·4	44·1
1849	199·0	28·9	1·707	49·4	214·0	31·2	1·745	58·9	57·0
1850	192·0	28·6	1·506	41·9	194·8	29·5	1·460	41·8	41·8
1851	196·6	28·1	1·596	44·2	203·1	30·2	1·621	50·1	48·9
1852	215·2	34·1	1·459	53·8	203·7	31·7	1·314	42·8	45·0
1853	162·6	28·7	1·560	36·5	178·4	28·4	1·483	38·2	37·9
1854	191·9	37·3	1·642	58·9	208·9	32·8	1·651	56·9	57·3
1855	187·9	31·4	1·493	44·2	192·2	31·9	1·516	46·8	46·3
1856	193·0	34·0	1·542	50·7	201·6	33·1	1·582	53·2	52·7
1857	190·6	35·1	1·514	50·8	195·5	36·7	1·626	58·9	57·3
1858	232·1	35·2	1·553	63·7	213·8	34·2	1·533	56·5	57·9
1859	200·8	36·9	1·518	56·9	213·5	35·6	1·427	54·6	55·1

Note: The general mean yield is obtained by weighting the Midland and Southern circuit means in the proportions 1 : 4, following a suggestion by Tooke.

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A Supplement to "Note on the History of Sampling Methods in Russia"

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SOME years ago I published a "Note on the History of Sampling Methods in Russia" (this *Journal*, **119**, 1956, pp. 336–338). In that note a reference was made to the *Basic Theory of Sampling Methods* by A. J. Kowalsky, a book published in 1924 in Saratov. I had not had a chance of seeing the book and mention of it was made on the basis of a review published in *Vestnik Statistiki*, **20**, 1925. According to what was said in the review it was possible to consider this book as a treatment of the subject along modern lines and very much in advance of the ideas and practice of sample surveys of that time. Recently, however, I was able to study the book in the Lenin Library in Moscow and the purpose of this Supplement is to give some additional information about it. This information might be of interest, considering the content and the year of publication of the work.

The book was published in the *Scientific Papers* of the State University of Saratov, **2**, Part 4, 1924, pp. 60–138. In the introduction Kowalsky says that in deriving certain formulae he had the help of the mathematician B. K. Riesenkampf, who had formulated an independent development of the mathematical theory of sampling. In Part I the author presents sample surveys as an objective scientific procedure, based on the theory of probability and containing no subjective elements, to which at that time recourse was often made in survey practice. In this respect Kowalsky says that he is adopting the attitude formulated by Tchuprov in the paper presented to the statistical sub-section of the session of the all-Russia congress of research workers in natural sciences and medicine in 1894. The same attitude was expressed in a number of other papers published by Tchuprov and others in subsequent years. Kowalsky proceeds to a review of the many uses of sampling methods in Russian statistics and points out the improvements needed if sample surveys are to be made into an objective and scientific procedure. To this purpose, he proceeds in the subsequent chapters to present the mathematical foundations of the theory of sampling.

In the establishment of the theory Kowalsky does not follow the authors who deal with sampling with replacement or with infinite populations, as such cases are of little practical importance in his area of interests. Instead he presents the theory of sampling without replacement from finite populations and is aware, together with Tchuprov, that sampling with replacement is a special case of this more general approach.

Part II of the book deals with the estimation of the proportions and the number of units having a given attribute. Formulae for unbiased estimates from simple random samples are shown together with their variances. A section follows on the difference between sampling with and sampling without replacement. Next comes the theory of stratified sampling presented in a general way, viz. with formulae which make it possible to use any sampling fraction from individual strata. This leads to the problem

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of the "most convenient proportion of sampling" from various strata which Kowalsky solves by introducing what is now called optimum allocation.

Using the method of the Lagrange undetermined multipliers, Kowalsky minimizes the variance of the estimated mean from a stratified population and obtains (p. 93) the size of the sample in the ith stratum

$$n_i = n\frac{N_i\sqrt{(P_iQ_i)}}{\Sigma N_i\sqrt{(P_iQ_i)}}$$

which is the same formulation now used by most authors in arriving at the optimum allocation.

The same result was published by Tchuprov in 1923 and by Neyman in 1934. Although Kowalsky associates himself proudly with the line of thought adopted by Tchuprov, it is difficult to say whether he benefited from Tchuprov in the establishment of the optimum allocation formula. Kowalsky gave much credit to Tchuprov throughout his book and it is hard to believe that he would have failed to do so with respect to this result if it were suggested by Tchuprov. It is even less likely that Kowalsky saw the paper by Tchuprov mentioned above. Kowalsky lived in a provincial city where the circulation of Western literature in the post-Revolution years was more than restricted. In addition, there are indications that Kowalsky had to wait a long time before he was able to find a publisher for his work.

The next section of Part II deals with the problem of the efficiency of stratified sampling compared to simple random sampling. This question was not clear at that time and Kowalsky solved it elegantly (p. 95) for the case of proportional allocation by showing that the variance of the estimated proportion from such a sample could be expressed approximately as

$$\sigma_p^2 \doteq \frac{P.Q}{n}\left(1-\frac{n}{N}\right)-\frac{\Sigma N_i(P_i-P)^2}{N.n}\left(1-\frac{n}{N}\right).$$

This formula shows that Kowalsky saw clearly the circumstances in which proportional allocation leads to improved efficiency.

Part III deals with the case of the continuous variable and presents the same theory as in Part II. Here there is a reference to Tchuprov as the first to develop (in 1917) the variance of the mean in sampling without replacement, although the result was not published. Kowalsky also says that this formula was developed independently by Riesenkampf. Regarding the gain due to stratification Kowalsky again restricts his discussion to proportional allocation and points out in what way this gain depends upon the differences between strata means.

Another interesting section in this part is the treatment of systematic samples, which were at that time often considered as equivalent to simple random samples. Kowalsky shows that systematic sampling could be conceived of as sampling of one unit from a population divided into n equal strata, n being the size of the sample. Hence a formula of the variance in the case of stratified sampling could be used to derive the precision of systematic samples compared with simple random samples. Kowalsky is aware that such a formula cannot be used in estimating the variance since strata means remain unknown. However, it helped him to introduce the concept of homogeneity as the essential element of the precision of systematic samples. Although in a somewhat different form, this concept is now generally used in presenting the efficiency of systematic samples. Kowalsky breaks down the total variation into

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its components and shows that systematic sampling may in some cases have a high relative efficiency with respect to simple random sampling. He is also aware of the possibility that the opposite may take place.

The last part of the book deals with numerical examples and explains the computation of estimates and their variances.

Although Kowalsky's book does not contain any treatment of multi-stage and multi-phase sampling nor the use of the supplementary information, it is a pleasure for any statistician interested in this field to see that a text published some 30 years before the main books on sampling was in fact written from the modern standpoint on the subject.

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The Chance of being Killed in a Road Accident for People Born During Different Years

By Grace O. Jeffcoate

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Summary

Road accident deaths (excluding motorcyclist deaths) per head of population in Great Britain have been found for each year of age up to age 19 and for each year from 1951 to 1960. The rates have been standardized to allow for any differences there were for the year as a whole, or for the age as a whole during each year, by following the method used in a Home Office Report on "Delinquent Generations". The results show that, on the average, persons born during World War II were more likely than others to be killed in a road accident during the years 1951–60.

A recent report by the Home Office (Wilkins, 1960) compared the number of persons for each year of age between 8 and 20 who were found guilty of indictable offences in the years 1946–56, in England and Wales, with the numbers of persons living at each age. It found that "Children born between 1935 and 1942 have been more delinquent over the whole post-war period than those born in any other seven-year period". Another finding of the investigation was that "youths aged between 17 and 21 in 1955 and onwards, who would have been expected because of their years of birth to be exceptionally delinquent, have in fact been even more delinquent than could have been foretold by the year of birth analysis".

It has been suggested that people involved in road accidents may, in some ways, be psychologically different on the average from those not involved and it is of interest, therefore, to see if persons born during World War II were particularly likely to be killed in road accidents.

In this article the numbers of deaths caused by road accidents in Great Britain have been compared with the numbers of persons living for each year of age, for all persons under 20 years of age, for the years 1951 to 1960. The basic data for deaths are given in Table 1, and these have been divided by the estimated mid-year populations to give the death rates shown in Table 2. Deaths of motorcyclists have been excluded, as it was thought that the large increase in motorcycling and in motorcyclist deaths might overshadow any other factor that might be operating. It is realized that changes in the numbers of people riding pedal cycles or driving cars will also have an effect, but it is thought that this effect would be small.

In Table 2 the figures in the end column are obtained by dividing the deaths of all persons under 20 by the total persons living under 20. In the case of the bottom row, the numerators are the sums of the deaths of all persons of one age; the denominators are the sums of the corresponding population figures for the successive years, 1951–60. Finally, the total deaths of all under 20 years of age in all the years considered have been divided by the total population of those ages during those years to give a general average of 6·98 deaths per 100,000 population. (In later tables those aged 0–1 have

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TABLE 1

Number of persons under 20 *years of age killed in road accidents in Great Britain,* 1951–60†

Year \ Age	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Total age 0–19
1951	1	41	74	126	118	107	79	64	62	47	41	40	33	40	41	42	59	54	53	41	1,163
1952	6	36	90	106	88	107	68	53	45	43	32	27	33	19	33	44	44	40	34	48	996
1953	3	35	75	75	99	108	95	74	52	39	27	34	31	27	23	36	49	43	54	45	1,024
1954	4	39	59	70	64	68	78	67	36	41	37	19	28	29	23	37	50	38	34	48	869
1955	2	39	63	68	85	77	86	64	68	36	34	46	42	33	21	54	57	55	47	75	1,052
1956	3	38	71	77	81	71	60	55	56	49	34	32	26	33	31	36	47	52	56	47	955
1957	6	36	55	48	66	72	55	49	32	50	37	26	34	36	27	39	39	54	55	79	895
1958	14	33	60	59	61	74	64	65	47	45	40	44	38	37	36	41	47	60	69	71	1,005
1959	8	50	68	68	56	56	52	44	35	34	36	46	51	42	33	56	66	73	74	96	1,044
1960	12	36	71	65	58	76	66	56	60	43	35	46	42	49	32	49	81	114	82	92	1,165
Total	59	383	686	762	776	816	703	591	493	427	353	360	358	345	300	434	539	583	558	642	10,168

† Excluding motorcyclists.

TABLE 2

Road-accident deaths per 100,000 *persons living in Great Britain,* 1951–60†

Year \ Age	0–1	1–2	2–3	3–4	4–5	5–6	6–7	7–8	8–9	9–10	10–11	11–12	12–13	13–14	14–15	15–16	16–17	17–18	18–19	19–20	*All ages*
1951	0·1	5·3	9·1	14·4	12·2	14·2	10·6	8·5	8·7	7·2	6·8	6·3	5·1	6·2	6·4	6·7	9·4	8·8	8·8	7·0	8·3
1952	0·8	4·8	11·6	13·1	10·1	11·0	9·1	7·2	6·0	6·0	4·9	4·5	5·2	2·9	5·1	6·9	7·0	6·4	5·8	8·5	7·0
1953	0·4	4·8	10·0	9·7	12·2	12·5	9·8	9·9	7·0	5·2	3·8	5·2	5·1	4·2	3·5	5·5	7·7	6·8	9·1	7·9	7·2
1954	0·5	5·2	8·1	9·4	8·3	8·4	9·0	6·9	4·8	5·5	5·0	2·7	4·3	4·8	3·6	5·7	7·7	5·9	5·6	8·6	6·1
1955	0·3	5·2	8·5	9·3	11·4	10·0	10·7	7·4	7·1	4·8	4·6	6·2	5·9	5·1	3·5	8·5	8·8	8·5	7·5	13·0	7·3
1956	0·4	5·1	9·5	10·4	11·1	9·5	7·8	6·8	6·5	5·1	4·6	4·3	3·5	4·7	4·8	6·0	7·4	8·0	8·8	7·9	6·6
1957	0·8	4·7	6·5	6·4	8·9	9·9	7·4	6·4	4·0	5·8	3·9	3·5	4·6	4·8	3·8	6·0	6·5	8·5	8·5	12·8	6·1
1958	1·7	4·2	7·9	8·1	8·2	10·0	8·9	8·7	6·2	5·6	4·7	4·6	5·1	5·0	4·8	5·8	7·3	10·0	10·9	11·4	6·8
1959	1·0	6·2	8·7	9·0	7·7	7·5	7·1	6·1	4·7	4·5	4·5	5·4	5·3	5·7	4·5	7·5	9·3	11·2	12·3	15·3	6·9
1960	1·4	4·3	8·8	8·3	7·7	10·4	8·9	7·6	8·3	5·8	4·6	5·8	4·9	5·1	4·3	6·7	10·9	16·1	12·6	15·3	7·6
Average	0·8	5·0	8·0	9·9	9·9	10·4	9·0	7·5	6·3	5·5	4·7	4·8	4·9	4·9	4·4	6·5	8·3	9·1	9·0	10·8	7·0

† Excluding motorcyclists.

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been excluded as it was thought that any effect of year of birth could have no effect on deaths from road accidents for those under 1 year of age.) The figures given in Table 2, i.e. deaths from road accidents per 100,000 population, are referred to as "death rates".

Table 2 shows that, apart from the year 1951 in which the general average death rate from road accidents was high, there is no general trend over the years considered. However, for those aged 3–4 to 6–7, which, until the last few years, was the age at which the peak accident rate occurred, there appears to have been a slight reduction in the accident rate in recent years. On the other hand, during these years there has been a great increase in the rates at higher ages, first at 19–20 and more recently at 18–19 and 17–18. Motorcyclist deaths have, as already stated, been excluded, so that these increases cannot be explained by any increase in the amount of motorcycling. Some increase in the number of youths driving cars may be part of the reason, but there has been a large decrease in the amount of pedal cycling, and the increases in the drivers' (other than motorcyclists) deaths appear to have been, to some extent at least, counterbalanced by the decreases in pedal cyclists' deaths of the same age group. There are, however, some differences between the rates for the years and also some differences between the rates for the age groups over the years. To find the effect, if any, of the year of birth it is necessary to eliminate any effects there may be for the general differences between years and between age groups.

The method of standardization used in the Home Office Report (Wilkins, 1960) has been used in this article, and the method of applying it is as follows:

If the average rate for all years (1951–60) for age a is $\bar{a}$, the average rate for all ages for year y is $\bar{y}$, and the average rate for all ages (0–20) for all years (1951–60) is $\bar{z}$, then the expected rate for age a in year y can be taken to be

$$\frac{\bar{a}\bar{y}}{\bar{z}} = E.$$

If the actual rate for age a in year y is O, then the percentage excess is

$$\frac{O-E}{E} \times 100.$$

Values of this excess have been found for each age in each year and are given in Table 3, the figures being arranged so that the rates apply to the years of birth.

The consecutive birth years are represented as overlapping, because, for example, a person aged 10 who was killed in a road accident in 1948 might have been born in either 1937 or 1938.

Table 3 shows that the average death rate from road accidents was higher for persons born during the war years (1939/40–1944/45) than it was for almost all other years. For these war-time birth years there are, however, some cases in which the actual number was less than the expected number, so that it cannot be said that children born during war years have a consistently higher risk. Also the effect apparently extends to the first few post-war years for those aged about 3–6. For the extreme left and extreme right of the table, averages are for accidents in one or two years only and are, therefore, of little value. A t-test shows that the differences between the means of the rates for the three periods, pre-war, during the war and post-war, are highly significant.

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TABLE 3

Percentage by which the road-accident death rate for all persons except motorcyclists was greater or less than the expected rate

Year of accident \ Year of birth	1931/32	1932/33	1933/34	1934/35	1935/36	1936/37	1937/38	1938/39	1939/40	1940/41	1941/42	1942/43	1943/44	1944/45
1951	−45·5	−17·4	−17·9	−3·2	−13·9	22·5	7·0	−12·8	10·2	23·1	10·4	16·6	−4·0	0·3
1952		−23·8	−36·3	−30·4	−16·5	4·4	13·4	−40·8	4·4	−8·8	4·5	8·4	−6·0	−6·0
1953			−28·8	−1·8	−27·6	−9·8	−17·7	−22·5	−15·5	1·6	5·0	−20·8	−8·4	8·1
1954				−8·8	−28·9	−25·0	7·0	0·2	−6·2	13·4	0·7	−36·2	22·2	15·4
1955					14·9	−20·2	−10·8	1·7	24·9	−24·8	0	15·6	22·4	−4·9
1956						−21·8	2·9	−6·3	−4·5	−2·6	14·6	2·0	−24·3	−4·2
1957							35·7	8·1	7·3	−9·9	5·6	−1·3	13·9	8·2
1958								8·6	24·9	13·1	−9·1	−8·4	12·3	6·8
1959									42·0	37·2	24·2	13·9	16·2	2·3
1960										29·1	27·6	61·5	20·6	−6·7
Average	−45·5	−20·6	−27·7	−11·1	−14·4	−8·3	5·4	−8·0	9·7	7·1	9·3	5·1	6·5	1·9
									←(Born during war years)→					

continued

Year of accident \ Year of birth	1945/46	1946/47	1947/48	1948/49	1949/50	1950/51	1951/52	1952/53	1953/54	1954/55	1955/56	1956/57	1957/58	1958/59
1951	15·7	4·0	23·3	−14·1	−9·0									
1952	0	5·1	1·5	30·5	28·2	−5·2								
1953	27·2	6·3	16·3	20·5	−5·0	8·0	−7·2							
1954	−12·4	5·8	15·8	−6·7	−3·4	8·7	3·5	20·6						
1955	−16·5	7·0	−5·6	14·2	−7·7	10·5	−9·9	−9·5	0					
1956	3·9	−1·9	9·6	−3·4	−7·1	−2·6	20·0	11·4	12·7	10·0				
1957	−17·1	−5·2	20·5	−27·6	−2·6	−5·4	9·5	3·8	−25·5	−4·2	9·0			
1958	7·4	−2·1	2·9	4·9	0·3	19·7	1·8	−0·5	−14·1	−15·9	−8·9	−12·8		
1959	16·8	8·8	11·7	−2·8	−18·9	−24·8	−18·7	−20·8	−27·0	−21·7	−8·6	−2·4	24·8	
1960	−11·3	−4·1	−8·9	8·7	−10·0	−4·5	20·1	−8·0	−9·4	−8·5	−29·0	−23·2	−10·5	−19·6
Average	1·4	2·4	8·7	2·4	3·5	0·5	2·4	−0·4	−10·6	−8·1	−9·4	−12·8	7·2	−19·6

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The general result does indicate that, on the average, persons born during the war years (1939/40–1944/45) were more likely than others to be killed in a road accident during the years 1951–60.

ACKNOWLEDGEMENT

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A Note on Small Value Transactions in India's Foreign Trade

By A. S. Prakasa Rao

Department of Commercial Intelligence and Statistics, Calcutta

1. Introduction

The statistics of India's foreign trade, compiled by the Department of Commercial Intelligence and Statistics, Calcutta, are based on the daily trade returns furnished to the Department by the Customs authorities at the various ports. These returns contain among other details the description, quantity and value of the commodity exported, re-exported or imported, as well as the country of destination or consignment. Code numbers of the commodities and countries according to lists laid down by the Department are also shown.

The returns are received in the Department in instalments every day. They are scrutinized in convenient batches in order to ensure the correctness of the codes, the direction of trade and the declared value per unit. The total value of trade in each batch is obtained, for control purposes, by compto-totalling. A card is then punched for each transaction in each batch. The cards are then sorted in commodity–country order, listed and the accuracy of codes and declared value of each transaction re-examined. After this the cards for the entire month are sorted together into a commodity–country order and summarized. A summary card for the trade in a commodity and country shows the quantity and value of that commodity traded in that month as well as similar data for the portion of the year that has elapsed. De-coded tabulations are prepared from these summary cards and sent to press.

The returns for a month run to some 16,000 sheets covering about 100,000 transactions and are received in the Department within 18 days of the end of that month. The processes of compto-totalling, punching, listing and verification usually take about 16 days. Final tabulations for the three types of trade require another 15 days, examination of the de-coded tabulations 6 days and printing 3 days. The publication for a month is thus released 58 days after the end of the month to which it relates.

2. Small Value Transactions

The compilation of India's foreign trade statistics for the last four years has revealed that (i) declared values in respect of low value shipments tend to be too low, and that (ii) the number of detail cards and summary cards for such transactions are more than proportionate to their total value. Hence it appeared desirable to restrict the compilation of detailed commodity–country statistics to large value transactions, and to show the trade by small value transactions in somewhat less detail [as is being done by the United States (U.N., 1951) and many other countries (Allen and Ely, 1953)]. A distribution according to value of individual transactions of India's foreign trade in the year 1960–61 was obtained with a view to choosing a suitable dividing line between large and small value transactions. The results are given in Table 1.

A suitable cut-off point should ensure that (i) the total value of small transactions below that level does not constitute more than some stated percentage of the total value of the trade and that (ii) the number of such transactions should constitute not

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TABLE 1

Distribution according to value of individual transactions in India's foreign trade—1960–61

Value class (Rs.)	*Exports*		*Imports*	
	Percentage distribution of cards	*Percentage distribution of value*	*Percentage distribution of cards*	*Percentage distribution of value*
Less than 50	5·5	0·08	4·9	0·05
Less than 100	9·8	0·12	6·3	0·18
Less than 250	17·3	0·38	18·6	0·44
Less than 500	24·4	0·49	27·3	0·62
Less than 1,000	28·7	0·60	34·9	0·90
Less than 1,500	36·6	1·11	39·1	2·80
Less than 2,000	44·1	4·50	46·2	4·10
2,000 and over	55·9	95·50	53·8	95·90
Total	100·0	100·00	100·0	100·00

less than a stated percentage of the total number. It is also desirable, for operational convenience, to specify a round number for the cut-off point. On these criteria, Rs. 1,000/– appeared to be suitable and was adopted in the compilation of the statistics for the first time in April 1960. The implications of this change are discussed below.

3. SHARE OF SMALL VALUE TRANSACTIONS IN THE TRADE FIGURES AT GROUP AND COUNTRY LEVELS

During the year 1960–61, India's exports were spread over 136 Indian Trade Classification groups of commodities, and imports over 144 groups. The distribution of these groups according to the percentage deficiency entailed by the omission of small value transactions is shown in Table 2.

TABLE 2

Distribution of trade groups according to percentage deficiency—1960–61

Percentage deficiency	*Exports*			*Imports*		
	No. of groups	*Total trade*		*No. of groups*	*Total trade*	
		Crores of rupees	*%*		*Crores of rupees*	*%*
Less than 0·5	40	515	83·0	51	600	56·4
0·5 to 0·9	9	19	3·0	16	230	21·0
1·0 to 1·9	15	60	9·0	16	152	14·0
2·0 to 4·9	20	20	3·0	26	65	6·0
5·0 to 9·9	18	8	1·0	13	18	2·0
10·0 and above	34	9	1·0	22	6	0·6
Total	136	631	100·0	144	1,071	100·0

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As many as 84 groups out of 136 in exports and 109 out of 144 in imports were deficient by less than 5·0 per cent. In exports these groups accounted for Rs. 614 crores out of the total of Rs. 631 crores, and in imports for Rs. 1,047 crores out of Rs. 1,071 crores.

During the year under study, India exported to 167 countries and imported from 142 countries. The distribution of these countries according to percentage deficiencies is set out in Table 3.

TABLE 3

Frequency distribution of countries according to percentage deficiency—1960–61

Percentage deficiency	*Exports*			*Imports*		
	No. of countries	*Total trade with these countries*		*No. of countries*	*Total trade with these countries*	
		Crores of rupees	%		*Crores of rupees*	%
Less than 0·5	82	509	81·0	96	508	50·0
0·5 to 0·9	18	30	5·0	6	95	9·0
1·0 to 1·9	15	16	2·0	3	434	39·0
2·0 to 4·9	14	56	9·0	8	28	2·0
5·0 to 9·9	18	12	2·0	7	5	—
10·0 and above	20	8	1·0	22	1	—
Total	167	631	100·0	142	1,071	100·0

There are 38 countries in the case of exports and 29 in the case of imports whose totals were deficient by 5 per cent. or more when small value transactions were omitted. India's total exports to these 38 countries were, however, only Rs. 20 crores, i.e. about 3 per cent. of the year's total exports. In the case of imports the percentage was less than 1.

4. DECREASE IN WORK LOAD

The exclusion of small value transactions from the detailed compilation of trade statistics has resulted in a considerable saving of time. At present small value transactions are aggregated at the commodity level only, while large value transactions are aggregated at the commodity–country level. As from the issue for April 1960, statistics are published in two volumes: Vol. I relating to exports and re-exports and Vol. II to imports. In order to speed up the publication of Vol. I, priority is given to the compilation of these statistics at almost all stages of compilation. Preliminary scrutiny, punching, listing and checking of listings for Vol. I is completed within 12 days after the receipt of returns. The final tabulation, examination of final tabulation sheets and printing take 7 days, 1 day and 3 days respectively with the result that statistics of exports and re-exports are published with a delay of only 41 days. The listing work for imports is begun only when the final export tabulations are completed. Here too 7 days are devoted to the tabulations. Examination of the final sheets for imports takes 3 days, and the volume is printed and published in another 3 days, i.e. after a gap of 54 days.

If small value transactions are shown only as a single total and not aggregated even at the commodity level, there is a further saving of 5 days in the case of Vol. I

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and of 6 days in respect of Vol. II. The scrutiny of export returns can be completed 2 days earlier, there is a similar gain in the preparation of listings and their checking and final tabulations can be prepared a day earlier. In the case of imports a similar saving can be obtained.

This improvement in speed, and the fact that the percentage deficiencies introduced by the omission of small value transactions in the statistics at various levels are small, justify the omission of small value transactions from the detailed compilation of the statistics. However, the trade in small value transactions, though not published in detail, continues to be recorded fully in order to enable the Department to take special tabulations as and when required.

Finally, it is worth mentioning that the possibility of sampling small value transactions in order to obtain reasonably accurate estimates of trade at the commodity and country levels is under study.

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The Recent Controversy Over Resale Price Maintenance: A Rejoinder

By P. W. S. Andrews and Frank A. Friday

The article by Professor G. Borts in this *Journal* (1961, **124**, 244–249)† puts the argument about resale price maintenance in the context of a conflict between scientific and ethical issues which we cannot accept. He states that this conflict depends not only on the effect of resale price maintenance on consumer prices and the organization of retail trade, but also on "an issue of choice as to whether consumers should be forced to pay for more services than they wish to receive". This seems to us to beg the question, for it assumes that consumers are being "forced" to do something which they do not want to do.

Professor Borts's criticism of the arguments in *Fair Trade* leaves out our main points. Since he deals separately in his review with Part I ("The Controversy"), written by Friday, and Part II ("Competition in Retail Trade"), written by Andrews, we here discuss his criticisms in the same way.

The Controversy

(Rejoinder by Frank A. Friday)

My main points in Part I were not the two attributed to me by Professor Borts. The following summarizes my arguments, with reference to the pages where they will be found in *Fair Trade*:

(a) Resale price maintenance is only one form of price maintenance, being the determination by manufacturers of the selling prices of their branded goods and services when sold in outlets they do not own. There is also direct price maintenance, which is the determination by the central offices of manufacturers or selling organizations of the prices of their branded goods and services in all of the outlets owned by the organizations. Both forms of price maintenance result in uniform prices for identical articles and services in all outlets and it is difficult to see why uniformity should be wrong with r.p.m. but not with d.p.m. Moreover, there are many instances where a branded article is sold by a manufacturer through outlets he owns and outlets which he does not own, so the article is subject to both r.p.m. and d.p.m. (pages 7–13).

(b) The case against r.p.m. is based on the *assumption* that the practice fosters high distribution costs, high gross margins and high prices, and that it prevents a structure of prices which can take account of different costs of retail service. This is based on a misconception of the nature of distribution costs and completely ignores the effectiveness of competition between brands (pages 13–18).

(c) A shop's costs can be apportioned only in a very rough manner to the hundreds of different lines handled by it and the percentage relationship of its costs to turnover depends on the mix of products sold in the shop as well as on the volume of sales of each article. Because of the importance of fixed general costs in retail trade, the prices of individual articles may be determined in an arbitrary manner, upwards as well as downwards, to give higher as well as lower than average margins (pages 18–20).

(d) This kind of arbitrary price determination usually gives rise to selective price cutting and although this may, in principle, be considered unobjectionable,

† *Editor's note:* This was a review article on *Resale Price Maintenance and Shoppers' Choice* by B. S. Yamey (1960) and *Fair Trade: Resale Price Maintenance Re-examined* by P. W. S. Andrews and Frank A. Friday (1960). I regret to record that Mr Frank Friday, who was a distinguished industrial economist and one of the authors of the present paper, died on May 9th, 1962.

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it is often, in practice, objectionable as applied to manufacturers' own brands because it gives rise to price wars in which well-known branded products are used by some distributors merely in order to sell other goods and attract custom away from other outlets. In the process, the distribution system of the manufacturer of the article sold in this way may, for a time, be thoroughly disrupted and his total sales be lowered. Examples of this kind of consequence were given and more could have been quoted (pages 20–29).

(e) It is, of course, easy to prove that the prices of goods sold in this way are lower than the usual prices of those goods, but this proves nothing about the general level of prices. It was shown that Professor Yamey is really quoting as typical, price reductions which are largely of the loss-leader kind, which he claims elsewhere in his writings to be "unimportant in extent". The important question is whether the prices prescribed by manufacturers under r.p.m. are necessarily higher than they would be if determined by the retailers instead (pages 32–35).

(f) Examination of a number of price studies in the U.S.A. points to the conclusion that absence of r.p.m. did not produce a lower level of retail prices. This accords with the theoretical arguments propounded by Andrews in Part II. Unless consumers are to be denied the full range of products, changes in the organization of retailing do not result in a lower percentage of costs to turnover. Moreover, the statistics of gross margins of retailers in Canada before and after the abolition of r.p.m. show that there could not have been the price reductions which the critics claim. It is clear that the reductions quoted by Professor Yamey are quite exceptional and generally reflect selective price cutting (pages 29–32 and 35–39).

Let me deal with a few of Professor Borts's criticisms. He quotes my reference to the loss of sales which can follow from the disruption of a manufacturer's distribution system and he comments: "This argument raises serious doubts in my mind, for it implies that, if r.p.m. has been maintained, sales of the articles in question would not have declined." Of course it does, but there is nothing wrong in the implication because the cause of the decline—a large reduction in the number of selling outlets—would not have occurred with the maintenance of r.p.m. Professor Borts concludes that "It is hard to swallow an argument which maintains that sales of a commodity will decline when its price falls. Certainly Friday would not argue that sales would rise when the price is marked up." In the kind of circumstance I have in mind, I would so argue. The problem cannot be reduced to a simple relationship between quantity demanded and price. I do not, of course, deny the usual inverse relationship between the two, *ceteris paribus*; but a firm's sales of an article depend also, *inter alia*, on the methods of distribution and the number of outlets stocking the product. There is a direct relationship, *ceteris paribus*, between quantity sold and the number of outlets. If a manufacturer has only one outlet, the sales of his branded product are likely to be smaller than if he has 100 outlets; and the sales through 100 outlets are likely to be smaller than the sales through 500 outlets. Just what is the optimum number of outlets may be very difficult to assess, but that the number does affect the volume of sales is known to every sales manager. It is not, therefore, surprising that if, as a result of a price war waged by a few outlets, a manufacturer loses three-quarters of his selling points for the product, his total sales may be reduced, although sales in the few outlets may be enhanced for a short period (see pages 27–29 of *Fair Trade*). A manufacturer is likely to get no benefit from this kind of selective, short-period price reduction and may well be severely injured.

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Professor Borts suggests that r.p.m. broke down in the cases which I cited because the total market declined. This is curious because most of my examples related quite clearly to occasions when r.p.m. was declared illegal. If retailers are overstocked because the quantity placed on the market by all producers is greater than the quantity wanted by consumers at the normal level of prices, then prices must be lowered to clear the surplus stocks and manufacturers have as much interest as distributors in doing this. In these conditions, price maintenance (resale and direct) is abandoned temporarily and prices are cut, with regret and loss, by manufacturers, wholesalers and retailers. But this does not mean that r.p.m. cannot and should not be held in other conditions and it is certainly not the same as the kind of selective price cutting which I was discussing in *Fair Trade*.

It is interesting to note that Professor Borts professes not to understand how the loss-leader argument can be applied to competitive situations in which there is no r.p.m. "In the absence of r.p.m. retail distributors will on occasion drop the price of an item to attract custom, but who is to say what is the fair price, or the customary price, and whether the price-cutter or non-cutter is charging it?" True; but how is the poor consumer to know if Professor Borts does not?

My reference to statistics of retail gross margins in Canada occupied two pages of *Fair Trade*, with one purpose only, to demonstrate that the very large price reductions claimed by critics to follow the abolition of r.p.m. certainly did not appear to have happened in Canada. For example, Professor Yamey quoted in the pamphlet discussed by Professor Borts "an average of almost 22 per cent. below the resale prices suggested (but not enforceable) by their manufacturers" of household electrical appliances, with "even greater" percentage reductions on television and radio. If these price reductions really occurred on the scale implied, they could not fail to show as a large reduction of retail gross margins in the Canadian statistics. Yet they do not. The changes in margins are mostly upward and are consistent with the sort of variation which could be expected had r.p.m. not been prohibited in December 1951. I was not trying to prove anything more.†

COMPETITION IN RETAIL TRADE

(Rejoinder by P. W. S. Andrews)

Here again Professor Borts reduces to two the arguments on the central contention which he summarizes as "that retail trade is competitive as a consequence of free entry, and that therefore r.p.m. cannot eliminate competition and does not raise prices".‡ The first of the two main arguments which Professor Borts ascribes to me is: "(a) If retail margins were higher under r.p.m. than without, we should observe the

† A later book of mine, entitled *Shops and Prices* (Pitman) (pages 29–39), gives a more detailed discussion of the influences on gross margins and examines the Canadian figures for 1950, 1952 and 1956 for products where r.p.m. was important before prohibition at the end of 1951.

‡ It is, incidentally, symptomatic of his whole treatment that he devotes a page to a summary of Professor Yamey's arguments against r.p.m., without comment and therefore apparently with approval, while in attempting to summarize my own and Friday's position he summarizes an argument (and not necessarily correctly) only to produce arguments against it. Thus he attacks Friday's use of statistics without ever pointing out that the statistical basis he is attacking was in fact ground chosen by Professor Yamey, and all Friday is doing is to point out that Professor Yamey's interpretation, even on these statistics, does not bear examination. Friday's independent statistical contribution is an assessment of the importance of r.p.m. and d.p.m., which it is surprising has not been done before and which should surely have been noticed in a review appearing in the *Journal of the Royal Statistical Society*. Actually, Professor Borts appears to argue at times as though *all* retail trade is subject to r.p.m.

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emergence of private brands sponsored by the dynamic elements of retailing." Professor Borts produces three arguments of his own in contradiction. The first is "Mr Andrews does not countenance the possibility that in this country the development of private brands might be stifled by a lack of competition at the manufacturing level". But one of the central points of my analysis is that r.p.m. *enhances* competition at the manufacturing level by enabling more small efficient manufacturers to survive than could if r.p.m. were abolished. Professor Borts makes no reference to my discussion of this effect on competition at the manufacturing level; if he wishes to contradict this he should at least say so clearly and say why. His second point is: "Nor does he [Andrews] mention that the purchase tax represents a serious barrier to the development of private brands. For the elasticity of the demand schedule facing the retailer contemplating the private brand is reduced by the fact that a cut in the retail price of the taxed item is not accompanied by a cut in the tax." I do not understand this. In what way is the private brand in any different position here from the manufacturer's brand? If the elasticity of demand is "reduced" for the one, so it is for the other too. Professor Borts goes on: "Finally, Andrews ignores the consideration that the resort to private brands is an indication that competition has been restrained. For the capital requirements necessary to compete are increased if the retailer must attend to the manufacture of his goods." But we are not talking about the retailer manufacturing. We are talking about brands and who gets the goodwill (or illwill). Does Professor Borts think that underwear sold by Marks and Spencer under their own brand has to be *made* by Marks and Spencer? Or that the Co-operative Wholesale Society has to make its "own" television sets?

Quoting Professor Borts again: "(b) Mr Andrews' second argument is that efficiencies in retailing may be achieved under r.p.m. through the introduction of innovations which cut the costs of distribution. In the absence of retail price competition, this would at first sight seem to be an impossibility; for how are the efficient to attract custom from the inefficient?" Thus Professor Borts puts up his own aunt sally to knock it down. We are talking only of the relatively small proportion of goods which are subject to r.p.m. by some individual manufacturers; yet Professor Borts describes this as "the absence of retail price competition"! Once more Professor Borts completely disregards the competitive impact of manufacturers' brands. Critics of r.p.m. too often write as though manufacturers fixed resale prices so as to be *un*-competitive! Much of the argument against r.p.m. has been in terms of it necessarily stopping innovation in retail trade, and that was why I pointed out in *Fair Trade* that it did not necessarily stop innovation.

In view of these errors concerning what Professor Borts presents as my central contention I do not discuss his other misinterpretations. I am grateful to him for at least trying to discuss the idea of competition in retail trade. I did not take an interest in this subject in the first place because I was "for" r.p.m. It was much more because those "against" were using arguments which obviously needed much closer examination and appeared to be based on an inadequate knowledge of the working of retail trade. In having another look at retail trade in all its existing variety I became convinced that there was much more to be said "for" such r.p.m. as exists than had been put. But r.p.m. is only one aspect of competition in retail trade and I would suggest that the time has come when a little un-narrowing of the eyes of all parties would not come amiss.

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Wholesale Prices in 1961

BY THE EDITOR OF "THE STATIST"

Method of Construction of the Index Numbers

The following table illustrates the method of construction of the index numbers. The index numbers here given are based on the average prices for the eleven years 1867–77. Take, for instance, the *Gazette* price of English wheat:

	s.	d.					
Average, 1867–77 . . .	54	6,	average point.				
,, 1914 . . .	35	0,	64 or 36 per cent.	*below*	the average point.		
,, 1930 . . .	80	7,	148 ,, 48	,,	*above*	,,	,,
,, 1936 . . .	53	3,	98 ,, 2	,,	*below*	,,	,,

The individual index numbers, therefore, represent simple percentages of the average point. The 45 articles are grouped in six categories. They are shown in detail in Table 5.

The general average is drawn from all 45 descriptions, which are treated as of equal value, and is the simple arithmetic mean.

TABLE 1

Construction of index number for 1961

1867–77 = 100

	Number of commodities in index	*Total numbers*		*Index for* 1961
		1867–77	1961	
General average . . .	45	4,500	15,951	354
Food	19	1,900	5,272	277
Vegetable food . .	8	800	2,192	274
Animal food . . .	7	700	2,253	322
Sugar, coffee and tea .	4	400	827	207
Materials	26	2,600	10,679	411
Minerals	7	700	3,667	524
Textiles	8	800	3,466	433
Sundry materials . .	11	1,100	3,546	322

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TABLE 2

*Annual and monthly index numbers**

1867–77 = 100

	Year	*Jan.*	*Feb.*	*Mar.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*
1902–1911 .	74	73·9	74·0	74·2	74·5	74·9	74·5	74·5	74·3	74·3	74·5	74·4	74·8
1912–1921 .	148	145·2	146·3	146·9	147·7	147·9	147·2	147·9	149·2	149·0	149·3	149·1	149·2
1922–1931 .	120	122·7	123·0	122·5	122·0	121·0	119·3	119·0	118·0	118·0	118·6	119·2	118·5
1932–1941 .	97	96·0	96·5	96·5	96·8	97·3	96·9	97·5	97·8	98·7	98·8	99·4	101·4
1942–1951 .	231	221·4	224·1	224·6	227·3	228·0	227·1	226·7	227·3	231·2	235·3	238·5	241·1
1952–1961 .	366	337·3	370·4	369·8	369·9	367·9	366·8	363·4	358·1	358·5	361·1	364·1	367·4
1943 . .	155	153·3	153·2	154·0	154·9	155·6	155·4	156·0	154·4	154·6	153·7	153·9	153·9
1944 . .	160	154·9	155·1	155·7	157·9	159·9	160·0	161·9	161·5	160·1	159·6	160·3	160·7
1945 . .	164	161·4	161·5	162·1	162·4	164·0	166·7	165·2	163·2	162·7	167·2	162·7	163·2
1946 . .	186	165·6	166·2	166·6	168·4	169·3	169·7	174·4	175·7	180·5	196·2	198·1	200·5
1947 . .	230	206·7	209·6	212·1	215·9	216·5	218·1	223·4	225·9	228·9	236·0	240·8	246·5
1948 . .	260	250·5	253·4	256·0	256·4	260·0	263·9	260·4	260·1	258·1	261·5	263·8	266·0
1949 . .	274	267·6	266·8	265·4	272·0	269·2	276·2	263·3	262·5	285·7	291·5	293·7	297·4
1950 . .	324	300·2	301·0	301·7	305·2	308·3	310·3	317·7	332·1	335·7	349·2	363·1	372·3
1951 . .	402	404·8	420·5	419·3	425·7	420·6	405·2	394·5	387·9	395·9	392·0	396·9	399·7
1952 . .	380	403·5	394·8	384·4	377·5	372·2	379·5	374·4	370·4	369·9	368·9	369·4	372·9
1953 . .	366	374·5	375·2	374·9	370·3	368·1	367·5	361·3	357·9	357·0	357·3	357·3	360·0
1954 . .	361	359·8	361·2	366·1	366·5	363·9	361·2	355·5	353·5	353·7	356·8	362·9	371·7
1955 . .	370	378·0	376·5	375·8	370·2	370·1	370·1	373·1	363·9	366·8	372·4	370·9	374·1
1956 . .	384	372·6	379·2	379·8	385·9	383·8	379·7	374·2	373·4	376·2	384·1	393·1	402·9
1957 . .	376	398·0	392·7	389·1	390·7	381·8	377·3	375·1	364·6	364·3	361·4	364·0	364·7
1958 . .	355	360·0	356·1	358·8	361·7	362·9	363·3	357·8	350·3	353·5	356·2	359·6	357·4
1959 . .	356	358·6	354·4	352·8	355·7	353·4	354·2	356·4	354·3	350·3	356·6	357·0	359·3
1960 . .	359	358·3	353·9	353·6	356·0	358·9	358·0	354·2	347·7	350·3	356·7	360·3	360·0
1961 . .	354	359·8	360·1	362·6	364·6	363·7	357·0	352·2	345·5	342·7	340·6	346·8	350·7

* The average of the twelve monthly figures of each year does not necessarily coincide with the annual figures, as the latter are calculated mostly from the average of 52 weekly quotations, while the former are based on end-of-the-month prices.

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TABLE 3

Annual and quarterly index numbers for groups

1867–77 = 100

	All groups	Food				Materials			
		Totals	Vegetable food	Animal food	Sugar, coffee, and tea	Total	Minerals	Textiles	Sundry materials
1902–1911	74	71	66	88	49	77	90	70	74
1912–1921	148	146	136	167	100	154	166	151	148
1922–1931	120	115	101	150	80	122	137	128	110
1932–1941	97	88	84	116	49	103	131	94	91
1942–1951	231	172	201	169	121	273	300	306	232
1952–1961	366	310	313	336	259	407	527	412	328
1943	155	138	156	156	72	167	187	166	156
1944	160	137	152	156	73	178	197	182	161
1945	164	139	155	156	78	182	209	189	159
1946	186	140	155	154	88	219	239	231	198
1947	230	156	191	149	100	284	304	295	263
1948	260	171	217	155	107	324	368	248	279
1949	274	201	240	185	151	328	382	356	273
1950	324	235	268	204	224	390	416	491	299
1951	402	265	305	227	249	502	516	639	394
1952	380	294	336	285	227	443	532	475	363
1953	366	325	365	317	261	399	473	430	329
1954	361	326	330	325	320	386	476	429	298
1955	370	329	313	367	281	400	529	410	310
1956	384	326	319	351	282	428	581	400	352
1957	376	313	295	343	298	422	570	408	338
1958	355	313	314	347	251	386	539	346	319
1959	356	305	303	350	230	393	528	369	324
1960	359	296	277	356	230	405	518	416	325
1961	354	277	274	322	207	411	524	433	322
1960 1st Quarter	355·3	289·3	288·1	322·4	233·9	403·5	521·7	396·0	333·7
2nd ,,	357·6	292·3	291·2	337·7	215·0	405·4	516·7	414·1	328·2
3rd ,,	350·8	290·2	265·8	355·5	224·8	395·2	511·9	404·1	314·0
4th ,,	359·0	291·1	265·5	350·3	238·4	408·6	514·3	445·8	314·2
1961 1st Quarter	360·8	283·9	271·8	337·3	214·8	417·0	509·3	459·9	327·1
2nd ,,	361·8	288·4	279·1	345·3	207·3	415·6	521·8	446·0	326·0
3rd ,,	346·8	266·4	279·7	288·5	201·0	405·4	530·3	412·8	320·6
4th ,,	346·0	270·6	281·0	294·5	207·9	401·1	531·5	407·8	313·3

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TABLE 4

Monthly index numbers for groups

1867–77 = 100

	Year	*Jan.*	*Feb.*	*Mar.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*
1960													
Food:													
Total	296	295·3	286·2	286·4	288·2	291·6	297·1	294·2	287·0	289·3	288·0	292·4	292·8
Vegetable food	277	289·3	287·4	287·5	288·7	289·2	295·7	278·7	258·8	259·9	260·8	268·0	267·7
Animal food	356	335·1	313·9	318·2	326·4	337·3	349·5	358·4	356·9	350·7	342·4	352·6	356·0
Sugar, tea, and coffee	230	237·8	235·1	228·8	220·6	216·2	208·1	212·9	220·7	240·8	247·1	235·9	232·2
Materials:													
Total	405	404·3	403·4	402·8	405·5	408·2	402·4	398·2	392·4	394·8	406·9	409·8	409·0
Minerals	518	524·0	521·3	519·9	521·5	511·7	516·9	519·7	506·4	509·7	513·7	517·3	511·9
Textiles	416	395·3	397·8	395·0	402·9	425·4	414·0	401·0	401·0	410·2	445·7	444·8	447·0
Sundry	325	334·6	332·4	334·1	333·7	329·8	321·2	318·5	312·8	310·6	310·7	316·0	315·8
1961													
Food:													
Total	277	285·8	279·6	286·3	291·6	291·2	282·3	275·1	265·3	258·7	259·1	271·5	281·1
Vegetable food	274	275·4	270·6	269·3	275·7	279·3	282·4	286·8	279·2	273·2	272·6	283·3	287·2
Animal food	322	335·5	328·4	348·0	356·6	351·3	328·0	306·6	286·7	272·3	272·9	295·6	314·9
Sugar, tea, and coffee	207	219·8	212·0	212·6	209·8	209·8	202·2	196·8	200·3	205·8	208·1	205·8	209·7
Materials:													
Total	411	413·7	419·0	418·4	418·0	416·7	412·0	408·3	403·9	404·1	400·0	401·8	401·6
Minerals	524	509·0	508·8	510·2	519·3	520·5	525·6	527·2	533·2	530·4	527·6	534·3	532·5
Textiles	433	458·5	465·4	455·7	452·3	450·1	435·7	420·6	405·1	412·8	409·2	407·9	406·4
Sundry	322	320·6	328·1	332·7	328·5	326·5	323·0	323·7	320·8	317·3	312·2	313·0	314·8

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TABLE 5

Average prices of each of the 45 commodities
1957–61

		Average prices					Index numbers (1867–77 = 100)				
		1957	1958	1959	1960	1961	1957	1958	1959	1960	1961
Vegetable Food											
1 Wheat, English *Gazette*	*s.* and *d.* per qr.	92 6	93 2	90 0	91 5	88 2	170	171	165	168	162
2 Manitoba	,, ,, ,,	122 0	117 8	117 3	117 2	118 9	218	210	209	209	212
3 Flour	*s.* per sack (280 lb.)	102 5/16	98 5/8	96 7/8	97 13/16	101	223	214	211	215	220
4 Barley, English *Gazette*	*s.* and *d.* per qr.	82 8	82 0	80 6	76 0	70 10	212	210	206	195	182
5 Oats ,, ,,	,, ,, ,,	63 8	66 10	62 10	62 8	54 4	245	257	242	241	209
6 Maize, American mixed (*a*)	*s.* per qr.	100 3/4	86 3/4	92	90	86 1/4	310	267	283	277	265
7 Potatoes, good English†	,, ton	397 1/2	635 3/4	574	410	435	340	543	491	350	372
8 Rice, Italian	*s.* and *d.* per cwt.	63 9	64 3	61 9	56 3	57 0	638	643	618	563	570
Animal Food											
9 Beef, prime	*d.* per 8 lb.	204	222	240	237	211	346	376	407	402	360
10 middling	,, ,,	190	211	226	218	186	380	422	452	436	372
11 Mutton, prime	,, ,,	201	205	166	191	180	320	325	264	303	286
12 middling	,, ,,	191	187	153	175	162	347	340	280	318	295
13 Pork, large and small, average	,, ,,	208	200	200	216	199	400	385	385	415	383
14 Bacon, Home Produced, Grade 1	*s.* per cwt.	276 1/2	283 3/4	280 3/4	271	252	374	383	379	366	341
15 Butter§	,, ,,	294	246 1/2	357 1/4	317 1/4	270 1/2	235	197	286	254	216
Sugar, Coffee, and Tea											
16 Sugar, Java floating cargoes (*b*)	,, ,,	41	28 5/8	23 1/2	25 1/16	22 7/8	144	100	82	88	80
17 British and Cont'l crystals	,, ,,	54 3/4	36 1/4	31 3/4	32 3/8	24 7/8	238	158	138	141	108
18 Coffee, Ceylon plantation, low middling (*c*)	,, ,,	439	392	406	340	319 1/2	505	450	467	400	367
Rio, good	,, ,,	313	278	211 1/4	236	220	489	434	330	369	344
19 Tea, average import price	*d.* per lb.	54·21	52·00	52·06	52·97	48·81	314	302	302	307	283
Minerals											
20 Iron, Scottish pig	*s.* and *d.* per ton	464 6	464 6	464 6	469 4	470 0					
Cleveland (Middlesbrough) pig	,, ,, ,,	401 0	401 0	401 0	423 0	426 0	671	671	671	692	695
21 bars, common	£ per ton	45	45	45	43 1/4	43	545	545	545	524	521
22 Copper, Standard	,, ,,	219 5/6	197 11/10	237 17/20	245 3/4	229 13/20	293	264	317	328	306
23 Tin, Straits	,, ,,	754 3/4	734 9/10	785 5/6	796 13/20	888 5/8	719	700	748	759	846
24 Lead, English pig	,, ,,	96 5/8	72 3/4	70 3/4	72 1/8	64 1/8	471	355	345	352	315
25 Coal, Best Yorkshire House*	*s.* per ton	79 3/4	82 7/8	81 7/8	84 5/8	89 3/4	362	377	371	384	408
26 average export price	,, ,,	116·23	105·31	87·06	73·40	72·04	930	843	700	587	576

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TABLE 5—*continued*

Average prices of each of the 45 commodities

1957–61

		Average prices					*Index numbers* (1867–77 = 100)				
		1957	1958	1959	1960	1961	1957	1958	1959	1960	1961
Textiles											
27 Cotton, Middling American	*d.* per lb.	25·35	24·21	21·49	21·66	23·27	282	270	239	241	259
28 East Indian‖	,, ,,	27·55	27·12	28·49	36·93	33·82	408	402	422	547	501
29 Flax, Medium grade continental retted	£ per ton	270⅜	260	270½	287	285	511	463	485	525	502
Belgian import price	,, ,,	210·1	174·9	185·2	206·4	186·9					
30 Hemp, Manila for roping	,, ,,	113	116	156	146	112	465	401	498	485	455
Italian S.B.	,, ,,	250	232¾	232¾	232½	242½					
31 Jute, L. J. A. Firsts	,, ,,	104¼	93	91¾	133¼	163¼	549	490	483	701	859
32 Wool, Merino, Port Phillip, average fleece	*d.* per lb.	100¾	70¾	75¾	71¼	79½	550	391	415	393	431
Merino, Adelaide, average greasy	,, ,,	70 7/12	51⅛	53¾	51	54⅝					
33 English Lincoln half hogs	,, ,,	68½	53½	55 1/6	57 5/6	58½	347	271	279	293	296
34 Silk, Japanese	*s.* per lb.	33 11/12	30⅛	30⅛	33 5/12	37½	148	131	131	145	163
Sundry Materials											
35 Hides, River Plate, dry‡	*d.* per lb.	24	22⅜	31⅛	26⅞	24⅝	272	260	353	311	281
River Plate, salted	,, ,,	15⅞	15¼	21½	16½	15¾					
average import price	,, ,,	22·40	21·79	28·17	27·79	24·01					
36 Leather, dressing hides	,, ,,	49	47	54½	60⅞	58	378	400	414	458	441
average import price	,, ,,	82½	92⅛	89½	98¼	95¼					
37 Tallow, Town	*s.* per cwt.	74¾	75¾	72	59 9/16	63 15/16	166	168	160	132	142
38 Oil, palm	£ per ton	92⅛	83	88⅓	81½	83	236	213	227	209	213
39 olive	,, ,,	345	279	271	255	240	690	558	542	510	480
40 linseed	,, ,,	119⅛	111¾	104⅞	110⅝	119¾	341	340	344	338	351
Seeds, linseed	*s.* per qr.	188⅛	194	205	193	196⅜					
41 Petroleum, motor spirit, c.i.f.	*d.* per imp. gall.	12·96	11·31	11·25	11·19	10·20	82	71	71	70	64
Kerosene (burning oil), c.i.f.	,, ,, ,,	12·82	11·39	11·93	11·95	12·04	203	180	190	190	191
gas oil, c.i.f.	,, ,, ,,	13·03	11·09	10·80	10·13	10·17	327	279	271	254	256
42 Soda, crystals	*s.* per ton	222½	222½	222½	222½	222½	242	242	242	242	242
43 Sodium nitrate	*s.* per cwt.	30	30	29	29	29	214	214	207	207	207
44 Indigo, Bengal, good consuming (*d*)	*s.* per lb.	15	15	15	15	15	207	207	207	207	207
45 Timber, hewn, average import price	*s.* per load	340·62	336·50	327·17	353·21	399·83	768	729	688	791	812
sawn, or split, average import price	,, ,,	534·72	494·33	457·14	549·30	525·58					

(*a*) La Plata from 1924–40; Argentine (Feeding Stuffs) from 1941–52; Yellow American subsequently. (*b*) Raw centrifugals, 96 per cent. Pol. (*c*) East India, good middling from 1908–47; Kenya, average all grades from 1948. * From 1954 the quotations relate to the average pit price of all grades of Yorkshire coal. † Average of weekly quotations during the eight months, January to April and September to December. ‡ Nigerian Butcher Dry from 1956. § Dutch butter to 1954; subsequently average of Australian, New Zealand and Danish. ‖ Sudan Sakel from 1959. (*d*) Quotations purely nominal.

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TABLE 6

Index of silver prices

The base of the index numbers given below is 60·84*d.* per standard ounce = 100, this being a parity of 1 fine oz. of gold to 15½ standard oz. of silver.

	Price per oz. Standard (d.)		*Index number*	
	Average for year	*At end of December*	*Year*	*At end of December*
1942	23½	23½	18·1	18·1
1943	23½	23½	18·1	18·1
1944	23½	23½	18·1	18·1
1945	30½	44	23·1	31·1
1946	48⅝	55½	36·7	41·6
1947	44 7/16	45	33·3	33·8
1948	45	42½	34·1	31·9
1949	49⅕	64	32·8	33·3
1950	64⅘	70	33·7	36·4
1951	77 27/32	77	40·5	40·1
1952	74⅜	72¾	38·7	37·9
1953	73 15/16	73¾	38·5	38·5
1954	73½	74¼	38·4	38·6
1955	77½	78¾	40·0	40·8
1956	79⅛	79½	40·8	40·9
1957	78 9/10	77⅛	40·7	40·0
1958	76 7/32	75⅞	39·4	39·1
1959	78⅞	80¼	40·7	41·5
1960	79⅜	79⅜	40·9	40·9
1961	80¼	88	41·3	45·4

TABLE 7

Gold and Silver: Production

	Gold value of output (£ *thousand*)	*Silver* (*million ounces*)				
		United States	*Mexico*	*Canada*	*Australia*	*Central and South America*
1942	289,153	54·5	84·9	21·8	14·2	31·8
1943	226,397	44·8	71·2	18·6	10·3	29·3
1944	209,031	37·4	63·0	14·8	9·4	29·7
1945	199,116	29·3	61·1	14·0	8·1	27·4
1946	202,435	21·4	48·3	13·6	9·0	26·1
1947	206,434	36·1	49·2	13·5	9·5	23·6
1948	214,647	36·1	45·8	17·0	10·1	23·0
1949	320,887	34·6	49·5	17·6	9·8	24·0
1950	329,580	42·0	49·0	23·2	10·7	28·0
1951	322,623	39·9	43·8	23·1	10·8	28·7
1952	329,381	39·8	50·4	25·2	11·4	32·6
1953	325,500	37·7	47·9	28·3	12·4	35·5
1954	344,865	35·6	39·9	31·1	13·8	33·2
1955	340,732	36·5	48·0	28·0	14·6	35·3
1956	351,600†	38·2	43·1	28·4	14·6	37·3
1957	366,543†	36·3	47·1	28·8	15·7	36·7
1958	377,500†	35·7	47·6	31·2	16·3	39·4
1959	405,000†	23·2	44·1	31·9	15·1	39·5
1960	423,750†	40·1	44·5	33·8	15·3	42·4
1961*	439,250†	41·0	42·0	32·0	12·5	45·0

* Estimate. † Excluding U.S.S.R.

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Regulations of the Research Section†

Organization

1. The Section shall be constituted and carried on subject to the Bye-laws of the Society, and in accordance with these Regulations, made and subject to amendment by the Council of the Society. The Section shall be concerned with the theory of statistics, with the development of statistical methods and with new applications of established methods.

2. The activities of the Section shall be arranged so that the session of the Section shall run from July 1st to June 30th, and the Financial Year from January 1st to December 31st.

Membership

3. Any Fellow of the Society, any member of a Local Group of the Society, or any other person who is approved by the Section Committee, may become a member of the Section, on making written application to the Secretary of the Section Committee.

4. A Fellow of the Society, or any member of a Local Group who is not a non-paying member of another Section, shall not be required to make any payment for membership of the Section. Every other member shall pay an annual subscription of one guinea, which shall be due in advance on each January 1st, provided that any person who becomes a member and pays his initial annual subscription in the last quarter of any year shall not be required to pay any further subscription in respect of the following year. Subscriptions shall be payable direct to the Honorary Treasurer of the Royal Statistical Society, 21 Bentinck Street, London, W.1.

5. If any member of the Section has not paid his annual subscription by June 30th, the Secretary of the Royal Statistical Society shall advise him and the Section Secretary in writing of the default, and if the arrears are not paid by September 30th, the defaulter shall cease to be a member.

6. Payment of an annual subscription in respect of membership of the Section shall not confer on the member the status or full privileges of Fellowship of the Society, or entitle him to receive the Journal of the Society, Series B, free of charge.

7. Members who are not in arrears with their annual subscription may withdraw from membership by giving written notice to the Section Secretary.

The Section Committee

8. Subject to these Regulations, all the affairs of the Section shall be directed by a Section Committee. The Committee shall consist of:

(a) two Fellows appointed by the Council of the Society;
(b) fourteen members elected by members of the Section;
(c) one of the Honorary Secretaries of the Society designated by Council an *ex officio* member of the Committee.

9. Casual vacancies on the Section Committee shall be filled (a) by the Council of the Society if the vacancy is for an appointed member, and (b) by the remaining members of the Section Committee if the vacancy is for an elected member. There shall be a quorum of seven.

† These regulations were approved by Council in May 1962 and supersede those published in this *Journal*, 1946, **109**, 58.

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10. The Committee may co-opt members of the Section as temporary members of the Committee for the remainder of the session.† There shall be not more than two co-opted members serving on the Committee at any one time.

11. Members of the Section shall be invited to submit suggestions for membership of the Committee for the following session to the Section Secretary, suggestions to be received not later than January 31st in the current session. The Section Committee shall then nominate members for election to the Committee for the following session and may designate one of the members Chairman and one Secretary for that session. These nominations shall be circulated to all members at least a month in advance of the date of the last Research Methods meeting of the Society in the current session. Members of the Section may nominate further candidates (being members of the Section) for election, with the consent in writing of such candidates, in which case a postal ballot for members of the Section resident in the United Kingdom shall be taken. Candidates so nominated must be proposed and seconded by members of the Section, and nominations must be received in writing by the Secretary of the Section Committee at least a week before the date of the meeting.

12. At least four of the fourteen members of the Section Committee elected by the Section shall retire annually and shall not be eligible for re-election for the period of one year. In making its nominations for the following session, the Committee shall have regard to seniority on the Committee and degree of attendance. No elected member of the Committee may serve as such for more than three consecutive sessions, with the following exception: a member who has served for three sessions shall be eligible for election as Chairman or Secretary for the following session and such person shall be eligible for re-election as Chairman or Secretary for a further session, although in no circumstances may either the Chairman or the Secretary serve as such for more than two consecutive sessions.

13. The Section Committee shall elect a Chairman and a Secretary from among its members unless these officers have been designated at the time of the election of the Committee. These two officers shall retire at the end of each session but shall be eligible for re-election, provided they are not excluded under the provisions of paragraph 12.

14. The Section Committee shall arrange a programme of Research Methods meetings in accordance with the resolution adopted by the Council of the Society on November 6th, 1958, and may also arrange other meetings and conferences. Particulars of all meetings should be circulated to the Secretaries of the other Sections and of the Local Groups. All meetings shall be open to all Fellows of the Society, members of Local Groups and members of Sections.

15. There shall be an Annual General Meeting of the Section preceding one of the Research Methods meetings during the session.

16. The Section Committee shall send to one of the Honorary Secretaries of the Society, designated by the Council (i) not later than November 30th in each year, a detailed estimate of the total expenditure of the Section in the ensuing year with a request for any funds required to meet such expenditure, and (ii) not later than February 28th, a detailed statement of the expenditure of the Section in the past financial year and a report on the activities of the Section during the current session.

17. The Section Committee shall, subject to direction from the Council, be entrusted with the editorial arrangements for the *Journal* of the Society, Series B.

† For example, an Editor of the *Journal, Series B*, who is not at the time of appointment a member of the Committee, may be co-opted.

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Annual Report of the Council

For the FINANCIAL YEAR *ended December 31st, 1961, and for the* SESSIONAL YEAR *ended June 20th, 1962, presented at the* ONE HUNDRED AND TWENTY-EIGHTH ANNUAL GENERAL MEETING *of the* ROYAL STATISTICAL SOCIETY, *held at the London School of Hygiene and Tropical Medicine, London, W.C.1, on June 20th, 1962.*

Number of Fellows

AT December 31st, 1961, there were 3,251 Fellows, the net gain during the year being 98 (compared with 95 in 1960). New elections and restorations numbered 194. Losses amounted to 20 by death and 76 by resignation or default. Since the end of 1961 (and up to the time of going to press) the number of Fellows has risen to 3,317. At the end of 1961 there were 40 Honorary Fellows including 10 as Presidents for the time being of other Societies.

Number of Fellows (excluding Honorary Fellows)

	Losses			
Calendar year	*By death*	*By resignation or default*	*Elected or restored to the roll*	*On the roll at December 31st*
1951	21	93	228	2,307
1952	15	89	221	2,424
1953	10	91	165	2,488
1954	14	106	171	2,539
1955	19	83	182	2,619
1956	16	85	254	2,772
1957	11	98	226	2,889
1958	12	90	188	2,975
1959	22	78	183	3,058
1960	22	89	206	3,153
1961	20	76	194	3,251

Losses by Death

	Date of Election
Alltoft, Leonard William	1958
*Brunner, Christopher Tatham, C.B.E.	1925
Carpmael, Charlton	1924
Cox, Henry James	1949
*Dick, Godfrey W., J.P., F.C.I.S., C.A. (S.A.)	1906
*cp*Elderton, Sir William Palin, K.B.E., F.I.A., F.F.A.	1919†
Ellefsen, Olaf Leonard Erik	1946
Farmer, Ernest C., F.I.A.	1918
*Fay, C. Ryle, D.Sc.	1913
Fleming, Ralph Alexis Vernon, M.A., A.C.A.	1956
*Gunningham, Sidney Joseph, C.B.E., B.Sc., F.I.A.	1922
McDougall, Frank L.	1925
*Mayer, Leo Kenneth	1939
Moore, William Henry	1929
*Pethick-Lawrence, The Rt. Hon. the Lord, P.C., M.A.	1897
*Redford, Professor Arthur	1925
*Spray, Charles H.	1926
*Swaroop, Satya, M.A., Ph.D., M.P.H., F.N.I.	1935

c Served on Council. *p* Has read a paper, or papers, to the Society. * Life Fellow.
† Also a Fellow 1907–12.

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Mr C. T. Brunner was managing director of Shell-Mex and B.P. and a distinguished pioneer in the study of the economics of road transport and the oil industry. He started the statistical department of Shell-Mex Ltd. in 1927 and thereafter held a series of increasingly important positions in the oil industry. Mr Brunner had been President of the Institute of Transport and at the time of his death was Chairman of the Road Research Board's Committee on Economics of the Department of Scientific and Industrial Research. He was a Frances Wood Memorial Prize winner in 1925.

Sir William Elderton achieved international eminence both as an actuary and as a statistician. He was a Fellow both of the Institute of Actuaries and of the Faculty of Actuaries in Scotland. Among the offices he held, in addition to presidency of the Institute of Actuaries, were the chairmanship of the Life Offices Association, presidency of the Insurance Institute of London and chairmanship of the British Insurance Association. He was made a Chevalier of the Legion of Honour in 1920, was knighted in 1938 and made a K.B.E. in 1946. From 1917 to 1920 he was statistical adviser to the Ministry of Shipping and served again in that capacity from 1939 to 1945. He was the author of a number of statistical books, the most notable of which was *Frequency Curves and Correlation*. He was a member of the Council of the Society for 14 years and was a Vice-President twice.

Dr C. Ryle Fay was Reader Emeritus in Economic History at Cambridge. He was a contemporary and close friend of Keynes. He was particularly interested in colonial development and paid special attention to events in the East and in the West Indies.

Lord Pethick-Lawrence, Secretary of State for India and Burma from 1945 to 1947, made his last appearance at a Society meeting when he attended the 125th anniversary dinner at the Mansion House in 1959. He was a Cambridge Wrangler and Adam Smith's Prizeman for Economics. After leaving the university he travelled and then threw himself with tremendous enthusiasm first into social work in the East End and then with his wife into the woman's suffrage movement, in which with others he suffered the penalty of imprisonment. He was Financial Secretary to the Treasury in 1929 and held strong views about how the 1931 crisis which cost him his seat could have been overcome. He was a sincere friend of India and played an important part in establishing Indian independence.

Mr C. H. Spray was statistical editor to *Lloyd's Register of Shipping* from 1932 until he retired in 1950. He represented Lloyd's Register in the Society from 1944–50.

Dr Swaroop was well known internationally as a medical statistician in the World Health Organization. His *Introduction to Health Statistics* was published only a short while before his untimely death.

Distinctions

The following Fellows were honoured during the year:

Knight Bachelor	R. M. Fraser, C.B.E.
C.B.	G. H. Daniel
C.B.E.	R. E. Beales
O.B.E.	L. F. Cheyney
	H. Kennewell
M.B.E.	A. Morris

The Council takes particular pleasure in the appointment of Sir Robert Shone, a member of Council and former Vice-President, as Director-General of the National Economic Development Council.

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Honorary Fellowship

The Council records with regret the deaths of two Honorary Fellows:

Professor Luigi Einaudi (elected 1955)
Professor J. F. Steffensen (elected 1954)

Obituary notices will appear in the *Journal*.

The following five statisticians were elected Honorary Fellows of the Society at the Ordinary Meeting held on June 21st, 1961:

WILLIAM FELLER. Eugene Higgins Professor of Mathematics, Princeton University; Ph.D. Gottingen; Fellow of the American Academy of Arts and Sciences; Corresponding Fellow of the Yugoslav Academy of Sciences; President of the Institute of Mathematical Statistics, 1947.

ARTHUR LINDER. Professor, University of Geneva, 1945; Professor, l'Ecole Polytechnique Fédérale, Zurich; Joint Director, Statistical Office, Berne, 1928–45; Dr Phil.; Honorary Member, Adolphe Quetelet Society and The Association of Biometricians of Belgium and the Belgian Congo; Fellow, American Statistical Association; Fellow, Royal Statistical Society.

V. S. NEMCHINOV. Member, Praesidium of the Academy of Sciences, U.S.S.R.; President, Economic and Philosophical Department, Academy of Sciences; Formerly Professor of Statistics, Academy of Agriculture, Moscow.

WILHELM WINKLER. Professor Emeritus of Statistics, University of Vienna; Formerly Chief of Section of Population Statistics, Austrian Federal Board of Statistics; Member, Austrian Academy of Sciences; Member, German Academy of Demography; Honorary Member, German, Mexican and Hungarian Statistical Societies; Past President, Austrian Statistical Society.

HERMAN WOLD. Professor of Statistics and Director, Institute of Statistics, University of Uppsala, 1942; Actuarial Consultant; Instructor and Research Associate, Institute of Insurance Mathematics and Mathematical Statistics, University of Stockholm, 1932–38; Lecturer, University of Stockholm, 1938–42; Visiting Professor at Madrid, 1949, 1951, Calcutta, 1949–50, London, 1951, Lisbon, 1953; D.Sc., University of Stockholm, 1938; Member, Swedish Academy of Sciences; Vice-President, I.S.I.; Fellow, Econometric Society; Fellow, Royal Statistical Society; Member, Biometric Society; Fellow, Institute of Mathematical Statistics; Fellow, American Statistical Association.

Guy Medal

During the year the Council awarded the Guy Medal in Bronze to Professor P. Armitage for his paper "The Comparison of Survival Curves" published in the *Journal, Series A*, 1959.

Vice-Presidents

The President appointed as Vice-Presidents of the Society for the session 1961–62, Miss S. V. Cunliffe, Professor H. E. Daniels, Dr D. J. Finney and Mr H. Tetley.

Ordinary General Meetings of the Society

The subjects of the Ordinary General Meetings of the Society during the session have been:

1961

November 17th F. D. K. LIDDELL. Coalminers' morbidity
December 13th I. D. HILL. Sampling inspection and Defence Specification DEF-131

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1962

January 17th — VIOLET CANE. Learning and inference
February 21st — A. R. ILERSIC. The prospects for householders' local rates
March 21st — G. A. BARNARD, G. M. JENKINS and C. B. WINSTEN. Likelihood, inference and time series
April 6th — One-day Conference on Food Supplies and Population Growth:
B. BENJAMIN. Population trends
R. PASSMORE. Estimations of food requirements
N. W. PIRIE. Future sources of food supply—scientific problems
COLIN CLARK. Future sources of food supply—economic problems
May 16th — D. J. FINNEY. The statistical evaluation of educational allocation and selection.

Attendances (excluding the Conference in April) averaged 70.

Research Methods Meetings of the Society

Papers read at Research Methods Meetings, organized by the Research Section, were as follows:

1961

October 4th — A. R. THATCHER. Some results on inventory problems
December 6th — C. R. RAO. Efficient estimates and optimum inference procedures in large samples

1962

February 7th — CHARLES M. STEIN. Confidence sets for the mean of a multivariate normal distribution
April 4th — G. E. P. BOX and G. M. JENKINS. Some statistical aspects of adaptive optimization and control. This meeting was preceded by the Annual General Meeting of the Research Section.

Attendances averaged 76.

The Research Section

Amendments to the regulations governing the Section were approved by Council in May 1962.†

Professor D. R. Cox was elected Chairman of the Research Section for the session, and Mr T. Lewis was re-elected Honorary Secretary. Other members of the Committee were: Mr E. M. L. Beale, Mr G. A. Coutie, Mr H. F. Downton, Professor J. Durbin, Dr F. G. Foster, Dr S. C. Pearce, Dr M. R. Sampford, Dr K. D. Tocher, Professor P. Whittle, Mr C. B. Winsten, nominated by the Section Committee; Professor M. S. Bartlett and Professor D. V. Lindley, appointed by the Council; and, *ex officio*, Mr B. P. Emmett, Honorary Secretary of the Society.

During the session the Section organized the four Research Methods Meetings listed above and, in addition, organized jointly with the Industrial Applications Section a Conference held at Churchill Hall of the University of Bristol over the week-end September 9th–11th, 1961. The organizing sub-Committee consisted of Dr K. D. Tocher (who also represented the Industrial Applications Section), Professor D. R. Cox and Dr S. Vajda. Over 170 Fellows and others participated in the Conference and the following papers were read:

G. A. BARNARD. Sampling inspection
J. KEILSON. Queues
D. R. COX. Renewal theory
A. BIRNBAUM. Some problems in the foundations of statistics
H. E. DANIELS. Spectral analysis
G. W. SEARS. Operational research
K. D. TOCHER. Error-correcting codes and redundant mechanisms.

† See this *Journal* issue, pp. 603–604.

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The General Applications Section

Amendments to the regulations governing the Section were approved by Council in October 1961. The revised Regulations are published in the *Journal, Series A*, 1961 (p. 547).

The Section Committee for the session has consisted of: Mr. W. Rudoe (Chairman), Mr L. A. Dicks-Mireaux (Honorary Secretary), Miss S. V. Cunliffe, Mr A. S. C. Ehrenberg, Mr P. G. Gray, Dr M. H. Quenouille, nominated by the Section; Dr B. Benjamin and Mr A. Stuart, appointed by the Council.

A joint meeting with the Institute of Actuaries Students' Society at which the President, Dr M. G. Kendall, took the Chair, was held on November 14th, 1961. Mr. N. H. Carrier and Miss G. Rowntree presented a paper on "The use of marriage records for social and demographic study".

Other meetings of the Section during the session have been:

1961	
October 10th	T. Corlett. The variability of quota sampling
December 19th	M. J. R. Healy. A statistical computing service
1962	
January 9th	C. A. B. Smith. Human genetics
February 13th	M. H. Quenouille. Designs for paired comparisons
March 13th	J. A. Rowntree. The design of the Post Enumeration Survey for the 1961 Population Census
April 10th	A. Maizels. Measuring the elasticity of demand for exports
May 8th	G. Morton. Generalized transport models. This meeting was preceded by the Annual General Meeting of the Section.

Attendances at these meetings have averaged 40.

The Section organized a Conference held at Trinity College, Cambridge, over the week-end April 6th–8th, 1962. The subject of the Conference was "Statistical aspects of some current social problems", and the following papers were read:

L. T. Wilkins. The measurement of crime
Brinley Thomas. Immigration into Great Britain, statistical aspects and social implications
A. R. Ilersic. Some thoughts on taxation and public expenditure
Jean Floud. The quality and quantity of higher education—some problems of forecasting
F. Garwood. Some problems of increasing traffic
K. G. Knowles. Can we be precise about the causes and effects of strikes?

An opening address was given by Sir John Wolfenden. Some 90 members attended.

The Medical Section

The Committee of the Section during the session has consisted of: Mr M. P. Curwen (Chairman), Dr S. Rosenbaum (Honorary Secretary), Dr N. T. J. Bailey, Dr A. S. Fairbairn, Mr P. G. Gray, Dr J. C. McDonald, Dr C. C. Spicer, nominated by the Section; Professor P. Armitage and Dr J. A. Heady, appointed by the Council.

The following meetings have been held:

1961	
September 26th	R. S. F. Schilling, Joan Walford and A. Colin. Recent epidemiological studies of respiratory disease among textile workers in different countries. This meeting was preceded by a sherry party.
October 31st	Computers in medicine—a discussion summed up by Dr F. Yates
November 28th	P. Froggatt. Accident-proneness, or variable accident tendency

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1962	
January 30th	M. P. CURWEN. Chairman's Address. Hospital statistics in the nineteenth century. This meeting was followed by a wine party
February 27th	J. J. GART. A screening procedure
March 27th	F. B. LEECH. National survey of brucellosis in dairy cows
April 24th	BABETTE E. STERN and E. S. STERN. Efficiency of mental hospitals
May 29th	F. HALBERG. Temporal parameters in biology. This meeting was preceded by the Annual General Meeting of the Section.

The attendances averaged 60.

The Industrial Applications Section

The Section Committee has consisted of: (*a*) appointed by Council: Mr B. P Emmett (Honorary Secretary of the Society and of the Local Groups Coordinating Committee), Professor G. A. Barnard, Professor R. L. Plackett; (*b*) nominated by the 1960–61 Section Committee: Mr D. A. Hopkins, Mr A. N. James, Mr W. N. Jessop, Mr. E. Jowett, Mr R. H. S. Lesser, Dr T. I. McGloughlin, Mr D. J. Page, Mr W. A. Pridmore, Dr K. D. Tocher, Mr I. H. Trueman; (*c*) representing Local Groups: Mr B. K. Burford, Mr J. C. Eaton, Mr W. A. Hay, Mr F. I. Musk, Mr J. Pedoe, Dr C. Priestner, Dr H. Ruben, Mr J. M. Runcie, Mr J. P. L. Truesdale.

Dr K. D. Tocher was elected Chairman, Dr T. I. McGloughlin Vice-Chairman, Mr E. Jowett, London Meetings Secretary, and Mr D. A. Hopkins was re-elected Honorary Secretary.

The following meetings were organized during the session:

1961	
October 6th	A. F. COWAN. Quality control—the statistical content as a means to an end. (Presented by P. DAISLEY)
December 1st	Debate on the motion "That operational research has replaced statistics"—proposed by R. SOLT and T. I. MCGLOUGHLIN, opposed by D. V. LINDLEY and F. B. LEECH
1962	
February 2nd	One-day Conference on The Analysis of Quality: S. J. MORRISON. Target distributions for quality control R. H. S. LESSER. Quality control of assembly operations P. G. MOORE. Regression as an analytical tool
April 13th	R. C. TOMLINSON. Statistical aspects of a stock control model and similar models A. MUIR. Statistical models for simulation
May 4th	R. D. ELSTON. A statistician in automatic data processing. This meeting was preceded by the Annual General Meeting of the Section.

The one-day Conference was attended by 76 members. The average attendance at the other four meetings was 53.

In addition to these activities the Section participated with the Research Section in a joint Conference held at Bristol in September 1961, details of which are given earlier in this Report.

The Local Groups

The Local Groups Coordinating Committee for the session has been: (*a*) appointed by Council: Miss J. Keen; (*b*) nominated by the 1960–61 Committee and approved by Council: Mr W. N. Jessop, Mr D. J. Page; (*c*) representing Local Groups: Miss F. Conway, Mr J. C. Eaton, Mr J. Luckman, Mr A. Muir, Dr E. Page, Mr W. S. Paige, Dr C. Priestner, Mr J. M. Runcie, Mr J. P. L. Truesdale; (*d*) representing Sections: Mr L. A. Dicks-Mireaux, Mr H. F. Downton, Mr D. A. Hopkins, Dr S. Rosenbaum; (*e*) *ex officio* as Honorary Secretary of the Society: Mr B. P. Emmett.

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Mr J. P. L. Truesdale was re-elected Chairman and Mr B. P. Emmett Honorary Secretary.

The Officers of the Local Groups were:

Group	*Chairman*	*Honorary Secretary*
Birmingham and District	Mr F. I. Musk	Mr J. Luckman
Bristol	Mr B. K. Burford	Mr W. S. Paige
East Midlands	Mr H. F. Downton	Miss F. Conway
Edinburgh	Dr R. W. Gloyne	Mr. R. S. Howell
Glasgow	Mr J. V. Gregg	Dr A. J. Howie
Merseyside	Dr C. F. A. Roberts	Mr D. R. Judd
North-Eastern	Dr D. J. Newell	Mr J. M. Runcie
Sheffield	Dr K. D. Tocher	Mr A. Muir
South Wales	Mr W. A. Hay	Dr E. Page

End-of-year membership and average attendances at meetings of the various Groups (excluding joint meetings with other organizations) for the year ended December 31st, 1961, were as follows, the comparative figures for 1960 being shown in brackets:

	Fellows	*Non-Fellows*	*Total Membership*	*Average Attendance*
Birmingham and District	55 (53)	11 (14)	66 (67)	18 (17)
Bristol	23 (22)	12 (11)	35 (33)	23 (25)
East Midlands	18 (21)	5 (16)	23 (37)	12 (21)
Edinburgh	11 (12)	7 (5)	18 (17)	15 (10)
Glasgow	15 (12)	11 (6)	26 (18)	17 (18)
Merseyside	34 (35)	17 (20)	51 (55)	13 (17)
North-Eastern	35 (36)	7 (6)	42 (42)	19 (19)
Sheffield	47 (44)	17 (17)	64 (61)	23 (19)
South Wales	30 (25)	8 (8)	38 (33)	21 (14)
	268 (260)	95 (103)	363 (363)	

Fellows who are recorded as members of Local Groups are those who signify their wish to be attached to a particular Group for the purposes of voting in its elections and being eligible to serve on its Committee.

The accounts of the Local Groups for the calendar year 1961 are summarized below:

Expenditure

	£	s.	d.	£	s.	d.
Birmingham and District	49	8	8			
Bristol	32	8	8			
East Midlands	32	13	6			
Edinburgh	43	7	11			
Glasgow	52	14	9			
Merseyside	20	14	0			
North-Eastern	34	11	0			
Sheffield	31	1	0			
South Wales	36	4	0			
				333	3	6
Carried forward to 1962				198	19	9
				£532	3	3

Income

	£	s.	d.
Brought forward from 1960	258	6	3†
Grants from Council	270	0	0
Other income	3	17	0
	£532	3	3

† Revised figure.

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The meetings held during the session by the various Local Groups were as follows:

Birmingham and District Group

1961	
October 12th	C. CHERRY. Information theory
November 9th	D. R. COX. Prediction
December 14th	G. M. JENKINS. Evolutionary operation (E.V.O.P.)
1962	
January 11th	P. ARMITAGE. Statistical experimentation in clinical medicine
February 8th	L. T. WILKINS. A science of management?
March 8th	R. L. ACKOFF. Quality control of policy
April 12th	J. A. NELDER. An analysis of plant growth

Bristol Group

1961	
October 19th	A. S. C. EHRENBERG. What is the use of the straight lines we fit?
November 16th	F. B. LEECH. Survey analysis on electronic computers
December 14th	ETHEL H. L. DUNCAN. The health of Bristol
1962	
January 18th	H. A. RANDALL. The problem of work measurement
February 15th	R. B. ROWSON. The use of statistics by management with particular relation to electricity supply
March 15th	P. D. OLDHAM. How to cure the common cold
May 3rd	RICHARD STONE. Perspective planning versus forecasting

East Midlands Group

1961	
September 28th	HILDA M. DAVIES. Practical experimentation as an aid to the teaching of statistics
October 19th	G. A. COUTIE. Some examples of exponential smoothing
November 9th	M. D. SKILLICORN. Current trends in retail distribution as revealed by published statistics
December 7th	E. H. INGAMELLS. Reliability
1962	
February 8th	W. N. JESSOP. Simulation methods and industrial problems
March 15th	J. P. R. TOOTILL. Some non-linear estimation problems in pharmaceutical research

Edinburgh Group

1961	
October 10th	R. HODGEN. Application of statistics to traffic engineering
November 14th	W. Z. EILLEWICZ. A statistical assessment of adiposity
December 12th	I. G. STEWART. Input–output analysis
1962	
January 9th	B. P. EMMETT. Audience research in the B.B.C.
February 13th	B. WOOLF. How not to cook raw data: or spurious precision in measurement and assay
April 10th	E. S. PAGE. Process control by cumulative sums

Glasgow Group

1961	
October 19th	G. A. BARNARD. New uses of computers in statistics
November 15th	J. TRAVERS. Statistical aspects of the 1961 Population Census
December 12th	A. W. MCNAB. The business game
1962	
January 17th	D. SMITH. A practical application of design of experiments
February 7th	A. Q. MORTON. The use of statistics in literary criticism
March 6th	W. N. JESSOP. Simulation, Monte Carlo and industrial problems
April 12th	A visit to the Glacier Metal Co. Ltd., Kilmarnock

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Merseyside Group

1961	
October 10th	H. Boyd. Estimating the mortality of wildfowl
November 21st	P. Whittle. The optimal regulation of linear systems. (Joint meeting with the North-West Operational Research Group)
December 12th	A. Muir. Statistical models for simulation
1962	
January 9th	A. Young. Promotion and recruitment in expanding organizations
February 13th	J. A. Proudlove. Statistical aspects of a traffic plan for a city
March 13th	C. Mack. Queues with a finite number of differing customers
April 10th	D. R. Cox. Prediction

North-Eastern Group

1961	
October 11th	R. H. Woodward. The use of cumulative sum techniques
November 8th	D. G. Owen. Some problems of using computers in the steel industry
December 13th	L. T. Wilkins. Prediction methods for Borstals
1962	
January 17th	R. C. Tomlinson. Operational research in a stock control problem
February 14th	B. Benjamin. The 1961 census
March 7th	G. E. H. Reuter. Markov processes

Sheffield Group

1961	
October 26th	H. Chernoff. Accelerated life testing
November 30th	D. J. Bartholomew. The promotion problem
1962	
January 25th	S. J. Morrison. Target distributions for quality control
February 22nd	T. Lewis. Some properties of point processes
March 29th	P. H. Smith. Who are the hire purchasers?
April 26th	H. Ruben. Estimation in Markoff processes

South Wales Group

1961	
September 28th	J. Spraos. Supply and demand in the cinema industry
November 17th	P. G. Moore. The frontiers of multiple regression
December 18th	N. L. Johnson. Statistics in the actuarial profession
1962	
January 16th	M. J. R. Healy. A repertoire of computer programmes for research statistics
February 21st	C. Cherry. The basis, purpose and applications of information theory
March 21st	C. E. Rossiter. Reduction of outpatient waiting times

The Journal

The Journal Committee, under the chairmanship of Professor E. S. Pearson, has continued to review the size, print orders and stock of the *Journal*, *Series A* and *Series B*. The arrangement with a reprint house (reported in the Annual Report for 1959–60) has relieved the office of some work, but the Society still holds the stock and handles the sales of the last five years' volumes, and the Journal Committee is responsible for reviewing and maintaining stocks of back issues for the whole of *Series B* and for the last 20 years of *Series A*.

Professor C. A. Moser continued as Editor of *Series A*, and Mr C. B. Winsten replaced Dr N. T. J. Bailey as Joint Editor, with Professor D. R. Cox, of *Series B*. The Editors were assisted by the following:

Series A, Editorial Board: Professor P. Armitage, Miss S. V. Cunliffe (until December 1961) and Miss M. Deane (from January 1962), Associate Editors;

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Mr D. G. Champernowne, Professor E. Grebenik, Mr M. J. R. Healy, Mr J. L. Nicholson, Mr C. T. Saunders, Dr S. Vajda and Dr G. B. Wetherill.

Series B, Editorial Board: Professor M. S. Bartlett (Chairman), Professor G. A. Barnard, Professor H. E. Daniels, Dr D. J. Finney, Dr J. M. Hammersley, Dr N. L. Johnson, Mr D. G. Kendall, Dr M. G. Kendall, Mr T. Lewis, Professor D. V. Lindley, Dr E. S. Page, Professor R. L. Plackett, Dr C. A. B. Smith, Dr K. D. Tocher, Dr S. Vajda, Mr A. M. Walker, Mr D. M. G. Wishart, Dr F. Yates.

Although the number of pages in each volume has continued to increase, for both *Series A* and *B*, the pressure on space remains. The number of papers submitted for publication, particularly to *Series B*, has increased substantially during recent months, and a higher proportion of competent papers must be rejected if the publication delay is to remain tolerable. The print orders have been increased to 5,200 for *Series A* and 3,700 for *Series B*.

The Journal Committee and the Council have recently considered whether any changes are called for in the number, or characteristics, of the Society's publications. The Council has decided that its future Journal policy should be given further consideration, and has appointed an *ad hoc* Committee to make recommendations. The members of this Committee are: Dr B. Benjamin (Chairman), Professor M. S. Bartlett, Dr W. R. Buckland, Miss S. V. Cunliffe, Professor E. Devons, Mr W. N. Jessop, Professor E. S. Pearson, Mr C. T. Saunders, Mr. L. H. C. Tippett.

Applied Statistics

The 1961 volume of *Applied Statistics* contained 192 pages. The supply of papers at the beginning of the year was scanty; the position has now improved, but the improvement occurred too late to bring the volume up to its usual size. It is essential that a good supply of papers should be maintained. One of the aims of this journal is to cater for the industrial statistician, yet the present volume has only three papers with a pronounced industrial flavour. This number is disappointingly small in relation to the considerable number of statisticians employed by British industry.

After the expenses additional to those shown in the publisher's account have been deducted, there is a surplus for the year of approximately £650. The accumulated surplus is about £2,780. The number of subscribers in the last three years is shown below:

	Fellows	*Non-Fellows*	*Total*
1959	518	1,230	1,748
1960	525	1,265	1,790
1961	470	1,242	1,712

Dr D. G. Beech has continued as Editor and Mr L. A. Dicks-Mireaux as Assistant Editor. The other members of the Committee are at present Dr N. T. J. Bailey, Mr J. A. Bound, Dr W. R. Buckland, Mr A. N. James, Mr W. A. Pridmore, Mr C. T. Saunders, Mr E. Shankleman, Mr P. J. Stanley, Dr Lilli Stein, Dr K. D. Tocher, Mr L. T. Wilkins and Mr M. Whyte.

The Library

The Library Committee for the session has been: Miss M. Deane (Chairman), Dr W. R. Buckland, Dr T. E. Easterfield, Mr D. J. Farlie, Dr F. G. Foster, Mr W. N. Jessop, Mr A. Maizels, Mr J. L. Nicholson, Mr C. T. Saunders, Mr W. F. Searle and Mr A. Stuart.

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The number of calls made on the borrowing services of the Library fell sharply in 1961 compared with the previous four years. The following analysis does not include books consulted for reference in the reading rooms.

	1957	1958	1959	1960	1961
Number of volumes borrowed	3,724	3,691	3,292	3,838	2,831
Number of volumes not possessed by the Society at time of request	295	286	340	326	235
Total number of volumes asked for	4,529	4,390	4,025	4,571	3,361
Non-serial works added to the Library	957	922	881	769	911

Fellows made greater use of the facilities offered for tracing and borrowing volumes not in the Society's Library through the National Central Library, the Science Museum Library and the Science Lending Library. Thirty-four requests were made through these services; six were unsuccessful. In 1960 ten such requests were made, one being unsuccessful.

The National Central Library applied to the Society's Library for 56 volumes of which 17 were not available for lending. In 1960, out of 81 volumes requested 39 were not available.

The volumes of marginal interest which were stored at the London School of Hygiene and Tropical Medicine were removed from there in December 1961. Approximately 10,000 volumes were sent to the British Museum, 2,500 to the British Library of Political and Economic Science and other libraries, 7,000 sold or offered for sale, and the remainder stored.

The Library Committee is now reviewing matters of policy related to the ultimate size and specialization of the Library.

Colleges of Technology

The Society has been represented on the Applied Science Advisory Committee of both the Birmingham College of Advanced Technology (by Mr W. N. Jessop) and the Bristol College of Technology (by Mr W. Ross Goobey).

British Productivity Council

Professor E. S. Pearson has represented the Society on the National Council for Quality and Reliability.

Traffic and Travel Surveys

In October 1961, the Council appointed a Committee consisting of Professor G. A. Barnard, Dr W. R. Buckland and Mr C. B. Winsten to prepare a memorandum on the contribution of the statistician to the solution of traffic problems. In February 1962, the Committee presented a report on Traffic and Travel Surveys. A copy of the report was sent to the Minister of Transport, and in April 1962, a meeting took place with representatives of the Ministry. The Society was represented by the President and Dr Buckland, and the meeting was attended also by Professor Barnard as Chairman of the Institute of Statisticians and by Mr L. R. England as Chairman of the Market Research Society.

At this meeting the representatives of the Ministry were informed of certain misgivings the Society had concerning the London Travel Survey, and they gave a number of reassurances. The Ministry observed that future surveys would be the responsibility primarily of local authorities. The Ministry would wish to give advice to local authorities to ensure that local efforts were well applied and coordinated,

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and would be anxious to take advantage of any help which the Society could give. It was left to the Ministry to suggest what form any further help from the Society might take.

Chair in Mathematical Statistics at Cambridge

At the request of the Mathematics Faculty of the University of Cambridge the Council agreed to sponsor an appeal for funds to establish a Chair in Mathematical Statistics. This appeal was directed to a number of industrial and commercial organizations in an endeavour to raise £4,000 for each of ten years, after which time the University will absorb the cost of the chair in its ordinary expenditure. The appeal was completely successful and the chair has now been established. The President, Dr M. G. Kendall, and a Past President, Professor E. S. Pearson, were invited to join the Electors, and Mr D. G. Kendall has been appointed to the Chair. The organizations which responded to the appeal for funds were:

Associated Electrical Industries Ltd
The Attwood Group of Companies
Bowater Paper Corporation Ltd
British Petroleum Co. Ltd
C.E.I.R. (U.K.) Ltd
Charities Aid Fund (Arthur Guinness Son & Co.)
Courtauld's Ltd
Electrical and Musical Industries Ltd
Ferranti Ltd
Imperial Chemical Industries Ltd
International Computers and Tabulators Ltd
Pilkington Bros. Ltd
Research Services Ltd
Shell Petroleum Co. Ltd
Unilever Ltd
United Steel Companies Ltd

Barclays Bank
Coutts & Co., Bankers
Glyn Mills & Co.
Lloyds Bank
Midland Bank
National Provincial Bank
Westminster Bank
Williams Deacon's Bank

Sessional Dinner

It was decided, as an experiment, to hold a Sessional Dinner of the Society on December 5th, 1961, at the Connaught Rooms. Some 150 Fellows and guests attended and the principal speech was delivered by Sir Edward Boyle, Bart., Financial Secretary to the Treasury.

Review of Society Policy

During the year, following proposals made by Dr D. J. Finney, a special committee was appointed "to consider whether the Society is sufficiently conscious of its responsibility for furthering the collection and dissemination of quantitative information on social phenomena and if not how it can be more effective in furthering this aim". One direct outcome of the many suggestions made to Council by the Committee was the Ordinary Meeting on April 6th, 1962, taking the form of a one-day conference on "Food Supplies and Population Growth" at the London School of Economics. This was attended by some 200 Fellows and visitors and was well reported in the press. Another result was the memorandum referred to above on the contribution of the statistician to the solution of traffic problems.

Finance

A statement of income and expenditure in 1961, and the balance sheet at December 31st, 1961, together with the Auditor's report thereon, are shown at Appendices A and B.

Both expenditure and income were reduced in 1961. This was because the normal rhythm of publication of the *Journal* was restored. (In 1960, to catch up publication

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delays resulting from the printing strike in 1959, six numbers of *Series A* had been issued, against the normal four, and three numbers of *Series B* instead of the normal two.)

In 1961, compared with the year before, the cost per issue of both Series of the *Journal* rose. For *Series A*, the average cost per issue rose very little (from £1,429 to £1,471); the extra cost was more than covered by extra sales to non-Fellows. The deficit per issue on current issues of *Series A* (allowing no credit for free distribution to Fellows) fell from £466 to £408. For *Series B*, however, the rise in cost was greater (from £1,680 to £2,152 per issue) largely because of an increase of nearly 20 per cent. in the average size of issue. Sales to non-Fellows also rose, but the deficit increased from £211 to £481.

At the same time, there was a fall in the sales of back issues of both Series (mainly because of the smaller number of issues). However, sales of back numbers, at nearly £2,000 a year for both Series, remain substantial. The deficit on total transactions on the *Journal*, including both current and back numbers (always allowing no credit for Fellows' subscriptions), therefore rose from £311 in 1960 to £1,397 in 1961. The latter figure must be regarded as closer to what must be expected in future at present printing costs and prices.

Apart from the *Journal*, expenditure was up, from £9,267 in 1960 to £11,014 in 1961—an increase of £1,747. Publication of the new Fellows' List accounts for an increase of £809. Salaries and wages were up by £808; a long-standing vacancy on the staff was filled, and National Insurance contributions rose. In addition, it was decided to increase the annual allocation to amortization of the leasehold premises from £425 (which was based on the original cost) to £925 (which is closer to the amount likely to be needed for "replacement" when the lease expires, although still far from sufficient at current property values). On the other hand, expenditure on repairs and on office equipment was reduced; in 1960, there had been some heavy special costs under both heads.

Income, apart from *Journal* sales, was up from £11,190 to £12,626—a rise of £1,436. Fellowship subscriptions rose by £301—continuing the very stable rate of increase in the Fellowship which has continued for more than ten years. *Applied Statistics* gave a surplus of £665—again a record. Dividends and interest continued to rise, from £2,077 to £2,190.

In total, the Society's income exceeded expenditure in 1961 by £215—considerably less than the £1,612 surplus of 1960, and a margin of only 1 per cent. on total expenditure. The balance was added to the Accumulated Fund, which was, however, reduced by a loss of £1,640 on sale of investments. At December 31st, 1961, the Fund stood at £14,358.

The Society's investment portfolio (described in Appendix C) stood at £39,966 at December 31st, 1961, valued at cost. The market value was £42,848. It was decided to cease the allocation of particular securities to the Leasehold Sinking Fund, and those securities are now included under "General Investments" in the Assets shown on the balance sheet. (The accumulated allocation for amortization, and interest thereon, is shown separately under Liabilities.)

The portfolio of gilt-edged stock was sold in early 1961, and the proceeds reinvested in longer-dated or higher-coupon gilt-edged and industrial debentures. The average yield on fixed interest stocks has been increased from 5·4 per cent. of market value at the end of 1960 to 6·4 per cent. at the end of 1961; adding about £290 a year to the Society's income.

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Of the market value of the Society's portfolio at the end of 1961, 43 per cent. consisted of preference and ordinary stocks (34 per cent. of book value), with an average yield of 4·15 per cent. on market value. The annual yield on the whole portfolio was 5·46 per cent.

Accommodation

The Council expresses its gratitude to the Dean and Board of Management of the London School of Hygiene and Tropical Medicine for their generosity in allowing the use of their lecture theatres during the session for the meetings of the Society and of the Medical Section; to the Director of the London School of Economics for providing accommodation for Council meetings; to the Leicester College of Technology for providing accommodation for meetings of the East Midlands Group; to the University of Sheffield for providing accommodation for meetings of the Sheffield Group, and to the University of Bristol for providing a room for the meetings of the Bristol Group.

The Council and Officers

Dr M. G. Kendall has completed two years of office as President and is therefore not eligible for re-election. The Society is greatly indebted to him for his distinguished service as President. His wide experience in academic and commercial activities and his international reputation as an eminent statistician have been of great value to the Society. His leading role in the establishment of a Chair in Mathematical Statistics in Cambridge is recorded with particular gratitude.

The Council has received with pleasure the consent of Dr J. O. Irwin to allow his nomination as President for next session.

The Honorary Officers are eligible and have been proposed for re-election with the exception of Dr B. Benjamin whose resignation has been received with regret.

Dr Benjamin has held the office of Honorary Secretary with distinction since 1955, acting as senior Honorary Secretary since 1958. During this period he has served the Society with enthusiasm, devotion and an administrative ability which has left its mark on the Society's affairs.

The Council is fortunate in having obtained the consent of Miss S. V. Cunliffe to be nominated to the office of Honorary Secretary.

The members of the Council who retire at the close of the current session are: Dr W. R. Buckland, Miss I. Douglas, Dr D. J. Finney, Sir Robert Shone, Mr A. Stuart and Mr H. Tetley. The Council warmly acknowledges their services. A vacancy will also be created by the proposed appointment of Miss Cunliffe as Honorary Secretary.

Having observed the requirements of the bye-laws and considered suggestions put forward by the Fellows, the Council recommended that the vacancies be filled by the election of: Dr B. Benjamin, Professor J. Durbin, Mr T. Lewis, Mr W. Rudoe, Mr W. F. Searle, Mr G. W. Sears and The Rt. Hon. J. Harold Wilson, M.P. The composition of the Council for the session 1962–63 as recommended by the Council is thus as follows. (Those whose names are marked * were not members of Council during the previous session. Those marked † have not previously served on Council.)

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President
J. O. Irwin

Council

G. A. Barnard
M. S. Bartlett
B. Benjamin
J. A. Bound
D. G. Champernowne
Colin Clark
H. E. Daniels
Marjorie Deane
*J. Durbin
R. F. George
J. A. Heady
M. J. R. Healy
Joan Keen
†T. Lewis
D. V. Lindley
C. A. Moser
J. L. Nicholson
D. J. Page
R. L. Plackett
†W. Rudoe
*W. F. Searle
*G. W. Sears
K. D. Tocher
J. P. L. Truesdale
†J. Harold Wilson
C. B. Winsten

Honorary Treasurer
C. T. Saunders

Honorary Secretaries
P. Armitage Stella V. Cunliffe B. P. Emmett

One alternative proposal for ordinary membership of Council has been received (under bye-law No. 14) and a postal ballot is being held (under bye-law No. 15). The result of the ballot will be declared at the Annual General Meeting.

On behalf of the Council,

M. G. KENDALL, *President.*

B. BENJAMIN
P. ARMITAGE
B. P. EMMETT
} *Honorary Secretaries.*

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STATEMENT OF INCOME AND EXPENDITURE

EXPENDITURE

1960 £	£		£	1961 £
131		Rents payable *less* rent received		141
742		House expenses		769
184		Repairs and painting		68
3,984		Salaries and wages (including contribution to staff superannuation scheme)		4,792
300		Pension		300
69		Insurance		72
644		Office and Library equipment		170
611		Postage and telephone		672
735		Stationery and miscellaneous printing		818
		Publication and distribution of Journals:		
	8,575	Series A—Current issues	5,886	
	—	Reprints	282	
	5,040	Series B—Current issues	4,305	
	561	Reprints	456	
14,176	——		——	10,929
		Library:		
	521	Books	591	
	119	Binding	—	
640	——		——	591
54		General Meetings—ordinary and annual		76
83		Council and committee travelling expenses		178
		Expenses of Sections:		
	20	Research	23	
	57	Industrial Applications	6	
	45	General Applications	47	
	39	Medical	36	
161	——		——	112
227		Expenses of Local Groups		270
43		Guy Medals		7
53		Auditor's fee (1960)		53
425		Amortization of leasehold premises		925
46		Income tax irrecoverable		69
—		Society Dinner		71
—		List of Fellows		809
40		*Deficits on Conferences*		—
46		Miscellaneous expenses		23
——				——
23,394				21,915
1,612		Surplus carried to Accumulated Fund		215
——				——
25,006				22,130
49		Amount carried to Life Composition Fee Fund ...		28
——				——
£25,055				£22,158
——				——

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DIX A

FOR THE YEAR ENDED 31st DECEMBER, 1961

1960		INCOME	1961	
£	£		£	£
		Annual subscriptions:		
	8,184	Fellowship	8,485	
	22	Research Section	20	
	49	Industrial Applications Section	47	
	6	General Applications Section	5	
	25	Medical Section	38	
	97	Local Groups	82	
8,383				8,677
50		Contribution from Royal Economic Society		50
	2,077	Dividends and interest (gross)	2,190	
		Less: Apportioned to Leasehold Sinking Fund and		
	180	Frances Wood Memorial Fund	199	
1,897				1,991
		Sales of Journal:		
	5,777	Series A—Current issues	4,253	
	1,865	Back numbers	802	
	4,407	Series B—Current issues	3,342	
	1,816	Back numbers	1,135	
13,865				9,532
642		*Applied Statistics*—Surplus for year		665
40		Sales of other publications		21
—		Surplus from Conferences		117
		Transfer from Life Composition Fee Fund, being fees of compounders known to have died during year and of		
129		those presumed to have died in previous years ...		185
—		Sale of Books from Library		892
25,006				22,130
49		Life Composition Fees received during year		28
£25,055				£22,158

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APPEN

BALANCE SHEET AT

1960 £	£	FUNDS AND LIABILITIES	1961 £	£
		Accumulated Fund:		
13,529		Balance at 31st December, 1960	15,784	
643		Loss (1960 profit) on Sale of Investments	1,641	
14,172			14,143	
1,612		*Add:* Balance per Statement of Income and Expenditure	215	
	15,784			14,358
		Life Composition Fee Fund:		
8,232		Balance at 31st December, 1960	8,152	
49		*Add:* Life Composition Fees received during year ...	28	
8,281			8,180	
		Less: Transfer to Statement of Income and Expenditure of fees of compounders known to have died during year, and of those presumed to have died in previous years	185	
	8,152			7,995
		Building Fund:		
	1,661	Balance at 31st December, 1960		1,661
		1950 Benefaction Fund:		
	30,971	Balance at 31st December, 1960		30,971
		Frances Wood Memorial Fund:		
654		Balance at 31st December, 1960	684	
30		Interest for year (apportioned)	30	
	684			714
		Leasehold Sinking Fund:		
3,639		Balance at 31st December, 1960	4,214	
150		Interest for year (apportioned)	169	
425		Provision for year per Statement of Income and Expenditure	925	
	4,214			5,308
		Liabilities and Income held in suspense:		
2,718		Creditors	4,725	
2,680		Amounts received in advance	3,015	
	5,398			7,740
	£66,864			£68,747

REPORT OF

I have examined the above balance sheet and annexed statement of income and Society's affairs as at 31st December, 1961, and of its income and expenditure for the year to me and as shown by the books.

5 London Wall Buildings,
London, E.C.2.
21st May, 1962

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DIX B

31st DECEMBER, 1961

1960 £	£	ASSETS	1961 £	£
		Leasehold Premises:		
21,368		At Cost (see Sinking Fund *per contra*)		21,368
		Investments (including investment of Leasehold Sinking Fund and Frances Wood Memorial Fund):		
41,424		At cost, per schedule attached ... ... (Market Value 31/12/60, £43,588; 31/12/61, £42,848)		39,966
		Current Assets:		
1,715		Debtors and payments in advance	2,739	
100		Arrears of subscriptions recoverable (estimated) ...	100	
150		Income tax recoverable	181	
2,107		Balances at banks and cash in hand	4,393	
	4,072			7,413
£66,864				£68,747

NOTES—

1. No value is placed in the Accounts on—
   (a) Journals and other publications in stock.
   (b) Books in library.
   (c) Pictures, furniture and equipment.
2. Interest credited to Leasehold Sinking Fund for the year represents 4% on the amount of the Fund at the beginning of the year.

M. G. KENDALL *President.*

CHRISTOPHER T. SAUNDERS *Honorary Treasurer.*

THE AUDITOR

expenditure which, in my opinion, respectively give a true and fair view of the state of the ended on that date according to the best of my information and the explanations given

W. G. DENSEM,
Chartered Accountant.
(Deloitte, Plender, Griffiths & Co.)

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APPEN

SCHEDULE OF INVESTMENTS—

1960 Market Value £	1960 Cost £		Nominal	1961 Cost £	1961 Market Value £
		British and other Government and Municipal:			
—	—	5¼% Conversion Stock 1974	£3,500	3,271	3,238
2,124	2,992	3% Savings Bonds 1965–75	—	—	—
—	—	5% Treasury Stock 1986–89	£2,500	1,996	2,062
—	—	5½% Treasury Stock 2008–12	£4,000	3,653	3,400
9,112	11,224	British Electricity 3½% Stock 1976–79 ...	—	—	—
1,842	2,020	British Electricity 4¼% Stock 1974–79 ...	—	—	—
990	1,012	Newcastle-upon-Tyne Corporation 6% Redeemable Stock 1973–76	—	—	—
1,010	985	New Zealand 6% Stock 1976–80	—	—	—
—	—	Australia 6% Stock 1981–83	£2,000	1,940	1,890
£15,078	£18,233			£10,860	£10,590
		Debenture and Unsecured Loan:			
478	481	Allied Suppliers Ltd. 6% Unsecured Loan Stock 1982–87 ...	£500	481	445
935	1,031	British Petroleum Co. Ltd. 6% Conv. Debenture Stock 1976–80 ...	£1,000	1,031	900
—	—	Colvilles Ltd. 4½% Debenture Stock 1975–85	£1,400	1,049	1,008
955	1,011	Decca Record Co. Ltd. 6% Unsecured Loan Stock 1980–85 ...	£1,000	1,011	895
442	492	Esso Petroleum Co. Ltd. 5½% First Debenture Stock 1979–83 ...	£500	492	424
494	524	Foreign & Colonial Investment Trust Co. Ltd. 4¼% Debenture Stock 1982–87	£700	524	465
1,030	1,040	Hawker Siddeley Group Ltd. 5¾% Conv. Debenture Stock 1979–84 ...	£1,000	1,040	865
905	980	Metal Box Co. Ltd. 5½% Unsecured Loan Stock 1977–80 ...	£1,000	980	865
845	975	Peninsular & Oriental Steam Navigation Co. Ltd. 5% Debenture Stock 1975–80	£1,000	975	785
482	490	Albert E. Reed & Co. Ltd. 6% Debenture Stock 1979–84	£500	490	442
465	520	Richardsons, Westgarth & Co. Ltd. 6% Conv. Unsecured Loan Stock 1976–81	£500	520	310
—	—	Steel Company of Wales Ltd. 5½% First Debenture Stock 1980–85 ...	£1,100	956	930
—	—	Thorn Electrical Industries Ltd. 6½% Unsecured Loan Stock 1985–89 ...	£4,000	3,940	3,820
860	955	Tube Investments Ltd. 5¼% Unsecured Loan Stock 1977–82 ...	£1,000	955	805
975	1,002	George Wimpey & Co. Ltd. 6% Unsecured Loan Stock 1975–80 ...	£1,000	1,002	900
£8,866	£9,501			£15,446	£13,859

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DIX C
31st DECEMBER, 1961

1960 Market Value £	1960 Cost £		Nominal	1961 Cost £	1961 Market Value £
		Preference and Ordinary:			
1,896	*987*	Associated Portland Cement Manufacturers Ltd. Ordinary Stock	£805	986	2,163
—	—	Beaumont Property Trust Ltd. Ordinary 5/– Shares	1,750	947	1,116
1,124	*1,025*	Bowater Paper Corporation Ltd. Ordinary Stock	£420	1,025	924
952	*1,038*	English Electric Co. Ltd. Ordinary Stock	£544	1,038	619
977	*1,012*	Ferranti Ltd. 7% Cumulative Preference Stock ...	—	—	—
1,580	*1,006*	Foreign & Colonial Investment Trust Co. Ltd. Ordinary Stock	£702	1,006	2,053
2,150	*1,126*	Globe Telegraph & Trust Co. Ltd. Ordinary Stock	£682	1,126	2,594
—	—	Great Universal Stores Ltd. "A" Ordinary Stock	£81	827	894
1,892	*969*	Guest, Keen & Nettlefolds Ltd. Ordinary Stock	£525	1,119	2,067
2,064	*978*	Imperial Chemical Industries Ltd. Ordinary Stock	£678	1,069	1,907
853	*948*	International Combustion (Holdings) Ltd. Ordinary Stock	—	—	—
2,738	*1,047*	Plessey Co. Ltd. Ordinary 10*s*. Shares ...	—	—	—
875	*990*	Albert E. Reed & Co. Ltd. "A" Ordinary £1 Shares	400	990	765
—	—	Scottish Agricultural Industries Ltd. 4½% Second Cumulative Preference £1 Shares	1,500	906	938
470	*535*	"Shell" Transport & Trading Co. Ltd. Ordinary Stock	£86	532	520
508	*496*	South Staffordshire Waterworks Co. Ordinary Stock Class "B" (7% Max.) ...	£500	496	458
922	*817*	Tube Investments Ltd. Ordinary Stock ...	£250	817	789
643	*716*	United Steel Companies Ltd. Ordinary £1 Shares	300	776	592
£19,644	£13,690			£13,660	£18,399
£43,588	£41,424	TOTAL INVESTMENTS		£39,966	£42,848

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Proceedings of a Special General Meeting of the Royal Statistical Society

Held at the London School of Hygiene and Tropical Medicine, Keppel Street, London, W.C.1, on Wednesday, June 20th, 1962, at 5.30 p.m.

The Chair was taken by the President, Dr M. G. Kendall.

The Notice convening the meeting was, with the consent of the meeting, taken as read.

The President moved the Resolution contained in the notice:

"That the bye-laws of the Society be amended as follows:

1. By adding after "Fellow" in the second sentence of Bye-Law 5 the words, "other than Fellows representing bodies or institutions" and to add a new second paragraph "Two Fellows who are married to each other may pay a joint subscription of four and a half guineas subject to their receiving only one journal issue between them".
2. By re-numbering the Bye-Laws so that the present Bye-Laws 13–17 now come between the present Bye-Laws 30 and 31.
3. To amend the word "twenty-five" in Bye-Law 29 to "fifty".
4. To alter the words "local Sections" in line 3 of Bye-Law 37 to "local Groups" and to alter "or local Section" in line 8 to "Section or Local Group".

Professor D. R. Cox seconded the motion.

The Resolution was then put to the meeting and carried unanimously.

The Proceedings then terminated.

The Special General Meeting was followed by the Eighth Ordinary Meeting in the session at which the candidates named below were elected Fellows of the Society:

BATES, John Maynard, B.A.
BEAR, Donald, B.Sc.
CASE, Robert Alfred Martin, M.D., Ph.D., M.R.C.S., L.R.C.P.
ELLIOTT, Antony Gilbert Lewis, B.Sc.
GITTINS, John Charles, B.A.
GOLDSTEIN, Harvey, B.Sc.
HOSKING, Zena Dorothy, B.A.
HUDSON, Derek John, B.Sc.
HUTTER, John, B.Sc.
PATEL, Manibhai Sukhabbai
ROSS, Gavin James Stirling, B.A., Dip. Math. Stats.

Corporate Representative

BRAMSON, Michael Joseph *representing* Associated Engineering Ltd.

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Proceedings of the One Hundred and Twenty-eighth Annual General Meeting of the Royal Statistical Society

Held at the London School of Hygiene and Tropical Medicine, on Wednesday, June 20th, 1962, immediately on the conclusion of the Eighth Ordinary Meeting

The Chair was taken by the President, Dr M. G. Kendall.

The President presented the Report of the Council for the financial year 1961 and the session 1961–62, and moved that it be received.

During his submission of the Report, the President presented the Guy Medal in Bronze to Professor P. Armitage.

Dr B. Benjamin presented the accounts and seconded the motion that the Report be received.

On being put to the meeting the Report was formally received.

On the proposal of Miss I. Douglas, seconded by Mr R. F. Fowler, Mr W. G. Densem was unanimously re-elected Auditor of the Society for the session 1962–63 at a fee of fifty guineas.

The President announced that an additional nomination had been received to the list of names recommended by Council as Council members for the session 1962–63. A postal ballot had therefore been held. The result of the ballot was that the President, Council and Officers nominated by the Council, as shown on the list circulated, were duly elected.

The Proceedings then terminated.

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Reviews

PAGE

1.—*Neyman, Jerzy* (Ed.). Proceedings of the Fourth Berkeley Symposium on Mathematical Statistics and Probability. 4 vols. 629

2.—*Ractliffe, J. F.* Elements of Mathematical Statistics . 633

3.—*Alder, Henry L. and Roessler, Edward B.* Introduction to Probability and Statistics (2nd ed.) 634

4.—*Weiss, L.* Statistical Decision Theory 635

5.—*Ray, William S.* Statistics in Psychological Research . 635

6.—*Reichmann, W. J.* Use and Abuse of Statistics . . 636

7.—*Savage, I. Richard.* Bibliography of Nonparametric Statistics . 637

8.—*Kendall, M. G.* A Course in the Geometry of *n* Dimensions . 638

9.—*David, F. N. and Barton, D. E.* Combinatorial Chance . . 639

10.—*Cox, Richard T.* The Algebra of Probable Inference . . 640

11.—*Blyth, Colin R. and Hutchinson, David W.* Tables of Neyman-Shortest Unbiased Confidence Intervals (a) for the Binomial Parameter, (b) for the Poisson Parameter 641

12.—*Hamming, R. W.* Numerical Methods for Scientists and Engineers 642

13.—*Wolfowitz, J.* Coding Theorems of Information Theory . . 643

14.—*Fano, Robert M.* Transmission of Information: A Statistical Theory of Communications . 645

15.—*Simonnard, M.* Programmation linéaire 645

16.—*Titchen, Robert S. and Rosenthal, Arnold J.* (Eds.). Operations Research/Management Science 646

17.—*Duckworth, W. E.* A Guide to Operational Research . . 647

PAGE

18.—*Houlden, B. T.* (Ed.). Some Techniques of Operational Research 647

19.—*Wallis, P. N.* Quality Control in the Office 648

20.—*Ogborn, M. E.* Equitable Assurances 649

21.—*World Health Organization.* Annual Epidemiological and Vital Statistics, 1958 . . 652

22.—*United Nations.* Methods of Estimating Total Population for Current Dates . . 653

23.—*General Register Office.* The Registrar General's Statistical Review of England and Wales . . . 1960, Part II, Tables, Population 653

24.—*General Register Office.* The Registrar General's Quarterly Return for England and Wales. Third Quarter, 1960 . 654

25.—*Williams, G. Prys and Goodwin, Harold S.* Road Risks of the Elderly 655

26.—*O.E.C.D.* Man-Power Statistics, 1950–1960 656

27.—*Church of England, Statistical Unit of Central Board of Finance.* Facts and Figures about the Church of England. . 656

28.—*Gayen, A. K., et al.* Measurement of Achievement in Mathematics: A Statistical Study on Effectiveness of Board and University Examinations in India 658

29.—*Lampman, Robert J.* The Share of Top Wealth-Holders in National Wealth, 1922–56 . 659

30.—*Hall, P. G.* The Industries of London since 1861 . . 660

31.—*Thomas, Brinley* (Ed.). The Welsh Economy: Studies in Expansion 660

32.—*Clement, A. G. and Robertson, Robert H. S.* Scotland's Scientific Heritage . . 662

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1. Proceedings of the Fourth Berkeley Symposium on Mathematical Statistics and Probability, held at the Statistical Laboratory, University of California, June 20 through June 30, 1960. Edited by Jerzy Neyman. Berkeley and Los Angeles, University of California Press, 1961. London, Cambridge University Press. 4 vols. 10″.

The Fourth Berkeley Symposium was held in the summer of 1960, and it is a remarkable tribute to Professor Neyman and his helpers that, although the symposium speakers were allowed to report on their very latest findings, and were not constrained by having to produce a paper long in advance, the *Proceedings*, running to four volumes and about 2,000 pages in all, are already available for review. It behoves reviewers to match this commendable promptitude, so that the general statistical public may know something of what they may expect to find in these four volumes as soon as possible. However, it is more difficult to apply the techniques of team work to reviewing than it is to apply them to writing, and so what follows must be taken as representing only preliminary reactions after a rapid survey of volumes which will clearly repay the most careful and prolonged study.

As usual the four volumes taken together exhibit the breadth of application of statistical and probabilistic ideas, especially in those fields of science which have received special attention from workers at Berkeley and, again as usual, there is more emphasis on probability than on statistics in these applications. Volumes III and IV, which are concerned with scientific applications, are almost wholly devoted to the discussion of probabilistic models for observable phenomena, apart from the application of simple ideas of tests of significance to astronomical data in Volume III. One might have looked in this volume for something in the physical sciences to correspond to Fisher's work on the design of experiments in biology but anyone who does so will find only Youden's paper on weighing and related problems. Volume II is explicitly devoted to general probability theory.

The title "Symposium" might be taken to suggest a meeting of minds, and a discussion. In fact, however, the character of the volumes differs very little from that of the assemblies of papers written in honour of various individuals by authors who have not actually met. An aggregation of work of this sort has its value, in demonstrating the size of the field to be covered; but one who is accustomed to the interchange of views normal at meetings of our Society feels rather severely the lack of printed comments on the papers. Such discussions, on the fields covered by the symposium, will doubtless take place at the meetings of the newly formed physical sciences section of the International Statistical Institute. Indeed, one wonders whether the Berkeley Symposium and the new section may not be arranged to complement each other, by having a session of the Institute's meetings devoted to just such prepared discussion on the previously available symposium proceedings.

Vol. I. Theory of Statistics. ix, xii, 767 p. 128*s*.

To turn now to Volume I, the largest of the four, containing 39 papers, each of about 20 pages on average. Anscombe, De Finetti, Ferguson and Kruskal discuss the problems of "outliers", while Hodges and Lehmann and Hotelling are concerned with closely related matters. Cox, Daniels, Lindley, Rao and Savage discuss some general problems in statistical inference, while Wold raises a closely related issue of principle. There are discussions of more special inference problems by David and Fix, Fortet, Tukey, Parzen, Wilks, Chernoff, Vincze and Ruben; many of these papers are concerned with statistical inference from time series. Another group of papers, by Blackwell, Dobrushin, Renyi, Shannon, Špaček, Thomasian and Wolfowitz are concerned with communication channels and information theory. Finally, there are a number of isolated papers. Birnbaum discusses the probability of failure of complex structures, Bellman discusses adaptive control, Breiman discusses optimal gambling systems, for the rather unrealistic case of

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favourable games. Chow and Robbins give a theorem on martingales which has applications to the maintenance of stores. Dalenius, Hajek and Zubrzycky discuss geometrical problems of sampling in the plane, Dantzig and Madansky discuss linear programming under uncertainty, James and Stein discuss the estimation of multi-normal means, Kitagawa gives a theoretical discussion of evolutionary operation and related methods, Schmetterer discusses stochastic approximation, while Solomon discusses the distribution of quadratic forms in normal variates. Kiefer continues the exposition of his attitude to experimental design. Finally, there is an interesting paper by Roy Radner, entitled "The evaluation of information in organizations", concerned with decision making in organizations having complex structures for internal transmission of information; studies of this kind are of extreme importance for the design of administrative systems. Radner pays special attention to problems arising from "management by exception".

The reviewer cannot resist the temptation to comment on one or two points. Kiefer's statement on p. 398 that the intuitive appeal of Box and Hunter's rotatable designs "has never been justified until the present paper" qualifies for an honoured place in a museum of exhibits of the misleading use in a technical sense of words having an ordinary meaning in current language. What Kiefer means by "justified" is that an abstract criterion of merit (of very doubtful validity) is set up and the designs in question are shown to have high merit in this sense. In the ordinary sense of the word, one might well ask how the special choice of merit function is "justified". In fact, as readers of Box and Draper's paper (*J. Amer. statist. Assoc.*, 1959, **54**) will know, the criteria set out by Kiefer are of doubtful relevance in real situations. The paper by Hodges and Lehmann, comparing the normal scores and the Wilcoxon tests, would have been improved had it included some discussion of the disadvantages of relative asymptotic efficiency as a practical way of comparing tests, and if, in addition, some discussion of the situation usually arising in practice when the alternatives are not simple shifts had been given. De Finetti's paper on outliers is particularly worth reading since it exhibits in a rather sharp way the differences in practice between the Bayesian and other attitudes to statistics. The set of papers on the examination of residuals is as a whole a very thorough treatment of this most important matter. Tukey's paper, which points out that the elementary statistical procedure of forming a histogram is an example of the very sophisticated problem of estimating a curve, is a very good example of his ability to point out how far removed are the activities of practising statisticians from the grossly over-simplified versions of these current among mathematicians. Dobrushin's paper is an extremely useful review of the field, giving special attention to the important contributions made by the Russians in this area. The list of unsolved problems is particularly valuable. Here again the group of articles concerned with information theory provides a very useful survey for statisticians.

Speaking generally, although this reviewer has indicated his opinion that the Berkeley Symposium still leans towards pure mathematics rather than towards the central areas of advance in statistics, the present volumes are certainly an improvement on earlier ones in this respect, and he would like to emphasize that there would be many more complimentary things he would like to add were there space and time available.

G. A. BARNARD

Vol. II. Probability Theory. xii (4) 633 p. 108*s*.

This volume begins with two notes of appreciation of the late A. I. Khinchin, one by B. V. Gnedenko and the other by J. L. Doob. These are followed by 34 papers on various aspects of probability, most of which are written in the pure mathematical vein which characterizes modern probability theory. Twenty of these 34 papers deal directly with stochastic processes of one kind or another. There is little of direct statistical interest in the volume, though one may conjecture that some of the results will find statistical applications in due course. It is difficult to make any other general remarks about the volume, except to say that it is essentially for probabilists, most of whom will find something of interest in this rich collection.

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The following papers were of especial interest to the reviewer: "Non-increase everywhere of the Brownian motion process" by A. Dvoretsky, P. Erdos and S. Kakutani, which demonstrates yet another curious property of sample functions of the Wiener process; "On sequences of sums of independent random vectors" by W. Hoeffding, which gives some general results on random walks on the multi-dimensional lattice of integers with time-dependent absorbing barriers; S. Karlin and J. McGregor, "Occupation time laws for birth and death processes" in which is discussed the detailed application to birth–death processes of some general results obtained for Markov processes by D. A. Darling and M. Kac; Yu. V. Linnik, "On the probability of large deviations of the sums of independent random variables", which extends an investigation started originally in 1938 by H. Cramér into the validity of the central limit theorem for deviations whose size depends on the number of summands; E. Lukacs, "Recent developments in the theory of characteristic functions", a useful survey of recent research; E. J. G. Pitman, "Some theorems on characteristic functions of probability distributions", which furnishes an interesting clarification of the relation between the tails of the distribution function and the behaviour of the characteristic function at the origin; and W. L. Smith, "On some general renewal theorems for non-identically distributed variables".

H. D. MILLER

Vol. III. Contributions to Astronomy, Meteorology, and Physics. xi, 335 p. 56*s*.

Contact between different fields is always a great stimulant in research, and the present volume shows clearly that the Berkeley Symposium continues to provide fruitful opportunities for such contact and exchange of ideas between statistics and the physical sciences.

About half the volume consists of papers dealing with the statistical behaviour of the "population" of comets in the solar system. The motion of a comet in the presence of the sun and a perturbing planet is governed by the complex 3-body equations of classical dynamics. But most of the life of the comet is spent at large distances from the solar system when the orbit can be accurately described as elliptical; and the complex motion during the brief period when it approaches the sun and the planets can be regarded as giving rise to a perturbation of the energy, eccentricity and periodic time of the orbit. Thus when considering the behaviour of large numbers of comets it is reasonable to take a stochastic model which treats each encounter with the solar system as instantaneous, the perturbation energy being a random variable taking positive and negative values and having a specific probability distribution. If the perturbation is such as to make the eccentricity greater than unity, the orbit becomes hyperbolic and the comet disappears from the solar system. Allowance must also be made for the disappearance of comets by a disintegration mechanism; and alternative theories due to Lyttleton and Oort postulate mechanisms for the creation of new comets. Hence it will be seen that the problems which arise are similar to those which have claimed a good deal of attention from statisticians in recent years.

An introductory paper by R. A. Lyttleton provides background astronomical information, and discusses how the perturbation energy can be calculated as a function of the perturbing planet (taken to be Jupiter). Further details of the calculation are given in an accompanying paper by R. H. Kerr. Lyttleton is also concerned with the distribution of the perihelia of comets on the celestial sphere. He draws attention to evidence that these are not randomly distributed; their mean position is close to the apex of the sun's motion, and he interprets this as strongly favouring his accretion hypothesis.

J. M. Hammersley's paper provides a comprehensive treatment of the distribution of the lifetimes of comets, after pointing out the analogy with first passage times in random walk theory. He derives an explicit analytical formula for the case of a Brownian path, but for the more relevant discrete walk he obtains only an asymptotic relation, and it is necessary to resort to Monte Carlo methods for numerical estimation of the distribution. Hammersley points out the unusual character of the problem since results for Brownian paths cannot be regarded as limiting cases of the results for discrete walks.

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D. G. Kendall contributes three papers, the first of which is concerned with a survey and analysis of experimental data on the energy perturbations of comets. The other papers consider the energy spectrum of comets present in the solar system at a given time, assuming that statistical equilibrium has been reached. One paper is restricted to the particular choice of a "double exponential" distribution of perturbations which proves convenient for explicit calculation. The remaining paper derives results for a general distribution. Kendall concludes that experimental observations on the energy spectrum clearly favour the mechanism proposed by Oort.

Although the reviewer found this whole discussion stimulating and instructive, he was disturbed by two features which he feels might be modified for future symposia. It is disconcerting to find papers on the same topic scattered randomly through the book; they should be grouped together in some systematic order. Also, despite frequent reference to the alternative theories of Lyttleton and Oort, there is no clear description anywhere of the essentials of these theories. A few pages devoted to a concise exposition would have made the discussion much more self-contained.

Another group of papers in the field of astronomy deals with statistical problems connected with galaxies: "Field galaxies, luminosity, red shifts, and abundance of types" by J. Neyman and E. L. Scott is concerned with observable distributions related to apparent magnitudes and red shifts, care being taken to distinguish between "space distributions" and "catalogue distributions". T. Page undertakes an analysis of data on masses and mass luminosity ratios of double galaxies, assuming that these describe closed circular orbits. "The expansion of clusters of galaxies" by J. L. Lovascich, N. U. Mayall, J. Newman and E. L. Scott devises statistical tests of the hypothesis that internal expansion is taking place amongst clusters of galaxies. When applied to available data the results are not very conclusive, although the authors are inclined to the view that the possibility of instability in some systems of galaxies merits serious consideration. R. Minowski tries to make an estimate from experimental data of the luminosity function of extra-galactic radio sources. This function is related to the space distribution of galaxies and therefore has cosmological implications; it might provide evidence which could discriminate between evolutionary and steady-state cosmological theories.

J. A. Crawford contributes a paper on "The motion of charged particles in a random magnetic field" which considers in more detail the stochastic implications of a mechanism proposed by Fermi for the production of cosmic rays. "A stochastic description of precipitation" by L. Le Cam attempts to construct a model to describe the structure of rainfall. The ideas used are quite elementary, and little detailed meteorology is employed; but it is hoped that the basic structure developed will remain useful for more sophisticated models.

The five remaining short papers are in the field of physics. S. M. Ulam discusses some simple models to throw light on one of the central problems of statistical mechanics—how a dynamical system with an arbitrary initial distribution of positions and velocities of the particles approaches the distribution corresponding to statistical equilibrium. The particular model employed is also relevant to the question whether a rocket in the presence of heavy gravitating bodies can extract energy by suitably choosing its orbit. In an accompanying paper J. M. Hammersley ingeniously obtains an explicit solution for a model similar to that of Ulam whose behaviour seems to be very different from that expected from a normal physical system. Most physicists will assume that this arises from the nature of the model; in any case reference should have been given to the well-known hard sphere Monte Carlo computations by Alder and Wainwright which are surely very relevant to this field.

W. B. Fretter raises statistical problems connected with the measurement of ionization tracks in a cloud chamber. L. Schwartz deals rigorously with topics in probability associated with the quantum mechanics of elementary particles. Finally, W. J. Youden discusses some of the problems arising in the establishment of physical standards. He points out that statistics have not played a very significant role in this field, and makes

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some practical suggestions regarding suitable design of experiments to minimize the probable error.

The quality of the publication is excellent, and the price, 56*s*. for a volume of 335 pages, is very reasonable by present-day standards. But the reviewer feels that the impact of the symposium on current research has been blunted by the excessive delay (nearly two years) in publication time. It should be possible to persuade contributors to have manuscripts available at the same time as they deliver their lectures and this would result in substantial time saving.

C. Domb

Vol. IV. Contributions to Biology and Problems of Medicine. xii, 413 p. 68*s*.

A large part of this volume is deliberately devoted to papers on carcinogenesis, contributed by Arley, Armitage and Doll, Blum, Bryan, Dunn, Tucker, and Waugh. Closely related to this theme are studies of the process of cell-division by Tobias, a two-dimensional stochastic growth process by Eden, and an empirical study of low-level radiation effects by Gentry and Parkhurst. It was the intention of the organizers of the symposium to bring together and compare various stochastic models of the induction of cancer and various pieces of observational data. The papers form a lucid summary of these topics, and indicate that work is going ahead; unfortunately, they also show that theory and observation have not yet met in a convincing way.

A second theme running through this part of the symposium is a physiological one, of the transport of various chemicals through the body and the reactions taking place in various organs, such as the thyroid, lungs and kidney. There are papers on this by Bellman and Jacquez and Kalaba, Berman, Dantzig and De Haven, and Sams and Martin. Another physiological paper by Greenhouse concerns stochastic processes in muscle contraction.

A third group of papers deals with "actuarial" problems such as survival times, death rates, and causes of ageing, with papers by Berger and Gold, Chian, Jones and Taylor.

In addition to these, there are several papers on miscellaneous biological applications and ecological problems, mainly done on an electronic computer. These show vividly how the stochastic model can lead to periodic epidemics. Chapman treats the problem of the method of exploiting a marine fish population to give the maximum yield. A paper by Mantel deals with the screening of new drugs; is it necessarily helpful to interpose a first stage of screening, which is easy to perform, but which may not be completely relevant to the final choice? Rasch proposes an ingenious new model for psychological testing and measurement. There are also three papers dealing with rather more theoretical problems, which nevertheless have applications in biological statistics. Berkson and Hodge define and study a minimax estimate of the logistic function. Dixon describes the statistical uses of large computers, with special reference to biological applications of regression, discriminant functions, and the analysis of variance. Geissler deals with the "Latin square as a repeated measurement design". In brief, these papers form a useful summary of work in progress in many fields, but few of them present any very final conclusions.

Cedric A. B. Smith

2. Elements of Mathematical Statistics. By J. F. Ractliffe. Oxford, University Press, 1962. 202 p. $8\frac{3}{4}$". 25*s*.

This is an elementary book and it is primarily intended for students preparing for the Diploma in Technology. Apart from the usual introductory material and the standard distributions, it contains chapters on estimation, quality control, the *t*-test, the *F*-test, analysis of variance, confidence limits of proportions, rank correlation, Chi-squared, regression and correlation. Each chapter is followed by problems, to which the answers

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are given and the book ends with some miscellaneous test papers and an appendix of extracts from the usual statistical tables. There is no index or list of suggestions for further reading.

The author has included many worked examples and numerical verifications of theorems. A special feature is the regular use of a table of random numbers to provide exercises for sample statistics. The interests of technology students have been catered for by considering several aspects of quality control. The chapter on rank correlation includes Kendall's τ—and the material has been used to arouse the student's interest in calculating critical values of the coefficient (tables for $n \leqq 10$ are in the appendix). The chapter on regression has the virtue of emphasizing the possibility of two lines and allows correlation to arise in a fairly natural way.

The level of mathematical difficulty varies rather widely. The normal distribution is obtained as the limit of a binomial with the aid of a double integral and a change of variable; yet the Poisson distribution is produced in such a rush that the beginner may not be able to follow. The first and second moments of the binomial and Poisson distributions are obtained by calculus. This requires a lightning introduction of probability generating functions which are then seen no more. These moments might have been obtained by more elementary methods.

The discussion of most topics is much too short. For example, Chapter 4 on Probability covers combinations, permutations and the rules for combining probabilities in five pages of fairly large print! This leaves no space for conditional probabilities or the connection between "equally likely" outcomes and frequencies of events. Significance and the t-test get nine pages, four of which are devoted to worked examples on the three most common problems. There is no mention of "one-sided" tests and indeed the table of "t" given does not contain enough levels for such tests to be made. A student might expect to find the general formulae for the tests involving the comparisons of means.

On matters of principle, there are several statements that need qualifying. On p. 72 we have "the variance of the sum or difference of two variates is the sum of their variances"—there is no mention of independence. On p. 79 (with reference to estimating σ^2) "to obtain an unbiased estimate, that is to say one that is just as likely to be too big as too small"—the reader is likely to think that he has a 50 per cent. chance of obtaining a random value above the population mean of even an unsymmetrical distribution. On p. 175 a new value of Y is predicted from a regression line, and a standard error found using the "t" distribution. Yet nothing is said about the form of the distribution of the variates involved.

It is a pity that the early part of the text uses the same symbol for both the sample and the population variance. Some of the examples are phrased in a peculiar way, for instance, "could $x = 2, 3, 4, 5, 5$ have been randomly drawn from a normal population with mean 2? The short answer is 'Yes' ".

No doubt there is still a fair demand for texts at this level. Unfortunately this one is too uneven in quality for it to be warmly recommended.

G. P. BEAUMONT

3. Introduction to Probability and Statistics. By Henry L. Alder and Edward B. Roessler. 2nd ed. San Francisco and London, W. H. Freeman, 1962. xii, 289 p. 9″. 32*s*.

The first edition of this book, published in 1960 at a price of 20*s*., was reviewed in this *Journal* last year (Series A, 1961, **124**, 97). The main difference in the new edition is the addition of two new chapters on the F-distribution and the analysis of variance between and within groups. The style of these two chapters is consistent with the remainder of the book, maintaining the pattern of algebraic manipulation of all formulae. The final

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chapter seems to be more than usually successful at laying a foundation for the student who may need to proceed on his own to one of the standard volumes on experimental design.

Minor textual alterations have improved the chapters reviewed previously.

D. J. NEWELL

4. Statistical Decision Theory. By L. Weiss. New York (etc.) and London, McGraw-Hill, 1961. viii, 195 p. 9″. 58*s*.

The author's aim in this book is to give a "reasonably complete discussion of decision theory at an intermediate mathematical level". The book is also intended to be self-contained so as to be suitable for those not acquainted with probability theory. The important principles of decision theory are well explained, and there is a liberal supply of exercises, so that the text would be suitable for course work.

Chapters 1 to 4 (60 pages) cover elementary probability theory through a discussion of elementary probability distributions, moment generating functions, etc., to the t, F and χ^2 distributions, and the central limit theorem. This is necessarily a condensed account, but the author has given intuitive explanations and illustrations, and has kept the mathematical level low, so that the text might be useful for some students of about B.Sc. General standard.

Chapter 5 begins decision theory proper, with a rather complicated geometric argument and a slightly different formulation from usual. The decision problem is carefully discussed through randomized decision rules, admissibility, etc., and it is stated (not proved) that Bayes's rules are admissible. The only interpretation put on prior probabilities is the subjective one, which is discounted, so that the minimax principle has to be invoked to obtain a unique solution. The Bayesian approach is viewed merely as a means to obtaining admissible decision rules. For the situation in which the observable random variables have a finite number of possible values, it is shown in detail how a Bayes solution can be obtained by linear programming. For situations not directly suitable for linear programming solution, approximate solution is suggested.

One chapter includes a derivation and illustration of the Wald sequential decision rule, and the last chapter includes a decision theory approach to estimation and hypothesis testing.

There are a good number of illustrative examples in the text, which are worked in such great numerical and algebraic detail that one tends to get rather lost. A list of references to proofs, applications and further reading would have been an improvement.

G. B. WETHERILL

5. Statistics in Psychological Research. By William S. Ray. New York, London, The Macmillan Co., 1962. xi, [2], 303 p. 9¼″. 45*s*.

This is an introduction to statistical methods for psychology students. The approach of the author is certainly rather unusual, and he takes care to emphasize certain fundamental aspects of the subject. However, it is not a book one can recommend, as it has many unsatisfactory features.

The first chapter, entitled "Language, Science and Statistics", discusses the logical structure of language, and suggests that the topics of statistics can profitably be classified as syntactics, semantics or pragmatics. Each chapter is subsequently classified in this way. Two chapters follow which revise elementary algebra and the summation sign. Apart from a chapter on the problem of scales and measurement, the next 130 pages (six chapters) are devoted to the calculation and meaning of descriptive statistics. The last seven chapters deal with sampling and inference.

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The first chapter is rather compressed and, while it contains some interesting ideas, anyone new to both the study of language and to statistics will find that there are several new technical terms to be mastered, and these do not seem to contribute to an understanding of statistical procedures. The more usual classification of the latter into "collection of data", "analysis" and "inference" seems more useful. In the following chapters far too much emphasis is laid on descriptive statistics. There is no suggestion that N should be "large", even in the examples and, as there is no sampling, this is of course correct, but the answers are of very limited value if no inference is to be made. Moments are discussed in detail, and it is suggested that electronic computers will be useful for computing moments higher than the third. The value of doing so is not clear, and the moments are not even compared with those of the normal distribution, which is not discussed until p. 210. On the other hand, there is no reference to the median and other quantiles, which are often useful in psychological statistics. Many of the examples are artificial, and this is particularly unfortunate in the following chapters on correlation and regression; it is difficult to believe that a student will appreciate the significance of the ideas involved.

Sampling is not mentioned until p. 210, and "probability" and "chance" are mentioned only three or four times in the whole book, although there are detailed sections on random sampling. While there may be some point in referring, for example, to "the relative frequency in the sampling distribution", one cannot help feeling that a student should be well acquainted with the use of the word "probability" as a technical term in order to understand the literature, and to know the meaning of $P < 0{\cdot}05$, etc., which is so frequently used in psychological journals. Another point of criticism is that standard errors are not mentioned explicitly, even in calculating confidence limits for a mean. The t-test for comparing two means is given only for the case where n is the same in both groups.

The best chapters in the book are those on experimental design. Certain designs are discussed in detail, and their analysis given. The basis of a good design is outlined and the difference between an experiment and empirical research is stressed. The final chapter is on "Frequency classifications", and is far too compressed for such an important topic. There is only one example of χ^2 (a 2×2 table) and no other non-parametric tests are mentioned. This is an important omission in a field where measurement is so frequently impossible or unsatisfactory.

There appear to be very few errors in the book, but the diagrams to show distributions with varying degrees of freedom are quite inadequate as the scales are omitted. The book may be of use to students already acquainted with the basic techniques who wish to revise them in another context, but there are better books covering similar ground.

MONICA A. WALKER

6. Use and Abuse of Statistics. By W. J. Reichmann. London, Methuen, 1961. 536 p. $8\frac{1}{4}''$. 22*s*. 6*d*.

As the use of statistics becomes more widespread, there is increasing scope for books which introduce the subject of statistics to the non-specialist reader and help him to distinguish between correct and incorrect use of figures. From the title of this book one expects the author to approach the subject simply through practical examples of right and wrong application of statistics. Although there are many and varied examples of this kind, the book also includes some background of statistical method, and explains the principles of some statistical techniques without going into the detail of their application. For example, one chapter is devoted to probability, and another describes the normal, binomial and Poisson distributions. The book covers the subjects included in most elementary textbooks on statistics: the presentation of statistics in graphs, diagrams and tables, and the importance of definition; the simple descriptive statistics: proportions, percentages and averages; correlation; time series; index numbers; simple probability

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and sampling; questionnaire design; quality control and acceptance sampling; together with a brief description of linear programming and games theory.

The author writes in an informal style which is very easy to read. However, I found it at times rather irritating (e.g. "it is not a function of statistics to attempt the impossible"). The text assumes no special prior knowledge and no particular aptitude for mathematics, so that the book is suitable for a non-mathematical reader. The minimum of mathematics is included in the text, some points being elaborated in a series of appendices. The arrangement of the book is not particularly logical and the text often wanders from one topic to another. Averages are dealt with in Chapter 5, before the importance of definition is discussed (Chapter 8) and before examples of meaningless statements are considered (Chapter 11). An example of spurious accuracy is given in Chapter 7, but the subject is not discussed specifically until two chapters later. The apparent lack of order as compared with most textbooks may be no disadvantage for the non-specialist reader whose main requirement is a book which will retain his interest as he reads it. The book will generally fulfil this requirement, although it will not afterwards provide him with a good source of reference.

The major disadvantage of the book is that the explanations are not always clear. To be successful in explaining a subject which is often found difficult and in which apparently small differences in meaning can be quite important, there must be no confusion. An important example occurs in the chapter on probability, which is usually a difficult topic. In considering the chances of predicting successfully the outcome of a series of independent races (p. 190) the author does not point out that the chance of *at least one* success does increase progressively as the number of races increases, although the chance of success with any one race remains the same. Nor does he distinguish clearly between mutually exclusive outcomes of one trial and dependence, in general, in a series of trials (p. 191). Possible errors from projecting time series are illustrated by an example (p. 171) which shows the result of an incorrect assumption about an incompletely specified time interval rather than an incorrect assumption of continuation of a trend. When discussing stratified sampling (p. 242) no clear distinction is drawn between equal chances of selection in the sample for all groups and chances differing from group to group. There are also a few confusing misprints.

The author has attempted the difficult task of writing an interesting and readable text for the beginner and the non-specialist reader. On the whole he has been successful. Unfortunately, however, his statements cannot always be taken to mean precisely what they appear to say and the reader may well be left with some confused ideas.

RITA J. MAURICE

7. Bibliography of Nonparametric Statistics. By I. Richard Savage. Cambridge, Mass., U.S.A., Harvard University Press; London, Oxford University Press, 1962. vi, 284 p. $8\frac{1}{4}''$. 54*s*.

This is a revised version, brought up to April 1961, of the bibliography published in the *Journal of the American Statistical Association* in 1953. It is unusual in giving abstracts, reviews and citations of each publication listed, with occasional brief explanatory notes in addition. This is a new departure, in statistics at least, and makes the bibliography especially valuable for teachers and for applied statisticians looking for detailed applications of methods, and for research workers following up a particular line of development. A detailed introduction (about 20 pages) describes the scope and method of compilation of the bibliography. Multiple-author entries are separately cross-referenced. The original bibliography's broad classification of references into 17 classes is retained, with some discussion of its limitations.

ALAN STUART

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8. A Course in the Geometry of *n* Dimensions. By M. G. Kendall. London, Griffin, 1961. viii, 63 p. 8½″. 16*s*. (Griffin's Statistical Monographs and Courses, 8. General Editor, M. G. Kendall.)

The inclusion of a book on *n*-dimensional geometry in a statistical series of monographs hardly needs justification. Since 1915, when the device of representing a set of numerical observations by a point (or points) in a multidimensional Euclidean space (or spaces) was introduced by Sir Ronald Fisher, in his derivation of the sample correlation coefficient in normal variation, geometrical methods have amply demonstrated their simplicity, efficacy and elegance in solving distributional problems, and have in addition thrown a powerful light on such varied and fundamental topics as regression and analysis of variance, the general theory of testing statistical hypotheses and the classification of populations. In more recent years, geometrical methods have perhaps not been used by theoretical statisticians as frequently as heretofore, though some suggestive applications of differential geometry to problems of estimation (B. R. Rao's study of the likelihood surface when sufficient statistics exist) and to the further development of the theory of population classification (C. R. Rao's construction of invariant Riemannian metrics) indicate that such methods have by no means exhausted their potentialities, and it is to be hoped that the present work will serve to reawaken interest and to stimulate further research.

Dr Kendall's slim volume divides into two unequal parts in the ratio 2 : 1. The first and longer part gives an elementary account of some basic notions in *n*-dimensional Euclidean geometry, the main topics included being varieties (hypersurfaces), linear subspaces and the angles between such spaces, the projection of the angle between two lines onto linear subspaces, solid angles, quadrics and their reduction to canonical form, and mensuration (the contents of spheres, ellipsoids, prisms, parallelotopes, pyramids and simplices); a few simple statistical applications are also included. The second and statistical part may itself be divided into two portions. In the first of these, the previous geometrical notions are applied to the derivation of the frequency function of χ^2 as a sum of squares (probability mass of a spherical shell), the frequency function of t as the studentized mean-statistic (content of a spherical annulus), the frequency function of the mean of rectangular variables (the volume of the region demarcated by two adjoining flats orthogonal to the equiangular line and the coordinate flats) and the joint frequency function of variances and covariances in samples from bivariate and multivariate normal populations, while the second portion discusses variances and correlations, including multiple correlations, from the geometrical point of view as lengths and angles, derives the null distribution of the multiple correlation coefficient in normal samples, and, finally, in two very brief sections, highlights the formal equivalence of canonical correlations to the angles between linear spaces and of component analysis to the representation of quadric surfaces in canonical form.

The book represents the published lecture notes of a course given by the author at the London School of Economics in 1960. It will undoubtedly make it much easier for lecturers elsewhere to prepare their own notes for similar courses. Such courses should have beneficial consequences. It must be admitted that it is in general not easy to tempt capable young mathematicians into statistics, partly because of competition from superficially more glamorous subjects and partly because a real interest in statistics as the art and science of obtaining and interpreting data goes hand in hand with a corresponding interest in general scientific methodology and experimentation, and this is not often to be found in young men in their early twenties. One way of overcoming this obstacle is through probability theory (used here in the narrow sense of a mathematical discipline) with its irresistible charm and virtually limitless applications, but another route may well lie along *n*-dimensional geometry. In conjunction with a course on the latter subject, the book will be of genuine value to a student. Whether it could be used for self-study without

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external supervision and help is more problematical. My guess is that the condensed presentation (especially in the statistical part) will make the going rather hard for all but the exceptionally gifted student.

The text contains one or two accidental mis-statements of fact as well as a fairly large number of misprints, some of which may perhaps be noted here. In Section 51, the numerator and denominator in the non-circular serial correlation coefficient for normal variation are incorrectly stated to be independent, the independence being in fact between the coefficient and the denominator; correspondingly, the references on pp. 45 and 46 to Example 5 should be to Example 7, and the reference, on p. 46, to Section 49 should be to Section 50. The determinantal equation (173) is meaningless, since the matrix is non-square. (The subscripts for η also require emendation.) In the derivation of equation (175), the minor of the leading element of the determinant (174) gives the *square* of the content of the parallelotope determined by the sample vectors. The inequality in line 9 of p. 6 should be reversed; N_0 after equation (4) is to be replaced by N_n; r, in the fourth line after equation (29), is to be replaced by f_2; R^2, in equation (71) and in the middle of p. 25, is to be replaced by R; in Section 44, a factor $n-1$ is missing in the denominator of the expression for the content of a hyperspherical cylinder; in Section 65, for $p<q$ read $p \leqq q$ (or, preferably, $q \leqq p$ with (ξ_p, η_p) replaced by (ξ_q, η_q)).

Dr Kendall has rendered yet another service to statistics by publishing this book. One hopes that it will run into more than one edition. One hopes further that the next edition will be considerably expanded in order to include more material, and also so that the uninitiated reader can be taken along at a less forced and more leisurely and deliberate pace.

HAROLD RUBEN

9. Combinatorial Chance. By F. N. David and D. E. Barton. London, Griffin, 1962. x, 356 p. 9″. 63*s*.

In their preface the authors state that they are "interested in combinatorial chance for its own sake" and that they offer "a delineation of such combinatorial methods as have been found useful by the statistician". In the chapters that follow, this interest is communicated to the reader and the "delineation" is both comprehensive and clear. The first chapter is particularly helpful in achieving this clarity. This chapter is devoted to a number of simple examples illustrating the topics and methods to be discussed in the rest of the book. One wishes that more authors would lead the reader into their subjects by first giving him some basic comprehension and insight in this way.

The contents of the book fall into three main categories. A number of chapters are devoted to basic distribution theory with particular attention to cumulants, multivariate moments, converse theorems and limiting distributions. The combinatorial theory surveys the theory of runs, including multiple runs in line and ring and runs of order, derangement and matching, ranking tests, occupancy and distributions arising in combinatorial extreme-value problems and non-central situations. In discussing these topics the authors give some emphasis to the historical origins and relative antiquity of their subject whilst, at the same time, they bring together much of their own recent work on the subject. Finally three chapters are devoted to a number of more mathematical topics arising in the combinatorial theory. These deal with generalized Bernoulli numbers, approximations to differences of zero, symmetric functions and polykays.

For those wishing to use the book for reference the otherwise clear, and nearly self-contained, presentation of each topic is marred by the lack of any method of cross-reference. Neither sections nor equations are numbered and much time has to be spent searching back to find where equations or methods were first introduced.

In spite of the mention of applications on the front dust cover, and the use of "applicability" to decide on the contents, the authors have deliberately refrained from mentioning the applications of their subject. These they say will be included in the introduction to a

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forthcoming book, edited by them and Dr M. G. Kendall, called "Symmetric Function and Allied Tables". But surely the natural place for comment on the applications is immediately after the results have been obtained, not in the introduction to a book of tables. This separation of the basic theory from the applications leads to the subject being discussed in terms of rows of coloured balls, packs of cards, etc., and also to the relevant distributions and moments being derived and then left without much further comment or illustration. Are not distribution functions and moments, like words, only fully appreciated when they are seen and used in various contexts? Nevertheless, leaving this debatable criticism aside, this is a book to be recommended not only to those particularly interested in its topic, but to anyone who enjoys a clear presentation of some very elegant methods.

W. G. GILCHRIST

10. The Algebra of Probable Inference. By Richard T. Cox. Baltimore, Johns Hopkins Press; London, Oxford University Press, [1961]. xii, 114 p. 9″. 40*s*.

This is a short book or rather a long essay on the nature of probability, written by a physicist in a clear and simple style. It has points of mathematical as well as philosophical interest and some original ideas.

In the first section, entitled "Probability", the author expresses the opinion that all probable inference, whether commonplace or technical, is governed by the same rules. If it is defined numerically, then it differs from other quantitative characteristics such as length, for example, only in the precision with which it may be measured and the author conjectures that it may only be in cases of "equally likely outcomes" that we are able to estimate probabilities at all precisely; otherwise we are limited to approximations and inequalities. It seems that the author intends probabilities to be objective, i.e. independent of the individual who makes the inference, but it is not clear how one would reconcile two individuals who may differ, say, about a particular inequality between two probabilities.

The author takes the view that in whatever way probability may be measured it should satisfy two axioms:

(1) "The probability of an inference on given evidence determines the probability of its contradictory on the same evidence."

(2) "The probability on given evidence that both of two inferences are true is determined by their separate probabilities, one on the given evidence, the other on this evidence with the additional assumption that the first inference is true."

Then, after a short, self-contained exposition of the Boolean algebra of propositions, he introduces the symbol $i|h$ to denote the probability of the inference i given the hypothesis and/or evidence h. Thus, probability is defined as a function of each ordered pair (i, h) of propositions in the Boolean algebra. If probability is taken as a *numerical* function satisfying the above two axioms, the author shows, assuming some conditions about differentiability, that the usual laws of probability are consequences of these axioms. This is a concise and interesting derivation and it may help, for example, in providing a more economical set of axioms for Sir Harold Jeffrey's theory.

The second section, entitled "Entropy", is devoted to a discussion of entropy as a concept in probability theory. The author broadens the scope of the Shannon measure $-\Sigma(a_i|h)\log(a_i|h)$ for a set of exclusive and exhaustive propositions a_i on a common hypothesis h by introducing the idea of a *system* (or *inductive* set) of propositions. This is an exhaustive set of propositions, on a common hypothesis, such that any proposition which *implies* a proposition of the set is itself a member of the set. (This is in contrast to a *deductive* set which contains propositions which are *implied by* members of the set.) The author defines the entropy of a system and derives several properties of entropy so defined, concluding the section with a short discussion of the notion of chance.

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The third and last section, entitled "Expectation", contains a discussion of Laplace's rule of succession. The author points out the validity of this rule provided that successive instances are mutually independent and concludes with some remarks on induction.

The book is recommended to philosophically minded probabilists who will have to be philosophical about the price, which is high for a book of this size.

H. D. MILLER

11. Tables of Neyman-Shortest Unbiased Confidence Intervals (a) for the Binomial Parameter, (b) for the Poisson Parameter. By Colin R. Blyth and David W. Hutchinson. From *Biometrika* (1960), pp. 381–391. $10\frac{3}{4}''$ 2s. 6d. (New Statistical Tables, Series No. 30.)

The standard tables of confidence intervals for the p-parameter of the Binomial $B(n,p)$ (Clopper and Pearson) and for the Poisson parameter λ (Garwood) as given for instance in the Biometrika Tables for Statisticians I (Tables 41 and 40 respectively) have the inherent limitation that the end points of the intervals are functions of a discrete random variable. (Let us call this r.) The important consequence of this is that it is only exceptionally possible for an interval to have a prescribed confidence coefficient, $1-\alpha$ (e.g. 95 per cent.). Thus Tables 40 and 41 have a nominal confidence coefficient which is in fact only a lower bound and the true coefficient in any given instance may be considerably more than the nominal one. To take an extreme example, for a 10 per cent. nominal level of α (when $n = 10$ in the binomial case) the actual level can be as low as $\frac{1}{2}$ per cent. This in turn makes it difficult to set up reasonable criteria for discriminating between the different sets of intervals which may be obtained by varying the proportion of the confidence coefficient contributed by the upper and lower limits (as was done by Crow, for instance, whose intervals were, on the whole, not so wide as those of Table 41 and which had a shade less variable confidence coefficient). They are also difficult to talk about since they are not confidence intervals at all in the strict usage as laid down by Neyman (1937) in his classic paper and in discussing them all terms have a "strict" and a "wide" meaning and theorems proved for strict sense intervals need, at least, verifying before they can be applied to wide sense ones.

If a random variable, u say, uniformly distributed in $(0, 1)$ is chosen from a suitable table of random numbers and added to r this gives a continuous random variable, z say, which is continuously distributed (though its density function is not continuous). By plotting the upper and lower $\alpha/2$ "percentage points" of z as against p (in the Binomial case) confidence intervals with uniform confidence coefficient $1-\alpha$ may be read off. The present tables give such confidence intervals, as functions of z, obtained arithmetically from the solution of the equations corresponding to an analogous reading-off process with unequal tail probabilities, adding to α (the corresponding percentage points being those of the unbiased test of $\mathscr{E}(z) = np + \frac{1}{2}$). The same is true for the tables of intervals for the Poisson parameter.

The history of this device, of adding an auxiliary random variable u, and such theoretical justification as it has, is an involved one. Eudey (1949) gave it, in what must be considered an unpublished report, wherein he outlined a proof that it gives unbiased intervals which are "Neyman-shortest" (i.e. what Kendall more felicitously calls most selective). This was relative to intervals based on z (not r, contrary to the statement of Blyth and Hutchinson) and is cast in terms of tests of significance; the relevance to confidence intervals following from the equivalence between confidence regions and acceptance regions. The distinction between *region* and *interval*, which elsewhere muddies the discussion of confidence intervals in terms of tests, does not matter here but the restriction to strict sense confidence intervals does seem *prima facie* to be important and the extension of Eudey's results to wide sense confidence intervals would seem to need a little more care than is given by Blyth and Hutchinson (who do this without comment). Simultaneously with Eudey's report Tocher (1950) showed that, relative to a wider class of auxiliary

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random modification, the operational equivalent of Eudey's device gave slightly weaker optimum properties relative to the class of wide sense confidence intervals whilst Stevens (1950) gave the first published proposal of the device in respect of confidence intervals for the Binomial parameter. In these circumstances it is a pity that Blyth and Hutchinson did not rehearse the proofs of the optimality of their intervals *de novo*, or at least refer the reader to the illuminating treatment of this problem given by Lehmann (1959) in his excellent book. Reference to this latter is particularly useful since the absolute desirability of unbiasedness and most selectiveness is by no means obvious. One might well prefer, for instance, a slightly biased interval if it more frequently excluded values very different from the true one, and this indicates a loss function such as Lehmann discusses in his Decision–Theoretic approach.

The wider question of the merits of adding an auxiliary random variable is of considerable interest, particularly in view of Durbin's recent exciting discoveries which have opened up a very wide field for the application of this technique. Statisticians who randomize their experimental designs without hesitation are strangely reluctant to use this form of *post facto* randomization. The commonest excuse is that it is undesirable that two statisticians should be able to come to different conclusions on the same data for the sake of an exact level of significance or confidence coefficient (which are themselves arbitrary things). But two statisticians very often will apply different tests or methods of analysis and since the "different conclusion" arising from use of auxiliary randomization will only occur in cases of borderline significance this objection equally makes a fetish of a particular level. A deeper reason must be the suspicion that additional randomization throws away "information" in some sense. There may be a degree of truth in this suspicion, although we can, in the Binomial case, for example, always recapture r from z (by taking the integer part) and although the point estimator $(z-\frac{1}{2})/n$ is unbiased and loses only $1/12pq$ units of Fisherian Information (whilst never giving the ridiculous estimates 0 or 1). Only an exhaustive numerical comparison between the consequences of using these tables and the Clopper–Pearson ones will finally settle the matter in this instance; and this has yet to be made.

D. E. BARTON

For details of references in this review see p. 663.

12. Numerical Methods for Scientists and Engineers. By R. W. Hamming. New York, London (etc.), McGraw-Hill, 1962. xvii, 410 p. 9″. 85*s*. 6*d*.

With the widely increasing use of electronic computers for numerical calculations it is refreshing and valuable to find a book which provides a coherent, unified and very readable account of some methods of numerical analysis, with particular reference to the use of these methods on computers.

This book is described in the preface as "written for scientists and engineers who expect to use modern large-scale digital computers . . . and as a text for advanced undergraduate-level courses in numerical analysis". It is therefore natural to consider to what extent it fulfils this dual role.

The book is divided into four parts. *Part I. The Discrete Finite Difference Calculus* (Chapters 1–6) is introductory and discusses difference operators and tables, summation calculus applied to finite and infinite series, linear finite difference equations with constant coefficients and finite Fourier series. An interesting application of these methods is to the generation of round-off errors. Where possible ideas are illustrated by analogy with comparable results in differential calculus. *Part II. Polynomial Approximation—Classical Numerical Analysis* (Chapters 7–20) is the most substantial part of the book and develops, by means of a very general approach, all the familiar methods of interpolation, indefinite and definite integration and the solution of differential equations—on the assumption that between the sample points the function under consideration may be approximated by a polynomial. For most of this section the approximating polynomial is chosen by the process of "exact matching" at the sample points, but some consideration is given

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to the principle of "least squares" (and the use of orthogonal polynomials) and there is a chapter on Chebyshev polynomials as an example of "minimax" methods. The treatment of "least squares" is somewhat imprecise. *Part III. Nonpolynomial Approximation* (Chapters 21–27) is concerned with more recent developments and replaces the approximating polynomial by Fourier functions with frequencies within a fixed band—so-called "band limited functions". These have the advantage that errors are expressed in terms of the function itself rather than some elusive high order derivative. Methods are developed for the evaluation of integrals and the solution of differential equations. There is also a brief study of linear filters and differentiation. *Part IV. Algorithms and Heuristics* (Chapters 28–32 and $N+1$) gives a brief and interesting, but hardly practical, account of the solution of "implicit" systems. There are short chapters on finding zeros, linear equations, inversion of matrices, simulation, random numbers, Monte Carlo methods; and the book concludes with some general remarks on the "art of computing".

This is a very well written book and the reader is kept constantly aware of the aims and objects of the subject and of the inherent problems involved in applying numerical methods on computers. Due attention is given to the requirements of stability and the control and estimation of truncation and round-off errors. The emphasis is on the derivation of general methods rather than specific formulae, although all the standard interpolation and integration procedures are discussed as particular cases. Unfortunately this leads, even at the relatively unsophisticated (but adequate) mathematical level of this book, to a very lengthy treatment of the various topics; and the author not only has to ignore or skim over certain sections of the subject, but also to restrict the number of examples and exercises, which would be so valuable with such a general approach to the subject. The many footnote references to the various journals and textual references to the comprehensive list of publications at the back of the book are welcome, but hardly compensate for the complete lack of mention of functions of more than one variable (in particular, the associated topic of partial differential equations) and the inadequacy of the single short chapters on linear equations, matrix inversion, eigenvalues and zeros of functions. Whilst this is understandable as a means of keeping this already sizeable book to a reasonable length, the author's justification of this restriction is both dubious and dangerous. To say that such methods should be amply covered by library routines provided by the computer centres both ignores the fact that very few people have access to reasonable library facilities and contradicts the alleged aim of the book which is to teach people not to rely on specific formulae for all situations but to examine the real requirements of each problem as it arises. As we are constantly reminded "the purpose of computing is insight—not numbers".

In view of the limitations of this book with respect to content (both topics covered, and the scanty attention to working detail or provision of tables) and the general nature of the approach to the subject, one must conclude that its main function is to instruct rather than to serve as a work of reference. The author is to be congratulated, however, on a very fine "advanced-undergraduate" textbook, providing a clear and attractive account of the topics covered in relation to their use on computers. One wonders, nevertheless, how many students will be able to afford this elegant and attractive book.

V. D. Barnett

13. Coding Theorems of Information Theory. By J. Wolfowitz. Berlin (etc.), Springer-Verlag, 1961. ix, 125 p. $9\frac{1}{4}''$. *DM*.22.

When a "square" voltage pulse is passed through a long wire, it becomes distorted and needs to be "squared up" or *regenerated* if it is to remain distinguishable from mere "noise". This process of regeneration can be interpreted in a very general sense, and is then seen to be fundamental for every communication system. For example, each member of some class of (short) time series, forming a clump in the space of all possible time series, will be interpretable in a hearer's mind by the same "word"; in fact a word can be

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defined as the class. In some contexts this process of regeneration is known as *error correction*. Likewise the *failure* to identify a sound or a word is an example of error detection. Another example of error detection is the use of parity checks in binary computers. If each "word" in a computer has several distinct parity checks, these checks can be used for error correction. The book under review is entirely concerned with existential theorems, i.e. with existence and non-existence theorems, concerning error-correction codes, and is not concerned with the constructive theory of codes.

A *communication system* between two points consists of a succession of transformations of physical time series. There might, for example, be four time series and three transformations, the time series being a *source* $S = S(t)$, an *encoded source* $\mathcal{T}_0 S$ (where $\mathcal{T}_0$ is a transformation), a received encoded signal $\mathcal{T}_1 \mathcal{T}_0 S$, and a decoded received signal $R = \mathcal{T}_2 \mathcal{T}_1 \mathcal{T}_0 S$. It is often desirable that R should resemble S in some sense, but with an inevitable time delay. Let us suppose that $\mathcal{T}_1$ is statistically fully specified and that its mathematical description is identified with the *communication channel*. A class of coding theorems of information theory is concerned with a part of the design of the *encoding box*, or more precisely with the choice of the transformation $\mathcal{T}_0$. The decoding transformation $\mathcal{T}_2$ will then usually consist of a regeneration of the output from $\mathcal{T}_0$ (by operating on the output of the *channel*), followed roughly speaking by the inverse of $\mathcal{T}_0$. The transformation $\mathcal{T}_0$ is often assumed to be many–one and deterministic, and its choice should depend both on the statistical properties of the source S and also on $\mathcal{T}_1$. The coding box is efficient if it enables us to pump a large amount of information through the channel with few errors, and at little expense, but the original Shannon theory was hardly concerned with the expense of the coding and decoding boxes, and nor is Wolfowitz in the present book. It is a rigorous up-to-date account of an important if rather specialized field, and every University library should have a copy, together with the books by Khinchin and by Feinstein. The student of existential coding theorems should also consult B. McMillan, *The SIAM Review*, Vol. 3 (1961), 211–229.

Most of the book is concerned with the case where $\mathcal{T}_1$ is a transformation of a discrete time series of "letters" of a finite "alphabet" into another such series. In most treatments of the subject, a measure of information is first introduced; then the capacity C of a channel is defined as the least upper bound of the information rates of inputs to the channel, when these inputs are stationary, or perhaps ergodic, and where the error rate is small. It is then usually proved that any source of information rate less than C can be encoded ($\mathcal{T}_0$ can be chosen) so as to be received with arbitrarily small error rate; and that this cannot be done if the information rate exceeds C. A feature of the present book is that the capacity is defined more constructively, but less intuitively, in terms of $\mathcal{T}_1$. Also, up to a point, the theorems are stated without reference to the stochastic properties of the source, and even the usual measure of information is not verbally defined. The part of the theory dealing with the communication channel is, as it were, split off from the part dealing with the coding box. The possibility of doing this is clarified by the following extract from the "heuristic introduction to the discrete memoryless channel": "To summarize then, the problem is to choose N n-sequences in such a way that, whichever of the N n-sequences is sent, the receiver can correctly infer which one has been sent with probability $\geqq 1-\lambda$. The solution is, roughly, to choose n sufficiently large so that the N n-sequences can be embedded in the space of all a^n n-sequences with enough distance between any two that, when any one of the N n-sequences is sent, the set of all n-sequences nearer to it than any of the other $N-1$ n-sequences, has probability $\geqq 1-\lambda$ of being received."

One reason why there are many different existential coding theorems is that there are many kinds of transformation $\mathcal{T}_1$. In fact the titles of Chapters 3 to 9 are in accordance with a classification of some of these transformations: for example, "discrete finite-memory channels", "discrete channels with a past history", "general discrete channels", "continuous channels with additive Gaussian noise". The author does not claim exhaustiveness even within the field of existential coding theorems. An example of a channel

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which the reviewer has believed for some years to be important, although it does not seem to have been treated yet in published literature, is as follows. Suppose that each mark or space of a teleprinter code is transformed into a *probability*, p, of a mark, with probability density $\phi(p)$ ("probabilistic reception"). In the application of coding theory to teleprinters and to computers it is commonly assumed that each mark is regenerated, i.e. each probability p is rounded off to 0 or to 1. Nearly always, the marks and spaces are regenerated first, then possibly groups of marks and spaces (such as teleprinter letters) might be regenerated by means of an error-correcting code, and finally a human might regenerate larger groups, such as words. It would certainly be possible for a machine to regenerate the marks and spaces and letters *jointly*, with considerable gain in some applications. Given a very large decoding box, it would even be possible for marks, spaces, letters, and words to be automatically regenerated jointly.

The author says in the Preface that the purpose of the book is "to provide, for mathematicians of some maturity, an easy introduction to the ideas and principal known theorems of a certain body of coding theory".

As mathematical books go, this one is rather clearly written, but it would be greatly improved if it had a list of frequently used symbols, since most readers of mathematical books read "forwards, backwards, anagrammatized" and not straight through. Another minor criticism is that the new abbreviations are not indexed; in fact there is no index. The style is forthright, logical and concise.

I. J. Good

14. Transmission of Information: A Statistical Theory of Communications. By Robert M. Fano. Published jointly by the Massachusetts Institute of Technology Press, New York, and John Wiley, London, 1961. x, 389 p. 9″. 60*s*.

This book is based on a series of lectures given in the Electrical Engineering Department of the Massachusetts Institute of Technology and is addressed primarily to electrical engineers. However, there are very few purely electrical terms used and the book would seem to present the clearest account to date of the Shannon theory of information as applied to technical problems of communication. Considerable emphasis is placed on practical problems of encoding and decoding and it is perhaps this emphasis which enables the real content of the central theorems of information theory to be properly appreciated. At the end of the book exercises are given for each chapter and these serve very well to help fix the content of the theory covered. Only one complaint might be made, that it would help if solutions and perhaps hints in connection with these problems were added. The chapter headings are as follows: 1. The transmission of information; 2. A measure of information; 3. Simple message ensembles; 4. Discrete stochastic sources; 5. Transmission channels; 6. Channel encoding and decoding; 7. Encoding for binary symmetric channels; 8. Multinominal distributions; 9. Encoding for discrete, constant channels.

G. A. Barnard

15. Programmation linéaire. By M. Simonnard. Paris, Dunod, 1962. xx, 419 p. 9½″. 68 *NF*.

This book by a French and American trained author shows how well the rigours of a training at the École Polytechnique serve when one is exposed to the knowledge about linear programming which is nowhere as abundant as in the U.S.A.

M. Simonnard has concentrated on the algebraic aspects of his subject, and in particular on those which support the most useful methods of solution, such as Dantzig's Simplex Method and its variants. They are the algebra of linear inequalities, and the fundamental

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concept of duality. Parametric and integer programming are introduced, and for the latter the methods of solution due to Gomory are explained. To my knowledge this is the first time that an explanation of Gomory's method for the mixed case (that when only specified variables are required to take integer values) appears in a book. This is also true of the Decomposition principle of Dantzig and Wolfe, designed to deal with very large matrices.

The last three chapters deal with various practical solutions of the Transportation Problem, introducing graph theory where convenient. Three appendices are headed respectively: "Survey of linear algebra", "Elementary concepts concerning convex polynomials" and "Definitions regarding graphs". This is a somewhat inconvenient feature, because the main text requires frequent reference to these appendices if one wants to grasp the mathematical essentials.

A list of programmes available for electronic computation of solutions to linear programming problems is given on pp. 133–138. They refer to the French market.

As regards linear programming, Professor Fortet is right when he says in his Introduction that it has probably found its final form as a theoretical discipline. Non-linear programming is deliberately left out of this book (except for a brief reference on pp. 200–202 to cases which can be linearized), since it was considered—I think erroneously—to be as yet undeveloped. Stochastic programming is also ignored, because "its results are, at present, rather fragmentary"—which is a good reason. Game theory is outside the scope of the book.

As one might expect in a book whose orientation is mainly mathematical, the economic and industrial contexts, though mentioned, are not deeply evaluated.

A bibliography contains 169 well-chosen titles.

I have no hesitation in affirming that this book is a valuable addition to the heretofore sparse French literature on linear programming.

S. VAJDA

16. Operations Research/Management Science. Edited by Robert S. Titchen and Arnold J. Rosenthal. New York, London, Interscience Publisher, John Wiley, 1962. 63 p. 9½″. $75 p.a. (International Literature Digest Service, Vol. 2, Issue 1.)

There now exist so many services supplying abstracts, digests or summaries of papers in statistics, operational research, and related fields that it seems appropriate to subject one such collection to a review of its own. The service here reviewed surveys about 30 journals (including, of course, the Royal Statistical Society's A and B series and *Applied Statistics*), but the list given does not include all those actually used. "Probability and statistics abstracts are included only to the extent that the original papers are directed specifically to Operations Research situations." Otherwise one is referred to a companion series "Quality Control and Applied Statistics". The primary classification "Methodology and Special Models" consists of nine topics, such as optimal allocation, queueing, etc. They are subdivided according to "functional area of application" and the editors point out that the filing can be done in different ways. The explanation of how to do this underestimates the intelligence of a junior filing clerk, let alone of a research scientist.

The distinctive mark of this service is the supply of reviews on loose leaves, printed on both sides, each review containing the reference code and the following headings: title, author, journal, purpose, summary, results, references, name of abstracter, though not always all of these. No colour scheme is used.

This particular issue extends to 63 pages (though the title page mentions, misleadingly, 64) and a list of contents of altogether 23 reviews. Curiously, this list contains what is called the "subject" of the paper, which is a paraphrase of its title. There is no index of names of authors, but a subject index will be provided after the completion of each volume.

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The abstracts are competent in all cases which your reviewer has studied in detail. There are reasons for preferring any particular type of such collections: more or less extensive summaries, more or less critical comments, on cardboard or on paper, loose-leaf or book, etc. It remains to be seen whether so many of them, conceivably serving different purposes, but appearing simultaneously, will turn out to be viable.

S. VAJDA

17. A Guide to Operational Research. By W. E. Duckworth. London, Methuen, [1962]. 145 p. 8½″. 20*s*.

This guide is written in a brisk, attractive style and might well appeal to managers, to whom it is addressed. It is also stimulating reading for others who want to know what operational research is about.

Its four parts are headed: What O.R. is; O.R. Techniques; The Organisation of O.R.; The Use of O.R. The second is by far the longest, containing sections on statistics, linear programming, queueing and a number of other techniques. The section on operational gaming is particularly intriguing. Examples are given that sound realistic and the case for the usefulness of O.R. is aptly presented. Two appendices deal with rapid statistical tests and with business games respectively.

While it would be churlish to apply too critical a standard to a book of this type, more care in writing would have made it more valuable. References to the literature are given after each chapter but the use made of them is disappointing. Thus on page 28 the reference to "Morton, Kay and Duckworth" should presumably be to Morton and Kay, and to Kay and Duckworth. The paper by Robinson, mentioned on page 38 and again on page 39, is in fact by Robinson and Duckworth. The name Kendall is misspelt on page 112. Also surprising is the reference to research by Gould (p. 34), the only work mentioned on the rapidly expanding field of non-linear programming. In fact, Gould's paper deals only with a transportation problem.

On page 53 there is a risk of the reader's confusing the mean and the median and the remark on pages 91–92 that "there is a mixture of strategies to which the game converges" is either meaningless or incorrect. Such phrases as that on page 99—"the degree of control of a system is proportional to the logarithm of the information in this system"—require adequate definition of the terms used.

However, we may assume that no manager will mind since he will not read the book as carefully as a reviewer must; and he will find it pleasant bed-time reading. It may even induce him to give O.R. a chance, offering the salaries mentioned on p. 111 as prevailing in 1962.

S. VAJDA

18. Some Techniques of Operational Research. Prepared by the Operational Research Group of the National Coal Board. Edited by B. T. Houlden. London, English Universities Press, [1962]. x, 153 p. 8½″. 21*s*.

The Field Investigation Group of the National Coal Board has for many years been one of the main centres of Operational Research in the U.K. and 23 of their present and former members have contributed to this pleasantly produced booklet. The text is derived from a training course for entrants to the group and deals with techniques found to have been useful.

It took some time to write the book and the techniques mentioned are not always the most recent ones. The presentation is uneven but some parts are a valuable contribution to the literature.

The first chapter is introductory. The next two deal with allocation (i.e. linear programming) and with the theory of games. These chapters read like notes, to remind the student of what has been said (with more precision, we hope) during lectures. Most

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statements are only just correct, if interpreted generously. For example, it is said on p. 27 that "in the present state of development" only two-person zero-sum games "can be solved". This ignores the massive and potentially more useful studies of non-zero-sum and of n-person games. But perhaps the writer means something special by "solved".

Chapter 4 on dynamic programming is excellent. It deals with just as much of the theory as is useful in this context. It starts with a simple problem, presents the basic principles, and proceeds to more complex examples. Chapter 5 on queues is equally well written, though in the nature of things it cannot be as exhaustive. It includes a report on a simulated sampling experiment and also a collection of steady-state formulae.

The next chapter, on stock control, can only scratch the surface of the subject, and the style of Chapter 7 is characterized by such phrases as "there are three main types of life curves" (p. 136). Proofs are generally omitted. Chapter 8, Theory of Search, might just as well have been omitted. It contains a section on the Mean Path Theorem without ever stating what this theorem is, it paraphrases an easily accessible paper by B. O. Koopman (not O'Koopman, as on page vii) and finishes off with a section on the use of clustering techniques in surveys.

Editing a book of this type is difficult and it has on the whole been successful. Only a few misprints were noticed (p. 55: there is no c in Shubik; p. 78, last line: for λ_t read λt). But what made the eminent writer of the Foreword think that "attention was first directed to the problem of queueing . . . when a large number of bombers had to land"?

S. VAJDA

19. Quality Control in the Office. By P. N. Wallis. London, Current Affairs Ltd, 1961. 190 p. $8\frac{1}{2}''$. 30*s*.

This book contains a competent exposition of the theory and practice of statistical quality control applied to office work. Good quality of work is taken to include not only the avoidance of error but such aspects as good appearance of typed work and prevention of delay. The non-mathematical reader will find no difficulty in understanding the reasoning and the practical office manager will appreciate the chapters on the application of sample checking in such common areas of work as accounting, production control and stocktaking. There is a chapter also on the particular problems raised by the use of electronic computers; here the quality of input data is even more vital than in clerical processes because the "common sense" of the clerk is lacking in the machine. The appendix summarizes the limited amount of mathematics necessary for the methods recommended.

The particular problems of audit (both internal and external) are dealt with and attention is drawn to the advantages of continuous sample checking as distinct from checking only those accounts and supporting papers which relate to a past period. Auditing usually involves basing an opinion on a check of a limited number of entries or papers and the case for a statistical basis to the sampling is clear. Unless the auditor calculates the size of sample statistically, he may be incurring too great a risk of forming a wrong opinion or alternatively of doing too much checking for his purpose.

Any book which encourages a more scientific approach to the problems of office management is to be welcomed, but it is only fair to point out that this volume covers in detail but one aspect of the subject-matter of the title. The fact is that *statistical* quality control can be applied only to work where an acceptable standard of quality has been achieved. To obtain the required standard of acceptability a 100 per cent. check may be necessary and, if such a check is made, an additional sample check may not be worth while (except for auditing purposes). If the process is one where a certain proportion of minor errors is tolerable (e.g. an internal statistical or cost analysis or correspondence typing) then a 100 per cent. check is unnecessary and a sample check will provide a safeguard against undetected deterioration in the standard of work. A problem that arises in practice is that the quality of work often varies considerably between one individual clerk

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or typist and the next, and between one day and the next. One day's work for each person will often not constitute a *population* sufficiently great to justify sampling so that any remedial action or additional checking has to be relatively long-term. In some cases there is a tendency for errors to be "bunched" rather than randomly distributed through the work.

Clerical quality control in the wider sense involves the analysis of errors with the object of preventing recurrence. This is often achieved by a change of method. For example, the use of address plates for entering customers' details on sales order forms instead of typewriting will prevent any error of detail. Photocopying similarly cuts out the need to check because errors of detail are impossible. Window envelopes effectively prevent the error of putting a letter in the wrong envelope. The areas of work where statistical quality control is most useful are those in which the operator has plenty of scope for error but normally produces good work which need not be checked completely.

Mr Wallis points out that the cost of checking must be compared with the cost of errors to find out whether checking is worth while. This is true but the cost of errors may be difficult to determine if part of the cost is in loss of prestige or goodwill. An error on an invoice may not be damaging financially if it is discovered by the customer, but how much weight is to be given to the annoyance caused? If a man's wages are wrong the firm may not suffer directly but confidence in the Wages Department will be affected. However, careful consideration of the cost of checking and the likely results of errors may lead to useful economies: many firms receive a large proportion of low-valued invoices and it may easily be shown to be wasteful to check the *arithmetical calculation* on these documents. A continuous sample check can show that the original assumption about the normal accuracy of these calculations continues to hold good.

For convenience in describing the application of quality control, the author introduces a character called the Quality Controller who goes around the firm applying statistical quality control. In practice this function may be undertaken by the Supervisor in each working group. In a large business an independent Quality Inspector would have to be remarkably versatile to provide sample checks of all the office work and he would tend to cause friction by running across normal operational responsibilities. A smaller business might be deterred from applying the methods of statistical quality control if it appeared necessary to employ a specialist for this purpose. The establishment of methods of quality control should be the duty of the operational manager aided and abetted by the Organization and Methods unit if the firm has one.

Statistical quality control has long been recognized as a valuable technique in factory production and its application in the office is overdue. Here is a further tool for achieving greater efficiency for the use of all those concerned with the running of offices. They will find this book a valuable aid provided they realize that statistical methods in this context are only one aspect of the problem of quality control in the office. The real value of sample checking lies in the extent to which it can be used in substitution for 100 per cent. checks rather than as a method of additional inspection.

D. F. C. MANN

20. Equitable Assurances. By Maurice Edward Ogborn. London, George Allen & Unwin, 1962. 271 p. 9½″. 42s.

There is no complete and authoritative history of the actuarial profession, with its origins extending as far back as the publication, in 1662, of John Graunt's *Natural and Political Observations upon the Bills of Mortality*. This is not to overlook the official history of the Institute of Actuaries by R. C. Simmonds, published in 1948 on the occasion of the Institute's Centenary. But that volume was concerned with the development of the Institute, the first professional body of actuaries, rather than with the evolution of the methods employed by actuaries in the discharge of their basic distinctive function as the

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financial advisers of life offices and other analogous institutions. The present study, written to celebrate the bicentenary of the Equitable Life Assurance Society, is modestly described as "The story of Life Assurance in the experience of the Equitable Life Assurance Society 1762 to 1962". It is no mere self-adulatory volume which, given a suitable occasion, important bodies in finance, commerce and industry are nowadays wont to produce for reasons of status and publicity. It is, in fact, a scholarly historical account of the manner in which the principles of modern life assurance finance came to be established and developed by the "Society for Equitable Assurances on Lives and Survivorships", now known as the Equitable Life Assurance Society (the "Old Equitable"). It provides vital chapters for any account of the rise of the actuarial profession.

There were actuaries—as there were life offices—before 1848 when the Institute of Actuaries was founded, and in the absence of professional examinations many of them were mathematicians of F.R.S. calibre. It is not surprising therefore to find that the basic actuarial principles of life assurance finance were first laid down by James Dodson, F.R.S., then Master of the Royal Mathematical School, within Christ's Hospital. At that time there were already in existence such life offices as the Royal Exchange Assurance and the London Assurance, both constituted by Royal Charters during the South Sea Bubble—in 1720 for the transaction of marine insurance business and in 1721 for fire and life assurance business. But these offices offered only limited life contracts from year to year, similar to those that were being granted for fire insurance business. These offices have survived to the present day and, led by the Equitable, were in due course to offer the long-term types of contract which are characteristic of life assurance as we know it.

As the author remarks, Dodson's general principles "are just as valid today as then and form the foundation of the vast life assurance businesses that have grown from such small beginnings". In essence, Dodson proposed that level premiums, related to the age at which insurance commenced, should be paid throughout the duration of the contract. This implied that, in the earlier years, the premiums would be in excess of the sums necessary to cover the current risks of death. This excess was to be saved up against the later years when the level of premiums would not suffice to cover the current risks. Under such a system it is apparent that for a long period ahead a society's funds would steadily grow, quite apart from the further growth that would accrue from any increase in the numbers insured. This natural, but mortgaged, growth was to prove a fruitful source of misunderstanding, for as the Equitable Society grew a proportion of the members confused such growth with the surpluses that were disclosed at the periodical valuations. These true surpluses represented the excess of the Society's accumulated funds over the amount calculated to be necessary to meet the future liabilities for the death claims in respect of existing members, making due allowance, of course, for the premiums they would pay and the interest earned.

Dodson called a meeting in March 1756 at the Queen's Head, Paternoster Row, which led to a series of further meetings that enabled him to explain his proposals for mutual life assurance and to enlist support with a view to obtaining a Royal Charter. Dodson died in 1757 and did not see the establishment of the "Equitable" which took place in 1762, after the Attorney-General and the Solicitor-General had, following prolonged cogitation, reported adversely on the petition for a Charter. As a result, the Society took the form of a voluntary partnership under a deed of settlement. This method of association was to give the members ample scope for harassing the Directors about the distribution of the Society's growing surpluses, quite apart from the misunderstanding of the nature of the surpluses to which reference has been made. In the preliminaries, the prominent figure was Edward Rowe Mores, who was incidentally, among other things, an antiquary and a classic. It was Mores who was responsible for the choice of the title "Actuary" for the chief executive official of the new Society—an antiquarian whimsy, as the author describes it. In due course the name became attached to the professional men who used their mathematical abilities in the service of life offices and, it can be added, of other bodies and activities.

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For many years the Society was indeed a battleground between the earlier and later generations of members concerning the equitable distribution of the substantial surpluses which were revealed at the successive decennial valuations. Among the factors which contributed to these satisfactory results was the assumption that the mortality of members would accord with Dr Richard Price's Northampton Table which, being based on the mortality of a section of the general population, provided a substantial margin in comparison with the mortality experience of lives who were selected for insurance with some care and who, for many years, were actually interviewed by the Court of Directors, while acceptance had to be by the unanimous vote of the Court.

In the long controversies and lawsuits about the surpluses William Morgan, F.R.S., was the sheet-anchor of the Society. He was appointed Assistant Actuary in 1774 and Actuary the following year, an office he held until 1830, when he was succeeded by his son Arthur, who, too, was later elected F.R.S. and occupied the post of Actuary until 1870. William Morgan's resistance to pressure from policy holders for the indiscriminate distribution of the Society's resources was a major influence in building up the Society's strength. His lucidity of exposition is reminiscent of that of the late Sir Alfred Watson. There can be little doubt that Morgan's skill and force of character made a major contribution to the establishment and reputation of British life assurance. Morgan was, it may be remarked, a man of outstanding ability whose intellectual activities extended far beyond his actuarial duties. His uncle was the celebrated Dr Richard Price, who advised the Society from time to time, and through him he was obviously influenced by Joseph Priestley, the chemist. In 1781, for example, Morgan published the results of his own experiments in *An Examination of Dr. Crawford's Theory of Heat and Combustion.* He was also interested in electricity and conducted experiments on the passage of electricity through a vacuum to which a minute quantity of air had been admitted. His experiments were described in a paper to the Royal Society in 1785, but were forgotten until 1925. He was clearly a man of parts. For his work on the evaluation of survivorships he was awarded the Royal Society's Copley Medal in 1789. He was elected a Fellow in the following year, subsequently serving twice on the Council.

The author has done much research into the origins and background of the numerous prominent personalities who, in their different ways, were associated with the growth of the Society. Those who are interested in people will find much of the information here recorded of fascinating interest.

It is difficult to find anything to criticize in so thorough and weighty an account of the development of life assurance and, as already pointed out, of the pre-Institute period of the actuarial profession when so much was owed to F.R.S.'s. But perhaps the author will permit the observation that the reference to John Graunt is a little inadequate. By a happy coincidence the tercentenary of the publication of Graunt's *Observations* was celebrated by the Royal Society in November 1962 as the foundation of the science of demography. Graunt's claim to be among the founders of the actuarial profession resides in the fact that he was the first to put forward the idea of a life table, which he did in the following passage in his *Observations.*

"From whence it follows, that of the said 100 conceived there remains alive at six years end 64.

At Sixteen years end	40	At Fifty six	6
At Twenty six	25	At Sixty six	3
At Thirty six	16	At Seventy six	1
At Fourty six	10	At Eighty	0."

This was surely a major "break through", and though his life table may have been "rudimentary" it was the first indication of a way of utilizing observed mortality data in a systematic fashion which has been followed ever since. The life table and its later derivative, the "service" table, which implicates other decremental forces as well as death,

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are so much the actuary's basic tools that John Graunt must be listed first among the actuarial pioneers, nearly all of whom, as already noted, were, like him, Fellows of the Royal Society. Actuaries, as such, would also have liked to know a little more about the technical details of the decennial valuations conducted by William Morgan. But this is mere carping about a work which can be unreservedly commended for its general interest and as a contribution to social and economic history as well as to the history of the actuarial profession. In the latter connection, the Society has played a leading part of which evidence is afforded by the long list of distinguished actuaries who have served the Society as Actuary and among whom in later days the names of Lidstone and Elderton are outstanding.

F. A. A. MENZLER

21. Annual Epidemiological and Vital Statistics, 1958. Geneva, World Health Organization, 1961. 860 p. 11″. 80*s*.

This is the eleventh volume of *Annual Epidemiological and Vital Statistics*, a series of reports published by the World Health Organization since 1948. The ten previous volumes covered the years 1939–46, 1947–49 and each of the years from 1950 to 1957. In essence, the series is a continuation of the *Annual Epidemiological Reports* published by the Health Organization of the League of Nations for the years 1922 to 1938. The general aim of the publication is to provide the most recent information available on population, mortality and morbidity.

The statistics published in the present volume are the result of a common effort by national health and statistical administrations, by the Statistical Office of the United Nations and by the World Health Organization. All the data were obtained from official sources. They were either extracted from the statistical reports published periodically by the various national administrations or obtained by means of questionnaires.

The information is arranged in three parts. Part I, consisting of eight tables, provides details on the area of the country, total population by sex according to the most recent census or population estimates, absolute figures by sex and age of causes of death according to the abbreviated List (List B) of the *International Classification of Diseases*, age- and sex-specific death rates from all causes and for selected important causes of death, and as much information as possible on infant deaths. Part II, covering 21 tables, deals with statistics of 32 infective and parasitic diseases. Absolute figures, showing the number of reported cases and deaths for 20 important infectious diseases, are given by month for most countries, but for some, where information by month is not available, the total yearly figures are shown. In some instances, these data are also given by age and sex. Part III, containing five tables, is a general section including information on such subjects as health personnel, hospitals and similar establishments, and prophylactic vaccination for 11 diseases. This part has been expanded as compared with previous issues since this is important in public health planning.

Over 50 countries and territories of the world are included in the analysis and it is not surprising that the volume runs to nearly 900 pages. The tables are well indexed with the result that rapid reference is possible. The print is clear and the layout pleasing. Although the price of £4 may seem substantial, the extent of the information contained more than justifies the cost. For students who cannot afford to purchase the complete volume, special offprints have been produced of the tables on causes of death that are of wide interest, such as malignant neoplasms, cardiovascular diseases, accidents and infant deaths. Offprints have also been made of the tables giving death rates by all single categories of List B and of the tables in Part III.

A. BARR

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22. Manuals on Methods of Estimating Population. Manual 1. Methods of Estimating Total Population for Current Dates. New York, Population Division, United Nations, 1952 [Reprint, 1962]. vi, 45 p. 10½″. 5*s*., $0.75. (ST/SOA/Series A Population Studies, No. 10.)

This is a reprint of a manual which was first published in 1952 and has now been re-issued without alteration. It is (or was) the first in a series of manuals on methods of estimating population which the Population Division of the United Nations Department of Social Affairs has prepared in accordance with a recommendation of the Population Commission.

The manual contains sections dealing with the methods and problems associated with estimating the total population under varying conditions regarding the availability of data. At one extreme only estimates of a conjectural nature are possible. Progressing towards greater availability, estimates may be based on incomplete censuses or non-censal counts, on a single complete census count, on extrapolation from two or more census counts, on regular censuses supplemented by vital statistics and migration records and, finally, on a system of continuous population registers.

This short manual is almost wholly descriptive in nature apart from formulae to be used in extrapolation. This is probably the correct approach in as much as there is little theoretical difficulty in making estimates of the current population; the troubles in this field usually arise because the required data are not available or are deficient in some way; the migration statistics for the United Kingdom provide an example of this problem, their condition being all the more aggravating because of the relatively high standard of the other data used in national estimates of population.

The manual is stated to be designed for people working in countries where the available statistical information is rather limited but its interest is not confined to such users. Although in most advanced countries population estimates at the national level are of a high standard the availability of data for local areas may well be similar to that prevailing in less developed countries at the national level.

This manual is well set out and interesting to read with less of the tendency to concentrate on the rather obvious than is sometimes found in such international publications. There has perhaps been a tendency for the United Nations work in the statistical field to aim too much at the level of the lowest common factor and more attention could be directed to exchanging information on more advanced techniques and for the discussion and refinement of new methods.

J. A. ROWNTREE

23. The Registrar General's Statistical Review of England and Wales for the Year 1960, Part II, Tables, Population. London, H.M.S.O., 1962. xii, 180 p. 9½″. 12*s*. 6*d*.

The population of England and Wales at June 30th, 1960, is estimated as 45,755,000, an increase of 369,000 over the estimate for the previous mid-year—one of the largest increases ever recorded. In the same period, the population of Northern Ireland increased by 12,000 (proportionally to population, a similar increase to that of England and Wales) whilst the population of Scotland increased by 16,000 and that of the Irish Republic decreased yet again (by 12,000). The natural increase (excess of births over deaths) element in this increase was least in England and Wales (0·57 per cent. compared with 0·75 per cent. in Scotland, 0·99 per cent. in the Irish Republic and 1·17 per cent. in Northern Ireland) so that migration is seen to account for the major part of this differential in growth rates. Much of this is customary—the outflow from the Irish Republic for instance —but the flow into England and Wales was unusual. Since statistics on migration to and from England and Wales are incomplete, net migration can only be judged from net passenger movement, which showed an inward balance of 122,000 in 1960, possibly the largest-ever inward balance in any single year. (The large inflows of refugees in the early years of this century were largely offset by outflows to the U.S.A. and the Commonwealth.)

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The marriage rate at 15·0 (14·9 in 1959 and 15·0 in 1958) was one of the lowest since marriage registration began in 1837. This low rate is due to the ageing of the population and depletion of the unmarried by the high age-specific marriage rates of the last two decades, since age-specific rates are still at an almost unprecedented high level; indeed for spinsters they are probably higher than ever before. It does appear, however, that the sex ratio, which has become unfavourable for males (i.e. there are more unmarried males than unmarried females some 2–3 years younger, from whom they most customarily seek their brides), is at last checking marriage rates for bachelors. For some age groups, the bachelor marriage rate did rise from 1959 to 1960, but for the age group 20–24 (the group into which more than half bachelor grooms fall) a small decline was at last registered (1960 rate 157·8; 1959, 159·9; 1958, 158·3). This may well herald a rise in the marriage age for bachelors or a decrease in the proportion marrying. The marriage age of spinsters will, however, have to decline still further before the "momentum generated" is lost and a stable state is reached.

The birth rate at 17·1 (1959, 16·4) was the highest since 1948 (17·8) and represents a continuation in the rising trend which started in 1955. The detailed fertility tabulations show that, with few exceptions, this rise is shared by women, whatever their age at marriage and present duration of marriage. For example, women celebrating their tenth wedding anniversary in 1960, who had married at ages under 20 years, had by then had 2·5 children on average, more than any marriage cohort since those married in 1921. Similar comparisons at this duration apply to those marrying at various ages over 20 years. Owing to the growing tendency to concentrate childbearing into the early years of marriage, the average family size of recent cohorts at durations over 10 years compare progressively less favourably with those of past marriage cohorts.

A more liberal attitude to illegitimate children was recorded with the extended provisions for legitimation (of children born illegitimate) contained in the 1959 Legitimacy Act, which came into operation on October 29th, 1959. In consequence, the number of re-registrations of the births of legitimated persons in 1960 (6,506) was more than double that of any recent year (2,905 in 1959; 2,636 in 1958).

A new feature regarding the adoption of children is the granting of *provisional orders* to enable persons to remove children from the country to adopt them abroad. These have been granted since April 1st, 1959, and 71 were granted in 1959 and 207 in 1960. These numbers compare with the current rate of granting of "full" adoption orders, currently running at 13,000–15,000 per year.

The persistence for so long of the rising trend in the birth rate has led the Government Actuary and the Registrar General to revise upwards the fertility assumptions on which their annual population projections of England and Wales are based. The current projections show the population of all ages to increase to 55,646,000 in the year 2000. Despite the revised fertility assumptions, the proportion of children under age 15 rises only slightly from 22·8 per cent. in 1960 to 23·8 per cent. in the year 2000. The proportion of "retired" persons (males over 65 and females over 60) rises from 14·9 per cent. in 1960 to 17·4 per cent. in 1980 and thereafter declines to 15·6 per cent. in the year 2000. From 1960 to 2000 there is a shift in the age structure of the population of working age (males 15–64, females 15–59)—those under age 45 increase from 39·5 per cent. to 41·3 per cent. whilst those over age 45 decrease from 22·8 per cent. to 19·3 per cent.

N. H. CARRIER

24. The Registrar General's Quarterly Return for England and Wales. Third Quarter, 1960. London, H.M.S.O., 1961. 20 p. 9¾″. 2*s.* 6*d.*

The marriage rate for the quarter (19·1 compared with 18·0 for the same quarter of 1960) was the highest third quarter rate since 1951 (20·2). During the period 1956–59, the marriage rate declined sharply—mainly due to the depletion of the unmarried popula-

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by the generally high marriage rates of the last 20 years—but there has been a slight recovery in the last two years. The high rate recorded for the third quarter of 1961 is partly explained by this trend. In addition, the dominant features of seasonal trends in marriage for the last 15 years have been concentrations in the first quarter (the most advantageous time to marry from Income Tax considerations when the bride is not working) and in the third quarter (the best when she is working). The third quarter 1961 rate may therefore be explained partly by an increasing tendency for wives to work.

The birth rate for the quarter at 17·6 (1960, 17·2) exceeded all corresponding rates since 1949 and continued the rising trend that started in 1955. The death rate at 9·5 was less than the rate for the third quarter of 1960 (9·8) but exceeded that for 1959 (9·0). Once again a record was set by the infant mortality rate (18·1—previous lowest was 19·4 in third quarter 1959) and this is now so common that it is too easily taken for granted. It is, however, a matter for congratulation that, although the rate is so low that there is little room for improvement, small gains continue to be made. The still-birth rate (18·3) was also a record low (1960, 19·3).

Poliomyelitis notifications, which had shown a spectacular fall as the vaccination campaign got into full swing, showed a rise over the third quarter figure in the previous year. (Notifications in third quarter: 447 in 1959; 138 in 1960; 261 in 1961.)

Deaths from transport accidents in the quarter showed some improvement over the figures for the previous year (males: 1960, 1,375; 1961, 1,232; females: 1960, 464; 1961, 425) but other accidental deaths showed little change (males: 1960, 1,101; 1961, 1,135; females: 1960, 1,047; 1961, 1,004).

N. H. CARRIER

25. Road Risks of the Elderly: The Dangers to which they are exposed, and how they may reduce the Risk of Serious Injury. By G. Prys Williams and Harold S. Goodwin. London, Magazines & Publications, Ltd, 1961. 36 p. 9½″. 3*s*.

This booklet of 36 pages, which is accompanied by 14 pages of diagrams, commissioned by the Christian Economic and Social Research Foundation, is based on data from the Ministry of Transport, the Road Research Laboratory, D.S.I.R., and the British Market Research Bureau. It gives the results of detailed analyses of road accidents to people over 60 years of age and suggests to them how they might behave in order to reduce their chance of being involved in road accidents.

Frequencies are given of the number of deaths, serious injuries, and other injuries to pedestrians, pedal cyclists, motorcyclists, other drivers and passengers, in the age-groups under 60, 60–69, 70–79, 80 and over. In addition, the results of a survey of the numbers of pedestrians of various age-groups who were on the road at different times of day are given. Frequency distributions of accidents by age of road user involved have been published, but the reviewer knows of no previous survey of ages of pedestrians using the roads of the age-groups under consideration.

The assembled data make it possible to deduce some results of interest:

(1) The greater the age the greater the likelihood of death or serious injury when involved in an accident, e.g. whilst the proportion of injuries that are fatal is only 2 per cent. for motorcyclists under 60, it is 13 per cent. for the few motorcyclists over 80. The corresponding figures for pedestrians are 2·5 per cent. and 13 per cent. and for pedal cyclists 1·2 per cent. and 7·5 per cent.

(2) The relative risks of being involved in a fatal accident, taking what the authors call pedestrian density into account, are greater for men than for women, are generally least between 10 a.m. and 1 p.m. in the morning in the summer, and at a maximum between 5 p.m. and 8 p.m. in the winter and autumn. The risk for a male pedestrian over 80 between 5 p.m. and 8 p.m. in the evening during October to December is 21 times the risk of a female pedestrian 65–69 between 10 a.m. and 5 p.m. in spring and summer, i.e. April to September.

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There is another point of interest not shown in this report. It has previously been found† that the elderly are specially prone to be injured by motorcycles, presumably largely because these vehicles are less likely to be seen than are larger ones. For example, the number of pedestrians of the older age-groups injured per vehicle-mile by motorcycles is more than three times the rate for cars on Class I and Class II roads in built-up areas.

The authors have written this book for elderly people to read, hoping that these people will adjust their behaviour in accordance with the results the authors find. For example, it is suggested that they use the roads at times when the risk is least. It is hoped that the authors will attempt to find out if, in fact, these results on the risk of accidents will be read and acted upon by the people concerned.

R. J. SMEED

26. Man-Power Statistics, 1950–1960. Statistical Bulletin of the Organisation for Economic Co-operation and Development. Paris. 89 p. 12½″. 9*s*. (O.E.C.D., 1961. London, H.M.S.O.)

This report contains the manpower statistics, so far as they are available, of the member countries of the European Economic Community, the European Free Trade Association, the European O.E.C.D. countries, Canada and the United States. The figures cover the years 1950 to 1960, giving annual data only. There are gaps in the statistics, but fairly satisfactory results have been obtained for 13 of the 20 countries. It has to be remembered, however, that there are differences in general concepts, classification and the methods used in collecting data. The information has been extracted from available publications, supplemented by questionnaires and checked by the national statistical authorities.

The subjects covered are population and labour force, persons in civilian employment and numbers of wage-earners and salaried employees, analysed by activities. For each of the countries definitions are given of the sources of the statistics with notes on such subjects as coverage, method of computation, dates of reference, etc.

The report serves a useful purpose in bringing together the available statistics and also in indicating the gaps which should be filled in the various countries in order that comprehensive information on the subject of manpower, which will be essential in the discussion of many problems by the organization, may be studied.

R. B. AINSWORTH

27. Facts and Figures about the Church of England. By the Statistical Unit of the Central Board of Finance of the Church of England. London, Church Information Office (Church House, Westminster, S.W.1), 1962. 92 p. 11¼″. 21*s*.

This attractively presented publication, embodying much new information, is one of the results of the establishment in 1955 of an enterprising statistical unit working under the Central Board of Finance at Church House; it owes much to the skill and pertinacity of the statistical unit's head, Mr R. F. Neuss. It contains fascinating material of general interest, on the debit as well as the credit side, about one of the great institutions of English society.

The bulk of the book is 93 tables, together with good charts and careful explanations. Most of the statistics (including much historical material) bear on three of the central problems of church policy: professional manpower, membership (in relation to the population "at risk") and finance.

The data provided about professional manpower are admirable; immense work has gone into the compilation of an accurate register of the clergy, which never existed before.

† GARWOOD, F. and JEFFCOATE, G. O. (1958), "Some facts about casualties to pedestrians in Great Britain", *Z. Verkehrssicherheit*, **4** (5/6), 216–225.

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Now, few professions can have so much statistical self-knowledge (contrast the controversy arising from the ambiguous statistics about how many doctors remain in this country).

Most interest centres on the manpower replacement problem, and some important conclusions can be drawn from the figures. The curve of ordinations—the new intake—has been rising steadily since the war; in 1961, it probably just exceeded losses (deaths, retirements and emigration), and, at over 600, was about as high as at any time this century (even at its height in the 1880's, the number of ordinations was never more than about 750). But although the present rate of recruitment will maintain for a time the number of active clergymen at about its present level, this may not last. The age structure of the clergy is such that death and retirement (which have been fairly stable lately) will take an increasing toll in the next 20 years. Recently, retirements have reflected the relatively small intake during, and for some years after, World War I. Ordinations continued to rise until they reached a plateau in the 1930's. The "bulge" in the age structure thus consists of the men in their early 50's, who were ordained in the peak period of the late 1930's, and who will be retiring in the late 1970's. (Moreover, the average retirement age, now 70, will doubtless fall if pensions are improved.) This particular bulge, of course, exists in other professions, for similar reasons, and will lead to increasing retirement losses even sooner; but for most of them, recent intakes have been higher, relatively to pre-war intakes, than for clergymen.

Do the figures suggest a reasonably satisfactory deployment of manpower? Sixty years ago, the total number of active clergymen reached its peak, probably at about 20,000, against 15,000 now. Although historical statistics are missing, there has no doubt been some rationalization: over 40 per cent. of livings have more than one church; the total population per clergyman under 65 has grown from 1,500 at the beginning of the century to 3,000 now. But one-third of the incumbents are still in parishes with total populations under 1,000.

When we come to church "membership", great difficulties of quantitative assessment arise. Unlike many Free Churches, the Church of England has no effective register of active membership. This seems to be one of the penalties of the Church's ambiguous status in society. Nominally, the main test of membership is baptism in the Church of England. On this theological but somewhat illusory test, the Church of England has about 21 million members of 16 and over—half the relevant population total. Certain other objective criteria are possible. The number of confirmed persons is nearly 10 million. The number registered on electoral rolls of the parishes is nearly 3 million. And the number of Easter communicants is 2¼ million. None of these figures, however, can really be regarded as a measure of the number of active Anglicans, although all have been used for this purpose.

The only other information available (not quoted in the book) comes from sample surveys. It appears from a survey of 2,300 people, made by the Gallup Poll in 1957, that rather under 20 per cent. of the population of England over 16 attend an Anglican church once a month or more (which seems a fair test of active membership). This would give a figure of about 6½ million. Perhaps enough prestige is still supposed to attach to churchgoing to suggest some overstatement. But a realistic count of active members might, on this basis, be expected to yield at least between 5 and 6 million, excluding children (and there are still over 1 million children on Sunday School registers). Until there is better information, the assessment of the Church's place in society must remain obscure, and one would doubt whether appeals for men and for money can carry full conviction. Much less (as the Bishop of Middleton points out in his introduction to this book) can the Church determine an appropriate policy for what is generally supposed—without much evidence—to be an "age of disbelief".

There are few data about the historical trends of religious activity. By the test of Easter communicants, the only long series, it appears that the current number—2¼ million—is not far below the peak (a fairly steady 2½ million in 1924–35); the figure dipped steeply

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after the mid-thirties, but has, on balance, been rising since the end of the war. The proportion of Easter communicants to population tells a somewhat different story: it is now about 7 per cent. of the population over 15. But it is surprising that even just before World War I, when this proportion reached its highest point since the records began in 1885, it was not much over 10 per cent. Perhaps the obligation to communicate at Easter enjoined by the Prayer Book is more strictly observed than it used to be; but could the "active membership", less strictly defined, have been much above a quarter of the English population within the last hundred years?

On finance, the statistics are more informative. But it would be still more helpful if the finances of the Church Commissioners could be consolidated with those of the parishes. (This is done only incompletely in two tables relating to "benefice incomes", of which the coverage is not fully explained.) The Church Commissioners administer the accumulated and mostly ancient church endowments; these now provide an income of about £15 million. The receipts of the individual parishes from other sources, now reported in a much improved form, amount to over £17 million, of which only a tiny fraction is new property income; over 90 per cent. comes from voluntary contributions (including profits from the essential bazaars and jumble sales). It thus appears that the active Anglicans of the Church Militant meet little more than half its expenses; the enterprising investment policy of the Church Commissioners has filled the gap by putting to better use the legacies of the Church Triumphant.

By bringing together the central and parochial statistics, it appears that the average incumbent's income has now been brought up to about £1,000 a year gross. This includes contributions specifically allocated towards expenses which most of the laity must meet out of their incomes: local rates, mortgage payments, dilapidations. It must be remembered that the clergy have to pay many other expenses of office out of this gross income and that maintenance of parsonages is often a heavy but unavoidable burden.

Now that the Church Commissioners pay about three-quarters of the total salary bill, there has been great progress towards a fairer distribution of incomes. The figures in this book do not show how much. There are no details of the size distribution of clergy incomes (of which, however, some figures are given in the 1961 Report of the Church Commissioners).

A fine job has been done in collating and presenting the information which can be got within the existing administrative structure. What seems to be needed now—both by the policy-makers and by the student of religious trends—is a new effort to find out how many people, and what kinds of people, respond to the Church by active participation, and who do not.

C. T. SAUNDERS

28. Measurement of Achievement in Mathematics. A Statistical Study on Effectiveness of Board and University Examinations in India. By A. K. Gayen, P. B. Nanda, R. H. Mathur, P. Duari, S. D. Dubey and N. Bhattacharyya. New Delhi, Ministry of Education, 1961. viii, vii, 176, xl, xiv p. $9\frac{3}{4}''$. No price.

This report, described as No. I of a Research Project, relates primarily to a detailed analysis of a 10 per cent. sample (3,700 candidates) of results from the compulsory mathematics paper in a School Final Examination. The question paper is deplorably old fashioned, and demanded of the candidate very careful reading in order to understand the system of options and the marking scheme; it appears in need of so much improvement of an elementary kind as to make the authors' efforts to study relative difficulties and discriminatory powers of individual questions scarcely worth while.

There is little to interest the statistician here, either in methods or in conclusions. The extensive discussions of discriminating power contain nothing of importance that is new,

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but perhaps the suggestion that maximum marks ought to be in proportion to the difficulty of a question deserves further attention. One may agree strongly with the authors' views that more research into the setting and marking of examinations is needed, without feeling that this quite excessively long report has any importance outside India.

D. J. FINNEY

29. The Share of Top Wealth-Holders in National Wealth, 1922–56. By Robert J. Lampman. Princeton, University Press, 1962. xxvii, 286 p. 9″. $6.50. (National Bureau of Economic Research, Gen. Series No. 74.)

In this country the death duties method has been used on numerous occasions to prepare estimates of personal wealth for various years since 1911, and such estimates are now prepared by the Inland Revenue and published in their Report. It thus comes as something of a shock to realize that this book represents only the second use of the method in the United States; the first was by Horst Mendershausen for Raymond Goldsmith's three-volume *A Study of Saving in the United States*. The necessary data are not even published regularly by the Revenue authorities, as they have been for over 50 years in this country, and the present study was partly based on special tabulations. The failure to publish the data, in its turn, is undoubtedly due to the high filing limit for federal estate tax returns. This limit has varied over the years covered by the study from $40,000 to $100,000, with a level in the more recent years of $60,000, so that the resulting estimates of personal wealth never relate to more than 2 per cent. of the adult population and in 1929 to as little as 0·44 per cent. This small but wealthy group of the population are the "top wealth-holders" of Professor Lampman's study.

It is interesting to see how the same problems occur with the federal estate tax as with the English estate duty although there are slight variations. Within the limits of the data available Professor Lampman seems to have tackled all these problems, and his use of a sample investigation by a large life office to reduce the value of insurance policies from sum assured to surrender value is ingenious. His mortality multipliers were also based on life assurance data. One of the biggest problems, especially with wealthier estates, is that of personal trust property which is not liable to estate duty. A smaller proportion of personal trusts seems to be caught in the United States than over here, but this gap is more than compensated for by having reliable income figures for all personal trusts; these can be capitalized to yield capital values which are tolerably accurate.

There is, however, a special problem in using federal estate tax data for estimating the concentration of personal wealth. In this country it has always been possible to use estate duty figures (together with figures from probate returns when necessary) to estimate almost the whole of personal wealth by the same method from comparable data. With American data it is necessary to estimate the total of personal wealth from quite different sources and by quite different methods. In this case the figures used were the national balance sheet estimates prepared by the National Bureau of Economic Research. Professor Lampman faces the problem that errors in the national balance sheet figures could vitiate his comparisons with the wealth of the "top people", but his judgement is that errors of the order necessary to cancel the observed trend in concentration of personal wealth are unlikely.

This trend in concentration shows an interesting pattern. On his figures the top 1 per cent. of adults held 31·6 per cent. of personal wealth in 1922 and 36·3 per cent. in 1929. Thereafter the share of this 1 per cent. declined to 20·8 per cent. in 1949, but has since risen again to 26·0 per cent. in 1956. Comparable data for these years are lacking for Great Britain, but it will be interesting to see on the basis of further work on our own data whether the same trend and the same turning points can be observed in the concentration of personal wealth over here.

It is the great merit of Professor Lampman's study that he is not content merely to record the changes in concentration or to ascribe them to built-in tendencies of the

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Affluent Society, but that he makes an exhaustive examination of the factors responsible. It seems that changes in relative asset prices and in asset composition of personal wealth, changes in saving by different wealth and income groups, demographic changes and the growing prevalence of splitting wealth within the family by gifts *inter vivos* have all played some part. One would have liked a more exhaustive treatment of the different forms of estate tax avoidance and their effects on the estimates of personal wealth although it may well be that differences in tax law make them relatively less important in the United States, and some projection into the future, particularly of the changes arising from the demographic structure. Professor Lampman's work remains, however, a pioneer study in the explanation of changes in the concentration of wealth, a field in which far more work is necessary in this country with our relatively abundant data.

J. R. S. REVELL

30. The Industries of London since 1861. By P. G. Hall. London, Hutchinson University Library, 1962. 192 p. $7\frac{1}{4}''$. 12*s*. 6*d*.

This book by Mr P. G. Hall, Lecturer in Geography at Birkbeck College, is of wider interest than the title might suggest. Three aspects are dealt with.

The first section, dealing with older industries, makes ingenious and well-referenced use of sources, particularly the 1861 Census. The author concludes that the "place of residence" classification used in the 1861 analysis is not very different from a "place of work" classification: this point is the basis for much of the argument, and would seem to have merited more discussion. In Chapter 6 (p. 97) the problem is discussed for printers, and some reservations are made.

Ben Jonson's comment on a "Hounds-ditch man" is quoted on p. 56, and again on p. 117. Mr Hall does not do justice to his range of sources by using this one twice, but the repetition is indicative of a certain lack of unity in the book.

The conclusions on the economic factors affecting the selected older industries (clothing, furniture and printing) are not surprising, but the more general conclusion that in both the nineteenth and the twentieth centuries manufacturing industry has tended to be concentrated in London to the same extent as all employment is worthy of reflection.

The second part deals with newer industries, where official documentation is not on the whole so good or so well known. General conclusions are difficult to draw, since the units of both firms and land development tend to be large.

A concluding chapter discusses planning and industry. It suggests, using 1952 and 1958 figures, that the lower rate of growth in Greater London of employment in manufacturing industry compared with the country as a whole over that period was a more substantial result of industrial location policy than is sometimes realized. Statistical considerations are put forward to suggest that the growth of employment over these years in Central London is exaggerated by published figures. A study of later figures seems necessary to any conclusion about the present day.

J. A. BOUND

31. The Welsh Economy: Studies in Expansion. Edited by Brinley Thomas. Cardiff, University of Wales Press, 1962. xvi, 216 p. $8\frac{1}{2}''$. 30*s*.

This book is interesting on two different planes. For those for whom the economy of Wales has an intrinsic fascination it provides a series of surveys of several aspects of economic life in Wales, and in particular of its post-war structure. There are chapters upon agriculture, coal, steel, transport, population, income and expenditure which are mainly descriptive: and in most cases each author has collected together the chief statistics relevant to his subject and analysed them by the method of disaggregation. These disparate contributions are drawn together by Professor Brinley Thomas in a discussion of the expansion of the Welsh economy; and his attempt to describe the conditions of growth and

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the processes by which it has taken place provides, in the context of the current general interest in economic growth, some suggestive ideas about how it is initiated, sustained and accelerated.

It is a standard convention for theoretical studies to postulate an exogenous shock in order to set in motion the processes of growth and fluctuation. Is this merely a convenient expository device, or is it in fact necessary to have a powerful initial impulse? Professor Thomas ascribes the fact of the resumption of growth in Wales after the long stagnation of the nineteen-twenties and -thirties to the radical redistribution of resources that took place during the war years. The strategic dispersal of manufacturing firms to Wales and the recruiting of men into the armed services changed the industrial structure of the Welsh economy, reducing its dependence upon coal and steel. The services took roughly 20 per cent. of the male labour force from both Wales and the rest of Britain; but in Wales women were drawn into industry in relatively greater numbers than elsewhere; and the total labour force was reduced by only 4 per cent. compared with 8 per cent. Massive investment in new steel plants in Wales just before the war had checked the increase in the average age of the equipment of the industry and enabled the production of increasing quantities of steel with slightly fewer men. In the remainder of the manufacturing industry, total employment roughly doubled during the war. The size of the increase in that sector of manufacturing is not, however, itself an adequate explanation of the continuation of growth since the end of the war, and a more detailed analysis of the structure of the enlarged manufacturing industry would have been worth making. The first answer to the continued growth appears to have been that the new capacity was mainly for the production of intermediate goods which were more easily adapted to supplying peace-time demands.

The discussion of the expansion of industrial activity since the war is based upon two main sources of information, an analysis of the labour statistics for Wales and a few brief calculations upon some of the census of production data. This approach illuminates something of the processes of industrial growth: it demonstrates the extent of the continued redistribution of labour resources between the major industry groups and by sex, and suggests the need for a close examination of the processes by which this redistribution has come about. Since 1949 the number of men employed has not changed: the increase in the labour force has been in women. The number of men in manufacturing has risen by drawing upon the declining industries, and upon men released from the service industries by women taking their place. Yet, whilst it may seem obvious what in principle has been happening, we do not know the extent of the margin of employment within which this reallocation has taken place, nor how roundabout it has been. If it is true that the basic principle of planning is that it should be conducted in terms of the most scarce resources we want to know far more about the conditions of occupational mobility.

The weakness of the approach which analyses employment data and takes loose note of a few sketchy indications of rising productivity provided by Mr Parry Lewis is that it overlooks the rate of growth, yet Wales has experienced a different rate that would have been worth noting and examining. The information (which is not used) is the estimate of real output in Wales from 1948 to 1956 in Dr Nevin's *Social Accounts of the Welsh Economy*, which is regularly extended in an index of industrial production. Mr Lewis has produced an alternative estimate of the gross domestic product in Wales for one year (1950) that lies within 5 per cent. of Nevin's estimate. The series are not used to examine the behaviour of the economy, yet a suggestive comparison can be made between Wales and the rest of Britain. Their gross domestic products appear to have grown at about the same rates since 1948, but in Wales the growth of aggregate output comprehends a greater degree of redistribution of activity. In Wales, whilst agricultural production has barely changed, output has risen faster in all major sectors of production (manufacturing, gas, water and electricity and construction) and fallen faster in coal mining. These changes have been reflected in the trends of productivity: output per head has changed little in agriculture; but has risen faster in Welsh industry than elsewhere, not only in the rapidly growing sectors, but also in the South Wales coal industry, where there has in fact been a greater

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improvement than in any other part of Welsh industry. If we probe a little further it appears that the more rapid rate of increase of industrial output in Wales compared with the rest of Britain is associated with a slightly greater rate of increase in wages and salaries and a much greater rate of increase in profits. The natural growth of profitable businesses has been a more potent factor in industrial growth than post-war location of industry legislation.

Can the present rate of growth of Welsh industry be maintained? By 1954 the rise in output had reduced unemployment to about 20,000 which probably represents an effective minimum in present circumstances; but the approach to the full utilization of labour capacity did not check the rate of increase in output. It would be consistent with previous trends in migration for the current emigration, which has tended to diminish, to be converted in time into a net inflow. It may well be that labour resources are more mobile occupationally within Wales, perhaps because there was little housebuilding during the nineteen-thirties. The current movement of industry from the valleys to the coastal plain has been associated with a boom in housing and also in a growth of investment in vehicles that has been as fast as in the rest of Britain. The authors demonstrate how during the inter-war years stagnation was maintained by and promoted an unfavourable change in the age distribution of the population and of capital assets in coal and steel. It is unfortunate that it is less easy to examine the positive aspect of this relationship during the years of expansion.

KENNETH PANKHURST

32. Scotland's Scientific Heritage. By A. G. Clement and Robert H. S. Robertson. Edinburgh and London, Oliver & Boyd, 1961. 8, 151 p. 8½″. 18*s*.

This book records Scotland's contribution to science over more than five centuries. Much of it is given over to brief biographical details of Scots who have made a clear contribution to science—defined here to include engineering, medicine and agriculture but to exclude the social sciences. The criterion for the inclusion of a scientist as part of Scotland's heritage is that he should have discovered a law, theorem, effect, etc.; given his name to a physical condition or a scientific instrument; discovered an element; received a Nobel prize; been immortalized in the language; or been acknowledged by his peers as outstanding in his branch of learning. The listing of so much virtuosity might be tedious if it were not put in a general setting of the development of scientific knowledge and if so many of the names were not familiar from elementary science courses. But here is the rub. Scotland's contribution is not what it used to be, and will never be again if those in Scotland turn too much to the past.

Enumeration of the eminent scientists thus recognized forms the basis of comparisons of accomplishments over various periods and of performance in relation to what may be called England. Whether by count or in relation to population, Scotland's maximum contribution to science seems to have been made about the period 1830 to 1870. The accomplishment of Scottish scientists at this date is beyond dispute; it needs little in the way of counting to tell us so. But it is a little startling to conclude from the figures that the contribution was, perhaps, five times greater than it had been or was to become.

The number of Fellows of the Royal Society relative to population is used to compare the performance of Scotland and Britain. The temporal comparison that emerges from these figures is less striking, though it still clearly reveals a Golden Age. It is disappointing to find, however, that in most years for which a count has been made, Scottish membership of the Royal Society is less than in proportion to its share of the British population. It may be that the geographical comparisons are more revealing than those over time. There are not many more elements to be discovered, it appears, and other of the qualifying criteria may be more difficult to fulfil when research is less individual and more costly. Nor can one feel at all sure, without some rudimentary testing, of the statistical significance of the comparisons. There are, however, some interesting by-products of the investigation: the

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suggestion of a pocket of brilliance in Buchan and Mearns quite apart from the remarkable Gregory family; and the, perhaps, expected contribution of the Manse, far out of proportion to its numbers.

What is the explanation of the failing Scottish contribution? The authors think emigration, and draw their own conclusions about the deficiency of scientific and other opportunities in Scotland. They feel that the exodus has been so great that irreparable genetic damage has been done already; and that it may not be enough to attempt to restore Scotland's scientific heritage by introducing sufficient additional research activity to give the country a share proportionate to her population.

But all this covers only a limited aspect of what may be called the Scottish problem. It was not just science that had its Golden Age and perhaps other disciplines and talents, including administrative skills, were better represented; and it is not just science that suffers now. If the authors wish to continue their pre-occupations with Scotland's heritage they could do worse than put their problem in a wider social and economic setting and see what might be done in the light of this.

J. R. PARKINSON

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Current Notes

United Kingdom Official Statistics

1. *Statistics of Education 1961, Part 2*

As noted in a previous issue, the Ministry of Education is now publishing its statistical information in two annual volumes, and Part 2 of the 1961 statistics appeared in July 1962 (H.M.S.O., 15*s.* 0*d.*).

This volume covers subjects on which information became available during the first half of the year. These include further education, training of teachers, G.C.E. examinations and school leavers, educational building and scholarships and awards.

The new publication gives additional information to that previously published, including revised statistics showing the growth of further education over the past eight years and distinguishing the growth of advanced level courses; statistics showing the growth of teacher training colleges in the past decade; age distributions of students following certain full-time and sandwich courses in further education establishments and of students in teacher training colleges; the percentage of employees under 18 granted day release in different industries; and the terms in which pupils leave school (excluding secondary modern and all-age schools).

2. *Domestic Food Consumption and Expenditure, 1960*

The eleventh Annual Report of the National Food Survey Committee, entitled *Domestic Food Consumption and Expenditure: 1960*, appeared in July 1962. The report follows the general pattern already established in this series; the main sections deal with the diet of the average household in Great Britain, and with the particular diets associated with variations in income level, household composition, region and type of area. The labour-saving prepared or semi-prepared foods, known as "convenience foods", receive more detailed attention in view of their increasing importance in the average diet; the real value of purchases of these foods has risen by 17 per cent. since 1956, compared with a rise of 1 per cent. for other foods.

The Report also includes the results of three special studies. The first of these examines changes in diet associated with the presence of an infant in the household. Families with infants had a lower average net family income than other families of the same size (since mothers of infants can less easily go out to work); although their expenditure on food was also lower, it represented a greater proportion of their income. Nevertheless, the diets of families with an infant were generally more satisfactory than those of other families of the same size, because the needs of an infant are less. A second study (in Appendix E of the Report) records details of food expenditure and purchases during the Christmas period, and in particular measures the day-to-day price movements of the main foods in the week before Christmas. In the third study, presented in Appendix G, estimates of income elasticity of demand are given for most of the main foods, together with comparable results for earlier years.

3. *Reports by the Government Actuary*

The annual interim reports by the Government Actuary on the operation of the National Insurance Acts and of the Industrial Injuries Acts in Great Britain, both for the year ending March 31st, 1961, were published in May 1962 (H.M.S.O., 8*d.* each).

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United Nations Statistical Publications

Statistical Yearbook, 1961. (English/French.) 678 p. Clothbound: U.S. $10, 71*s.* stg., Sw. fr. 43; Paperbound: U.S. $8, 57*s.* stg., Sw. fr. 34.

This is the thirteenth issue of a comprehensive collection of international statistics for more than 250 countries and territories. It presents a wide range of up-to-date statistical series on economic and social subjects, such as: population; structure of mining, manufacturing, construction and electricity and gas industries; production of commodities in agriculture, mining and manufacturing; textile and transport equipment; production, trade and consumption of energy; postal, telegraph and telephone services; civil aviation; tourist travel; external trade; balance of payments; international economic aid; wholesale prices; national income; budget accounts and public debt; housing; medical personnel; educational institutions; newsprint and newspapers; books, films, cinemas and radio and television broadcasting receivers.

The contents cover information received up to December 1961; the figures are presented in 186 tables, generally covering the year 1948 and some nine most recent years. Appendices are included showing conversion factors and alphabetical subject and country indices.

Yearbook of National Accounts Statistics, 1961. (English/French.) xxiv, 305 p. U.S. $4, 28*s.* 6*d.* stg., Sw. fr. 17.

This presents detailed estimates of the following economic measurements for 77 countries: gross product by type of expenditure and by industrial origin; national income by distributive shares; capital formation by type of capital goods, industrial use and purchaser; sources for the financing of capital formation; sector accounts for households and government; private consumption expenditure by category of goods and services; external transactions of the nation. To facilitate country comparisons, estimates for most countries are set out in standard tables and conform to the international standards and definitions in this field. Tables are documented with explanatory notes and source information. The book also includes international tables showing the principal aggregates and their interrelationships for 98 countries and territories, and rates of growth of total and *per capita* real gross product.

Statistical Indicators of Housing Conditions. (English; Spanish in preparation.) Statistical Papers, Series M, No. 37. 21 p. U.S. $0.35, 2*s.* 6*d.* stg., Sw. fr. 1.50.

This contains a formulation of indicators of housing levels of living or, in other words, a series of statistical yardsticks by means of which countries can assess their housing conditions in relation to those of other countries and also evaluate national progress over time. It comprises statistical measures of the basic housing requirements of adequate shelter, privacy and sanitation as reflected by availability of permanent dwellings, density of population and provision of essential facilities. It presents the items on which information is required for the computation of the indicators, together with references concerning their definition and methods of collection.

A New Journal of Mathematics

Professor D. G. Kendall has kindly sent the following note on the new journal *Topology* (Pergamon Press: Oxford, £10 p.a.):

The late Henry Whitehead revitalized Oxford mathematics, contributed fundamentally to topology, and inspired an enormous number of young mathematicians (not all of whom were topologists). Before his untimely death he founded this new journal, volume 1 (part 1) of which has now appeared. *Topology* will appear four times yearly, and will publish papers on topology and related subjects. From this first number it is clear that abstract harmonic analysis, at least, will be among the related subjects. It is to be hoped that the editors will continue to interpret the scope of the journal in a liberal way, and in this way help to

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mitigate the tendency for the community of mathematicians to fragment into a number of disjoint open connected subsets.

The printing of the journal is excellent, and the publishers are to be congratulated on their decisions (i) to supply authors with 100 free reprints, (ii) to supply personal subscribers with the journal at 35 per cent. of the published price. It is much to be hoped that other publishers will follow the lead of Pergamon.

A New Austrian Statistical Journal

Dr M. G. Kendall has kindly supplied the following note:

There has recently appeared the first issue of a new journal devoted to probability, and related subjects, *Die Zeitschrift für Wahrscheinlichkeitstheorie und Verwandte Gebiete*, published by Springer under the general editorship of Professor L. Schmetterer of Vienna, assisted by an international panel of Editors and collaborators. The first issue comprises 108 pages and costs 19.80 Deutschmark. It contains seven articles, five in German and two in English, ranging in topic from the severely theoretical (e.g. on "Folgen normierter Masse auf kompakten Gruppen") to the severely practical (e.g. on "The AOQL of Multi-Level Continuous Sampling Plans").

Canadian Political Science Association—Conference on Statistics, 1960

In 1958 the Canadian Political Science Association set up a committee on statistics to investigate ways and means of improving the facilities for research in the social sciences involving the use of statistics. One of the outcomes was the conference on statistics held at the Queen's University in Kingston, Canada, in June 1960.

The Association has now issued a volume of papers (edited by E. F. Beach and J. C. Weldon, University of Toronto Press, 1962). The papers range over a wide field as the following list testifies: Historical estimates of internal migration in Canada (by K. Buckley); The "mass society" and "community" analyses of the social present (by R. E. du Wors, R. Batson and M. Daffron); Canadian criminal statistics (by P. J. Giffen); The post-war rise of the crude petroleum industry in Canada (by E. J. Hanson); Salaries of engineers and scientists, 1951 (by G. Rosenbluth); The distribution and functions of Canadian engineers and scientists (by D. Solomon and A. M. Fergusson); The structure and growth of the Canadian air transport industry (by K. W. Studnicki-Gizbert); and The Canadian manufacturing industry, 1900–1957 (by T. R. Vout).

Professor Cox's Inaugural Lecture

Professor D. R. Cox's inaugural lecture, "The role of statistical methods in science and technology", delivered on November 21st, 1961, has been published by Birkbeck College, London. Professor Cox discusses briefly, and illustrates by examples, a wide variety of ways in which statistical concepts can provide insight into scientific and technological problems.

Population Council Fellowships, 1963–64

The Population Council is offering about 25 fellowships for study in demography at the predoctoral or postdoctoral levels. These fellowships are available to qualified students from all countries: particular consideration is given to students from the economically underdeveloped areas. Preference is given to applicants who have completed at least one year of study beyond the college level and who have a background in the social sciences

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and statistics. The plan of study and choice of university are made by the applicant. Fellowships are for training in demography, although related study in sociology, economics, biostatistics and other relevant fields may form part of a total programme. Fellows need not be candidates for a degree. The basic stipend is £2700, which may be supplemented to provide for tuition, travel, maintenance of dependants and other expenses. The deadline for receipt of completed applications for the 1963–64 academic year is February 1st, 1963.

Further information and application forms may be obtained from the Fellowship Secretary, The Population Council, 230 Park Avenue, New York 17, New York, U.S.A.

Houblon–Norman Fund Awards

The Trustees of the Houblon–Norman Fund, on the recommendation of their Advisory Committee, have made the following awards of research grants for 1962–63:

Dr G. C. Allen, C.B.E. (Professor of Political Economy, University College, London): Japan's economic recovery; Dr Srinivasa Ambirajan (Research student, University of Manchester (Renewal)): The influence of economic thought on the policies pursued by the British Government and East India Company in India; Mr G. G. Barber, M.A. (Bodleian Library, Oxford): The international book trade in the eighteenth century; Mr Michael Kidron, B.Litt. (Research student, The Royal Institute of International affairs): Foreign capital and Indian development; Miss D. M. Livock, M.A., F.C.A. (Supernumerary Fellow, St Anne's College, Oxford): The financial history of the Corporation of Bristol; Mr R. L. Marris, M.A. (Fellow, King's College, Cambridge): Financial relations between management and shareholders; Mr M. V. Posner, M.A. (Fellow, Pembroke College, Cambridge): Economic problems of nationalized industries; Mr J. R. Sargent, M.A. (Fellow, Worcester College, Oxford): England in the international economy since 1870; Mr P. A. Stockham, B.A. (Dillon's University Bookshop Ltd, London University): Economic conditions governing the British book trade; Dr S. G. Sturmey (Lecturer in Political Economy, University College, London (Renewal)): British shipping and world competition; Mr David Williams, B.Sc. (Econ.) (Lecturer in Economics, University of Hull): Banking in South and East Asia.

The Trustees now invite applications for Fellowships or Grants in aid of Research into the working of industry and finance in Great Britain and elsewhere and the economic conditions affecting them.

Fellowships, which are intended for experienced research workers, are awarded for full-time research for one year, which may be renewed for a second year. Grants are awarded for either full-time or part-time work. The amount of the awards, which will take effect from October 1st, 1963, will depend upon the circumstances of the candidate and the probable expenses of his work.

Applicants must be British subjects normally resident in the United Kingdom. The Trustees are prepared to relax this rule only in exceptional circumstances.

Forms of application, which may be obtained from the Secretary of the Fund, c/o Bank of England, London, E.C.2, should be returned not later than March 1st, 1963.

Shewhart Medal

We offer our congratulations to Mr L. H. C. Tippett, who has been awarded the Shewhart Medal of the American Society for Quality Control "for outstanding leadership in the field of modern quality control".

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Obituary

Sir Ronald Aylmer Fisher, 1890–1962

We record with deep regret the death of Sir Ronald Fisher on July 29th, 1962. A series of obituary articles, together with a bibliography of his writings, will be published in this *Journal*, Part 1, 1963.

On the occasion of Sir Ronald's death the Society received the following telegram from the Statistical Laboratory, Department of Statistics, Iowa State University:

> "We were shocked to learn of the untimely death of your distinguished past President, Sir Ronald Fisher. We in the Statistical Laboratory at Iowa State University feel this loss most deeply since we were honored by the first professional visit of Sir Ronald to the United States in 1931 and therefore we owe a particular debt of gratitude quite apart from that due to immense heritage he has left the statistical community in general. It was Professor George W. Snedecor's privilege to recognize quite early the importance of Fisher's work to scientific research and to bring it to the attention of research workers in the United States particularly in the biological and agricultural sciences. Subsequently this methodology found uses in all sciences concerned with inferences in the face of uncertainty."

The President of the Society replied as follows:

> "On behalf of the Royal Statistical Society I thank you for your moving tribute to Sir Ronald Fisher. We shall always remember the great part played by Professor Snedecor and the Statistical Laboratory at Ames in spreading the new ideas throughout the United States and the world."

Luigi Einaudi, 1874–1961

Luigi Einaudi was born at Carrù, a small village in the mountains of Piedmont (near Turin, in Northern Italy), on March 24th, 1874, and died in Rome on October 31st, 1961. He graduated in law at Turin University in 1895 and qualified for university teaching in public finance, in the same faculty, in 1898. From 1902 to the date of his death he held the professorship at the same University (at his request, a special law was passed granting him a life professorship after his presidency of the Republic; this was the only privilege Einaudi requested in his lifetime). From 1902 to 1925 he also taught public finance at the Department of Economics of Bocconi University, Milan.

In 1912 Einaudi was elected a member of the Academy of Sciences in Turin; many of his most important essays in theoretical economics were originally published as transactions of that Academy. In 1919 the King of Italy appointed him Senator (which, at that time, was a life appointment) as one of the outstanding members of the Academy of Sciences. For some years he served actively in Parliament, working at a tax reform programme. When the Fascists took over the Government he withdrew from active politics. Only twice did he take part in Parliamentary debates and on both occasions he censured Mussolini's government; first, in 1926, he was among the very few who voted for Senator Ruffini's motion against the Government, and then in 1936 he was among the very few who censured the conquest of Ethiopia. The Fascists did not dare to send him to prison or to political confinement because, like Croce, he was a symbol of Italian culture and Risorgimental traditions. In 1926, however, they deprived him of his professorship at Bocconi University in Milan and in 1936 they suppressed the scientific journal *La Riforma Sociale*, of which he was editor. From 1896 to 1926 Einaudi contributed many articles on current economic problems to two Italian newspapers: *La Stampa* of Turin and

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Il Corriere della Sera of Milan, and later he wrote about Italian problems as a correspondent for *The Economist*. We have been told by the editor of *The Economist* that Einaudi used to send many of his reports from Nice, rather than from Italy, in order to avoid censorship. The articles were published as "from a special correspondent" to prevent his being persecuted by the Italian police.

Einaudi was appointed President of Turin University in July 1943 after the fall of Mussolini's government. He escaped to Switzerland when the Fascists came back to Northern Italy, and in Lausanne and Geneva he taught economics to Italian university students who were refugees in Switzerland. Early in 1945, one of the darkest periods of Italy's financial history, he flew to Rome, where he had been appointed Governor of the Bank of Italy by the new democratic Government. He fought successfully against inflation and in June 1947 was appointed Secretary of State for the Budget (a special law was passed to allow Einaudi to hold this post as well as that in the Bank of Italy, in order to concentrate monetary and credit policy largely in his hands). Quite soon, inflation was definitely overcome. In May 1948 Einaudi was elected first President of the new Italian Republic. After being President for seven years he went back to his studies, retiring entirely from public activity. In the remaining years of his life he worked on the twenty-volume re-edition of his economic and political works. At the time of his death the general plan had been completed and the most important contributions had been reprinted with his new prefaces and bibliographical appendices.

Einaudi's contributions cover many aspects of economics. He made outstanding contributions to the study of the economic and welfare effects of taxation, with special regard to the effects of taxation on saving and to the significance of quasi-rents in relation to earnings of factors of production. Einaudi's contributions to the problems of choice in public finance are also very important. These works set him among the most prominent figures of Italian neo-classical economics. But he devoted his research activity also to the general theory of economic policy and social organization and to the methodology of economics (a famous controversy between Einaudi and Croce on economic principles was published in the *International Economic Papers* some years ago). Einaudi's production of short articles and commentaries on current economic problems was very extensive. In conclusion, special mention must be made of his writings on the idea of a Federal Europe (some of them dating back to the First World War), political freedom and democratic institutions. Einaudi's ideology may be called "neo-liberal". However, it is hard to classify Einaudi's position, since he himself refused to be framed within a definite school of thought.

FRANCESCO FORTE'

SIR WILLIAM PALIN ELDERTON, 1877-1962

William Palin Elderton, who died on April 6th, 1962, in his 85th year, was one of the select but growing number of latter-day actuaries who clearly perceived that the work of the profession, of which he was so distinguished a member, was a form of applied statistics. This rediscovery harked back to the early years of the Institute of Actuaries when, in the 1850s and 1860s, such a President as Samuel Brown, among others, was prominent in the affairs of the International Statistical Congress and the Economics Section of the British Association. In this context, it is noteworthy that Elderton's earliest impact on the actuarial outlook of the day occurred in 1903 when, in submitting to the Institute of Actuaries a paper—his first—on "Temporary Assurances", he graduated the data with the aid of a Pearsonian frequency curve and the method of moments—this at a time when graphical methods of graduation were the fashion. As George King, the author of what was for many years the basic Institute of Actuaries' textbook, remarked during the discussion on the paper:

> "Frequency curves were now always cropping up, but very few of the Fellows understood what they were."

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He went on to confess:

> "He did not feel equal to the task of following step by step all that had been written in the paper"

and called for an "explanatory paper" by the author.

Priority for the employment of the Pearsonian system in graduations for actuarial purposes must, however, be assigned to G. F. Hardy who, like Elderton, became President of the Institute (1908–10). Hardy had reviewed in the *Journal* of the Institute Karl Pearson's *Chances of Death and other Studies in Evolution* (1897), and had suggested that the Pearsonian curves might have applications in actuarial work. In 1902–03, Hardy in fact employed them in his celebrated graduations for purposes of the official British Offices' Life and Annuity tables. Nevertheless, it was Elderton who could fairly be said to have carried the Pearsonian torch to later generations of actuaries, and to others outside the profession, for George King's plea for an elucidatory paper led directly to Elderton's well-known work *Frequency Curves and Correlation*, first published in 1907 under the auspices of the Institute of Actuaries, which was widely used by the universities and reached its fourth edition in 1948. As Elderton put it in the first edition, the book was an attempt "to bring before Actuaries the more practical methods of modern statistical work". Even if these methods should happen to be of only slight assistance to them, he thought it "advisable for Actuaries to have some knowledge of the contemporary study of a subject connected with their own work on the theoretical side". It must have been a source of satisfaction to him that, after World War II, "the more central and systematic treatment of statistics" was at last reflected in the syllabus of the Institute of Actuaries' examinations, supported, moreover, by full-blooded textbooks, including "Johnson and Tetley" in two volumes.

Elderton was indeed fascinated by Pearson's system of curves—an interest that was sustained throughout his life. They were the theme of his Presidential Address to the Institute in 1932, in which he said he had chosen this subject because he had "cared more about it than any other at which I have worked". In that Address he wrote:

> "I vividly remember sitting in the Guildhall Library studying Pearson's early papers to the Royal Society . . . I was free of examinations in 1901 and spent more time than ever on curve fitting and graduation"

and he went on:

> "I found a strange pleasure, almost an artistic satisfaction, in obtaining the curves to fit the facts and smooth away unmannerly roughnesses."

It was probably in 1900 that Elderton had come to know Karl Pearson, and, although he never attended the lectures on statistics at University College, he was soon associated with the activities of the early biometric workers, and especially with *Biometrika*, to which he made numerous contributions over the years including, in 1903, a paper on the "Graduation and analysis of a sickness table" in which, as in the earlier paper that year before the Institute of Actuaries, he employed the Pearsonian curves. It was Elderton who, in 1901, computed the first table of the χ^2 integral. He was for many years active in the affairs of *Biometrika*, and, at the instigation of Karl Pearson, became the first Chairman of the Biometrika Trust formed in 1935, an office he occupied until 1961.

The extensive correspondence between Elderton and Karl Pearson is significant evidence of the status of Elderton in the early, formative years of the modern statistical methods. After Pearson's death he wrote (in a letter to Professor Egon Pearson):

> "I did not always agree with K.P. Generally, of course, I was wrong, but if I was right and convinced him, he was always pleased about it, and I went on my way feeling 'good all over'."

This is not the place to refer to Elderton's contributions to the actuarial profession which culminated naturally and inevitably in his election to the Presidency of the Institute (1932–34). It must suffice to state that his "distinguished services to actuarial science"

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were recognized by the presentation in 1937 of a gold medal jointly by the Institute and the Faculty of Actuaries in Scotland. There has been only one other such joint presentation, namely that in 1929 to G. J. Lidstone, his friend and predecessor at the Equitable Life Assurance Society. It was a fitting coincidence that he should for many years have administered the affairs of the "Equitable", which is this year celebrating the 200th anniversary of its foundation, marking the inception of scientific life assurance as it has ever since been practised.

Elderton's work in the actuarial field alone would have entitled him to prominent recognition in any account of the evolution of statistical methods. Yet, important as were these activities, no adequate impression of his stature as a statistician can be gained without references, however brief, to what may be styled his "extra-mural" interests. Mention has been made of his associations with Karl Pearson and of his work on *Biometrika*. Then there was, for example, a paper in 1907 to Section A of the British Association on "Examples of modern methods of treating observations", in which he illustrated the application of statistical methods to meteorological data. In 1908, he spoke to the Psychological Section of the British Medical Association on "Some statistical points in connection with the study of the inheritance of insanity". There were, too, contributions to the publications of the Galton Laboratory in two joint papers in 1910 and 1911 with S. J. Perry on the statistics of pulmonary tuberculosis. With his sister, Ethel Elderton, he was joint author of the *Primer of Statistics* (1909), written at the suggestion of Francis Galton who added a Preface. And this list is far from exhaustive.

In the wider spheres of insurance, he became President of the Insurance Institute of London (1935–36) and the Chartered Insurance Institute (1942–43); and Chairman of the Life Offices Association (1927–28) and of the British Insurance Association (1937–38). But perhaps the most important of the array of "extra-mural" activities was his service with Sir Alfred Watson as Joint Statistical Adviser to the Ministry of Shipping (1917–20), for which he was awarded a C.B.E. He performed similar functions in World War II in the Ministry of Shipping, later absorbed into the Ministry of War Transport: for his service in this capacity he was awarded a K.B.E.—he had previously been knighted in 1938. After World War I, he wrote *Shipping Problems 1916–21* in which he described some of the problems requiring statistical methods for their solution.

Elderton was for many years a Fellow of the Royal Statistical Society, and, with the conventional breaks, served on the Council between 1921 and 1945. He was appointed a Vice-President in 1926–27 and 1941–42. During this period he contributed papers to the *Journal*, in 1934 with G. H. Hansmann on "Improvement of curves fitted by the method of moments", and in 1943 with M. E. Ogborn on "Mortality of adult males since the middle of the 18th century as shown by the experience of life assurance companies". There was also a paper in 1945 on "Cricket scores and some skew correlation distributions", when famous cricketers took part in the discussion.

Elderton was a shy man with whom many found it difficult to establish contact, but those whose good fortune it was to enjoy his respect and confidence will all testify to his sympathy and understanding as well as to his wide-ranging ability, of which ample evidence has here been provided. He was fond of sketching and painting, and it was a privilege to receive one of his sketches as a Christmas card. He could talk with interest and knowledge about literature, poetry, philosophy and architecture, particularly old churches.

He maintained an enduring interest in the junior members of his profession, and was the driving force behind the development of the Institute of Actuaries Students' Society, formed in 1910, serving as its Chairman as well as Editor of its Journal for the first ten years of its existence. This interest in his junior colleagues also manifested itself in the scheme arranged in 1932 by the Institute of Actuaries, at Elderton's suggestion, under which Scandinavian actuarial students have been given facilities for studying in the United Kingdom. It was fitting, therefore, that in 1938 the University of Oslo should have conferred on him an honorary degree of Doctor of Philosophy.

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Elderton was educated at Merchant Taylors' School where he shared the first two places in the same form with J. H. Jeans—later the eminent astronomer. His father was a Wrangler in the 1860s, but the latter's early death made it impossible for Elderton to go to Cambridge, and he started work with the Guardian Assurance Company at 17. His younger brother, however, became a Wrangler in 1918. Those who, contrary to the spirit of the times, hold, with Karl Pearson, that there is rather more "nature" than "nurture" in the determination of ability will derive support for their thesis from Elderton's career and quality, of which this is but an inadequate sketch.

F. A. A. MENZLER

JOHAN FREDERIK STEFFENSEN, 1873–1961

Steffensen was one of those who "warmed both hands before the fire of life"—he kept his faculties in full to the end of his 88 years and he was with his friends in the Danish Actuarial Society only five days before his death on December 20th, 1961. A sudden heart attack took him and left a host of friends in many countries glad for his life, and sad for their loss.

Steffensen was born on February 28th, 1873, in Copenhagen, the son of the Supreme Judge of the Danish Army, and he took his degree in law at the University of Copenhagen. After a short period in Fredericia, he returned to Copenhagen to begin his career in insurance, at first in re-insurance and later on the official board which supervised insurance. The mathematical nature of the problems involved in insurance led him to develop his mathematical talents, which were superb—he was self-taught in this subject. In 1912 he gained his Ph.D. at the University of Copenhagen for a study in the theory of numbers.

After three years as Managing Director of a mutual life assurance society, he found his life's work in the teaching of insurance mathematics. He was the lecturer in this subject at the University of Copenhagen from 1919 to 1923 and Professor of Insurance Mathematics there from 1923 to 1943. But the teacher was not divorced from the practical man of affairs. Steffensen was a member of the Danish Life Insurance Council from 1926 to 1929, when he joined the Board of Statsanstalten for Livsforsikring (the oldest Danish life office, an institution established by state guarantee), being Chairman of the Board from 1941 to 1951.

Yet notwithstanding these responsibilities, Steffensen found time for research in various fields of mathematics, the complete tally of his scientific publications comprising 107 items, the first in 1904 and the last in 1957. His more important works included the theory of statistics (1923), interpolation (1925), insurance mathematics (1934) and the calculation of interest (1936). His work displayed a clarity of thought which revealed the pure mathematician, whatever the subject. The book on interpolation, for example, was a thorough treatment of a subject which would have received more attention had mechanical methods of calculation developed less rapidly. His contributions to pure mathematics went well beyond the fields of the ordinary statistician, and to his own original contributions he added the joint editorship (for Denmark) for 30 years of *Skandinavisk Aktuarietidskrift*.

It is little wonder that his ability was recognized throughout the Scandinavian countries and beyond. He was President of the Danish Actuarial Society in 1922–24 and 1930–33, and of the Danish Mathematical Society in 1930–36; both Societies made him an honorary member, as also did the Swedish Actuarial Society and the Statistical Society of Czechoslovakia. The University of Sorbonne invited him to lecture in Paris in 1931.

Steffensen had close connections with this country. His marriage to an Englishwoman, Annie Caroline, the daughter of Albert Chesterfield Jenour, made him as much at home in England, both physically and in the English language, as in his own country and tongue. His friends here delighted to honour him; he was an honorary Fellow of the Royal

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Statistical Society and received the rare distinction of becoming a Fellow of the Institute of Actuaries by election.

The University of London invited him in 1930 to lecture on his researches in the theory of statistics and actuarial science. His lectures gave insights into the depths that might be hidden in apparently simple problems; he graciously presented the copyright to the Institute of Actuaries and the lectures were published under its auspices. A personal recollection is of his ready help with the derivation of a formula for the rate of interest in an annuity-certain, stated without proof in an eighteenth-century work by William Jones, F.R.S. Steffensen, in a few lines, showed how the expression should be expanded in the powers of i, when the term involving i^3 disappeared and the higher powers could be neglected, leaving a quadratic which was the basis of the solution.

His mastery of English and his love of English literature, especially Shakespeare, were shown at the Centenary Assembly of the Institute of Actuaries in 1948. In the course of a discussion, he remarked that when a certain professor and he went out shooting sparrows, the professor took a cannon whereas he himself took a shot-gun; he hoped both hit the sparrows! Few present were unmoved when at the closing ceremony he referred to the hardships of the war years and quietly said, "These few days have been to us like a Mid-summer Night's Dream, which we shall always remember."

His last visit to London was in 1955 for the celebrations of the return of the Institute of Actuaries to its traditional home in Staple Inn—but he was proud to come privately, in his own right as Fellow, and no longer as a guest.

Steffensen, himself, was a modest man but should be ranked among the great.

M. E. OGBORN

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List of Books received for Review

(These books are available to Fellows in the Society's Library.)

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Ancker, C. J. & Gafarian, A. V. The function

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INDEX

To the Journal of the Royal Statistical Society, Series A (General)

Volume 125, 1962

PAGE

ALANEN *et al.* Abstracts of statistical computer routines . . . 317
ANDREWS, P. W. S. and FRIDAY, F. A. The recent controversy over resale price maintenance: a rejoinder . 592–595
ANNUAL General Meeting, Proceedings of the 128th, June 20th, 1962 . 627
APPOINTMENTS. Recent appointments, elections and awards . . 177, 317, 519

BARNARD, G. A. Comments on Stein's "A remark on the likelihood principle" 569–573
BARNARD, G. A., JENKINS, G. M. and WINSTEN, C. B. Likelihood inference and time series . . 321–352
Discussion: J. Durbin; P. Whittle; M. S. Bartlett; D. G. Champernowne; C. M. Stein; D. V. Lindley; M. G. Bulmer; M. G. Kendall; E. H. Lloyd; A. Birnbaum; E. S. Pearson; P. Armitage; P. Sprent; R. Pope; C. A. B. Smith; Authors in reply 352–372
BARTLETT, M. S. Probability, Statistics and time, Inaugural Lecture, University College, London . 316
BAYES, The Reverend Thomas, F.R.S. (1702–61), *See* HOLLAND, J. D.
BENJAMIN, B. World population trends. *See also under* FOOD SUPPLIES . 378–386
BIOMETRIC medicine, Research group in 316
BOOK REVIEWS:
Ackoff, R. L. (Ed.). Progress in operations research, vol. I . . . 162
Aitken, A. C. The case against decimalisation 503
Alder, H. L. and Roessler, E. B. Introduction to probability and statistics, 2nd ed. 634
Alexander, H. W. Elements of mathematical statistics . . . 489
Armitage, P. Sequential medical trials 289
Bailey, N. T. J. Introduction to the mathematical theory of genetic linkage 496
Bartlett, M. S. Essays on probability and statistics 484
Bellman, R. Adaptive control processes: a guided tour . . . 161
Bennett, M. K. and Peirce, R. H. Change in the American national diet, 1879–1959 502
Blyth, C. R. and Hutchinson, D. W. Tables of Neyman-shortest unbiased confidence intervals . . . 641
Bowen, E. K. Statistics—with applications in management and economics 163

PAGE
Cabell, R. W. and Phillips, A. Problems in basic operations research methods for management . . . 163
Chung, K. L. Markov chains with stationary transition probabilities . 494
Church of England, Statistical Unit of Central Board of Finance. Facts and figures about the Church of England 656
Clement, A. G. and Robertson, R. H. S. Scotland's scientific heritage . . 662
Cole, D. with Utting, J. The economic circumstances of old people . . 499
Council of Europe and Overseas Associated Countries. Statistical data . 300
Cox, R. T. The algebra of probable inference 640
Cox, D. R. and Smith, W. L. Queues . 151
David, F. N. and Barton, D. E. Combinatorial chance 639
David, F. N. and Pearson, E. S. Elementary statistical exercises . 286
Deming, W. E. Sample design in business research 158
Devons, E. and Crossley, J. R. The Guardian wage indexes . . 506
Dewhurst, J. F. *et al.* Europe's needs and resources: trends and prospects in eighteen countries . . . 311
Duckworth, W. E. A guide to operational research 647
Dynkin, E. B. Theory of Markov processes 495
Fano, R. M. Transmission of information: A statistical theory of communications 645
Francis, R. G. The rhetoric of science—a methodological discussion of the two-by-two table 156
Frisch, R. Planning for India: selected explorations in methodology . . 504
Furneaux, W. D. The chosen few: an examination of some aspects of university selection in Britain . . 169
Gayen, A. K. *et al.* Measurement of achievement in mathematics: a statistical study on effectiveness of Board and University examinations in India 658
General Register Office. The Registrar General's statistical review of England and Wales . . . 1960, Part II, tables, population 653
—— The Registrar General's quarterly return for England and Wales. Third quarter, 1960 654
Gibbs, J. W. Elementary principles in statistical mechanics developed with especial reference to thermodynamics 497
Gibson, E. and Klein, S. Murder: a Home Office Research Unit report . 500
Gnedenko, B. W. and Khinchin, A. An elementary introduction to the theory of probability 152
Grossman, G. Soviet statistics of physical output of industrial commodities: their compilation and quality . 165
Hall, P. G. The industries of London since 1861 660
Hamming, R. W. Numerical methods for scientists and engineers . . 642

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BOOK REVIEWS (*cont.*)

Von Handel, P. (Ed.). Electronic computers, fundamentals, systems and applications 161
Hill, A. Bradford. Principles of medical statistics 157
Houlden, B. T. (Ed.). Some techniques of operational research . . 647
International Labour Office. Family living studies: a symposium . . 298
Jeffreys, H. Theory of probability, 3rd ed. 487
Jewkes, J. and S. The genesis of the British National Health Service . 499
Kaufmann, A. and Cruon, R. Les phénomènes d'attente—théorie et applications 292
Kendall, M. G. A course in the geometry of *n* dimensions . . . 638
—— and Stuart, A. The advanced theory of statistics. Vol. 2—inference and relationship 284
Klaman, S. B. The postwar residential mortgage market 309
Kopal, Z. Numerical analysis . . . 288
Krupp, S. Pattern in organization analysis: a critical examination . 293
Kulldorf, G. Contributions to the theory of estimation from grouped and partially grouped samples . 493
Kurth, R. Axiomatics of classical statistical mechanics 154
Kuznets, S. assisted by Jenks, E. Capital in the American economy: Its formation and financing . . . 506
Lampman, R. J. The share of top wealth-holders in national wealth 1922–56 659
Lynes, T. National assistance and national prosperity 499
Marshall, A. Principles of Economics . 164
Maxwell, A. E. Analysing qualitative data 289
Medgyessy, P. Decomposition of superpositions of distribution functions . 155
Michal, J. M. Central planning in Czechoslovakia. Organization for growth in a mature economy . . 310
Ministry of Pensions and National Insurance and the National Assistance Board. Social security statistics . 168
Moore, G. H. (Ed.). Business cycle indicators 508
Morrison, P. and Morrison, E. (Eds.). Charles Babbage and his calculating engines: selected writings by Charles Babbage and others . . . 491
Moser, C. A. and Scott, W. British towns: a statistical study of their social and economic differences . 295
Mosteller, F. *et al.* Probability with statistical application . . . 153
—— Probability: A first course. . 153
McArthur, N. Introducing population statistics 501
Naas, J. and Schmid, H. L. Mathematisches Wörterbuch . . . 490
National Bureau of Economic Research. Demographic and economic change in developed countries. . . . 167
—— Output, input and productivity measurement: studies in income and wealth, vol. 25 305
—— Public Finances: Needs, sources and utilization 303
Neyman, J. (Ed.). Proceedings of the Fourth Berkeley Symposium on mathematical statistics and probability. 4 vols. 629
Nigam, K. and Chaudhuri, N. C. The corporate sector in India: a factual presentation of long- and short-term trends 166
Nurkse, R. Equilibrium and growth in the world economy . . . 509
O.E.C.D. Man-power statistics, 1950–1960 656
O.E.E.C. Demographic trends, 1956–76, in Western Europe and the United States 295
Ogborn, M. E. Equitable assurances . 649
Peacock, A. T. and Wiseman, J. The growth of public expenditure in the United Kingdom 302
Petersen, W. Population 294
Ractliffe, J. F. Elements of mathematical statistics 633
Ray, W. S. Statistics in psychological research 635
Rees, A. Real wages in manufacturing, 1890–1914 305
Reichmann, W. J. Use and abuse of statistics 636
Rodwin, L. Housing and economic progress: a study of the housing experiences of Boston's middle-income families 171
Runnenburg, J. Th. On the use of Markov processes in one-server waiting-time problems and renewal theory . 288
Savage, I. R. Bibliography of nonparametric statistics 637
Schumpeter, E. B. English overseas trade statistics, 1697–1808 . . 306
Shiskin, J. Signals of recession and recovery: An experiment with monthly reporting 508
Simonnard, M. Programmation linéaire 645
Solodovnikov, V. V. Introduction to the statistical dynamics of automatic control systems 291
Solomon, H. (Ed.). Studies in item analysis and prediction . . . 290
Sorsby, A. *et al.* Refraction and its components during the growth of the eye from the age of three . . . 497
Sraffa, P. Production of commodities by means of commodities . . . 307
Syski, R. Introduction to congestion theory in telephone systems . . 160
Talbot, P. A. and Mulhall, H. The physical anthropology of Southern Nigeria: a biometric study in statistical method 498
Thomas, B. (Ed.). The Welsh economy: studies in expansion 660
Titchen, R. S. and Rosenthal, A. J. (Eds.). Operations research management science 646
Tortrat, A. Principes de statistique mathématique 287
U.K. Interdepartmental Committee on Social and Economic Research. Census of production reports . . . 301
United Nations. Methods of estimating total population for current dates (reprint) 653
—— A short manual on sampling, vol. 1: Elements of sample survey theory 159
Vajda, S. Mathematical programming 495
Wallis, P. N. Quality control in the office 648
Weiss, L. Statistical decision theory . 635
Williams, G. P. and Goodwin, H. S. Road risks of the elderly . . . 655
Wolfowitz, J. Coding theorems of information theory 643
World Health Organization. Annual epidemiological and vital statistics, 1958 652

BOOKS received for review, lists of 178, 318, 520, 674

BUTLER, E. B. University students, staff and recurrent grants, prewar and post-war 118–143

CALDER, R. The mathematics of hunger, *See also under* FOOD SUPPLIES 373–377
CANADIAN Political Science Association. Conference on statistics . 666

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PAGE

CANCER survival rate. *See* HAYBITTLE, J. L.
CANE, V. R. Learning and inference . 183–200
Discussion: M. Stone; A. R. Jonckheere; R. J. Audley; A. E. Maxwell; I. J. Good; N. L. Webb; G. A. Barnard; M. S. Bartlett; M. Bud; A. Battersby; Miss Cane in reply 200–209
CENTRAL STATISTICAL OFFICE, Financial Statistics 515
CLARK, C. Future sources of food supply: economic problems. *See also under* FOOD SUPPLIES . . 418–444
COALMINERS' morbidity. *See* LIDDELL, F. D. K.
COMPUTER routines, statistical, abstracts of. *See* ALANEN *et al.*
COUNCIL, Report of, for the financial year ended December 31st, 1961, and for the sessional year ended June 20th, 1962 . . . 605–625
COX, D. R. Inaugural lecture . . 666

DAWSON, R. F. F. Estimated expenditure on road transport in Great Britain, 1960 462–472
DEFENCE SPECIFICATION DEF-131, Sampling inspection and. *See* HILL, I. D.

EDUCATION, Ministry of. *See under* MINISTRY
EDUCATIONAL allocation and selection, The statistical evaluation of. *See* FINNEY, D. J.
EINAUDI, Luigi. Obituary . . . 668
ELDERTON, Sir William Palin. Obituary 669
ELECTIONS to Fellowship 29, 209, 231, 372, 564, 626

FACTORY accidents, statistics collected by H.M. Factory Inspectorate. *See* RUDD, J.
FARRELL, M. J. and FIELDHOUSE, M. Estimating efficient production functions under increasing returns to scale 252–267
FEDERATION of British Industries, Report on export incentives . . 517
FIELDHOUSE, M. *See under* FARRELL, M. J.
FINNEY, D. J. The statistical evaluation of educational allocation and selection 525–549
Discussion: T. Lewis; W. D. Wall; F. Yates; J. Floud; R. N. Curnow; P. M. Levy; I. D. Hill; F. Sandon; D. J. Finney in reply . . 549–563
FISHER, Ronald Aylmer. Obituary . 668
FOOD requirements, estimation of. *See* PASSMORE, R.
FOOD supplies and population growth. Conference proceedings, April 1961 373–450
Paper by R. Calder . . . 373
Papers by B. Benjamin and R. Passmore. *Discussion:* W. H. Beckett; W. Klatt; A. S. Mani; R. A. M. Case; M. S. Brown; R. Calder; B. Benjamin; R. Passmore . 378
Papers by N. W. Pirie and C. Clark. *Discussion:* G. V. Jacks; A. S. Mani; R. Calder; D. F. Hollingsworth; J. C. McKenzie; C. B. Howe; W. Klatt; M. Hills; E. H. M. Badger; D. S. Madge; T. W. Wyatt; Brinley Thomas 399
FOOD supply, Future sources of: economic problems. *See* CLARK, C.
——— Future sources of: scientific problems. *See* PIRIE, N. W.
FRIDAY, F. A. *See under* ANDREWS, P. W. S.

HAYBITTLE, J. L. The estimation of *T*-year survival rate in patients treated for cancer 268–283
HEALY, M. J. R. and JONES, E. L. Wheat yields in England, 1815–59 574–579
HILL, I. D. Sampling inspection and Defence Specification, DEF-131 . 31–73
Discussion: G. Horsnell; G. B. Wetherill; G. A. Barnard; S. R. Broadbent; K. Hopkinson; P. G. Moore; K. A. J. Crawford; A. Hald; Mr Hill in reply . . 74–87
HOLLAND, J. D. The Reverend Thomas Bayes, F.R.S. (1702–61) . . 451–461
HONOURS. Awards to Fellows . . 177, 519
HOUBLON–NORMAN Fund Awards, 1962–63 667
HOUSEHOLDERS' local rates, The prospect for. *See* ILERSIC, A. R.
HUNGER, The mathematics of. *See* CALDER, R.

ILERSIC, A. R. The prospect for householders' local rates . . . 210–224
Discussion: B. Benjamin; P. J. O. Self; R. H. A. Chisholm; J. H. Woods; W. F. Page; C. T. Saunders; Mr Ilersic in reply . 224–231
INDUSTRIAL Injuries Act, Government Actuary's reports . . . 664
INFERENCE, Learning and. *See* CANE, V. R.
INPUT–OUTPUT tables for the United Kingdom, 1954 . . . 173
INTERNATIONAL STATISTICAL INSTITUTE Elections to membership . . 518
Thirty-third session . . . 176
ISRAEL, Statistical abstract of, Central Bureau of Statistics . . . 517

JEFFCOATE, G. O. The chance of being killed in a road accident for people born during different years . 583–587
JENKINS, G. M. *See under* BARNARD, G. A., JENKINS, G. M. and WINSTEN, C. B.

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Digitized by Arya Samaj Foundation Chennai and eGangotri

JOHNS HOPKINS University, New Statistics department . . . 316
JONES, E. L. *See under* HEALY, M. J. R.

LEARNING and inference. *See* CANE, V. R.
LEWIS, B. N. On the analysis of interaction in multi-dimensional contingency tables 88–117
LIDDELL, F. D. K. Coalminers' morbidity 1–19
Discussion: J. A. Heady; P. A. B. Raffle; A. H. A. Wynn; B. Stevens; J. Thomas; C. J. Cornwall; C. M. Stewart; H. W. Haycocks; Mr Liddell in reply . . . 19–29
LIKELIHOOD inference and time series, *See* BARNARD, G. A., JENKINS, G. M. and WINSTEN, C. B.
——— principle, A remark on. *See* STEIN, CHARLES. *See also under* BARNARD, G. A.
LONDON Statistics, London County Council's handbook on London 1950–59 517

MATHEMATICAL Probability, Conference on 518
MEGALITHIC unit of length, The. *See* THOM, A.
MILK distribution, English, The structure of, *See* STRAUSS, E.
MINISTRY OF EDUCATION, Statistics of Education, 1961 . . . . 515, 664
——— LABOUR. Statistics on income, prices, employment and production, Bulletin 515
——— POWER. Statistical digest, 1960 173
MULTI-DIMENSIONAL contingency tables, On the analysis of interaction in. *See* LEWIS, B. N.

NATIONAL Food Survey Committee. Domestic food consumption and expenditure, 1959 314
——— 1960 664
NATIONAL Insurance Acts. Government Actuary's reports . . 664

PASSMORE, R. Estimation of food requirements. *See also under* FOOD SUPPLIES 387–397
PIRIE, N. W. Future sources of food supply: scientific problems. *See also under* FOOD SUPPLIES . . 399–417
POPULATION Council Fellowships, 1963–64 666
——— trends, World. *See* BENJAMIN, B.
PRODUCTION functions, estimating. *See* FARRELL, M. J. and FIELDHOUSE, M.
PROTECTIVE duties 173

RAO, A. S. PRAKASA. A note on small value transactions in India's foreign trade 588–591
RESALE price maintenance, The recent controversy over: a rejoinder. *See* ANDREWS, P. W. S. and FRIDAY, F. A.
RESEARCH Section, Regulations of . 603–604
ROAD accident, The chance of being killed in, for people born during different years. *See* JEFFCOATE, G. O.
——— transport, Estimated expenditure on in Great Britain. *See* DAWSON, R. F. F.
RUDD, J. The statistics of accidents collected by H.M. Factory Inspectorate, Ministry of Labour . 144–150
RUSSIA, Note on the history of sampling methods in, Supplement to. *See* ZARKOVICH, S. S.

SAMPLING inspection and Defence Specification DEF-131. *See* HILL, I. D.
——— methods in Russia. *See* ZARKOVICH, S. S.
SCOTTISH agricultural economics . 174
SHEWHART Medal. Award . . 667
SPECIAL General Meeting, Proceedings of, June 20th, 1962 . . . 626
STATIST, THE. Editor of. Wholesale prices in 1961 596–602
STEFFENSEN, Johan Frederik. Obituary 672
STEIN, CHARLES, A remark on the likelihood principle . . . 565–568
STOCHASTIC Analysis group . . 176
STRAUSS, E. The structure of English milk distribution . . . 232–242
SUTHERLAND, I. Statistics of human conflict 473–483

TECHNICAL CO-OPERATION. Department of 516
THOM, A. The megalithic unit of length 243–251
TOPOLOGY. Note on the new Journal by D. G. Kendall . . . 665

UNEMPLOYMENT, Ministry of Labour Gazette report 314
UNESCO World survey of education. Vol. III. secondary education . 315
UNITED NATIONS External trade statistics. Seminar on. 1961 . . 315
Industrial and distributive-trade statistics, Bibliography of . . 518
Industrial seminar, 1961 . . 175
Input-output conference, Geneva, 1961 175
International standard industrial classification of all economic activities 175

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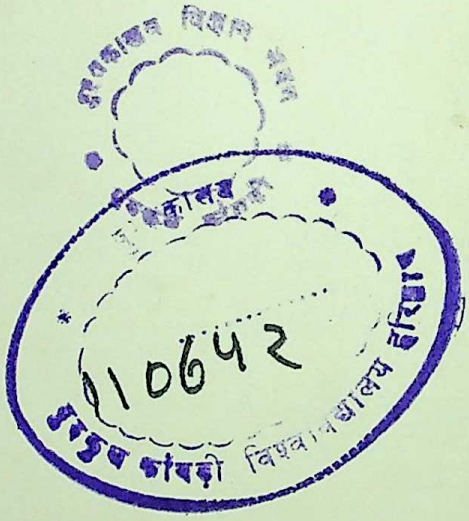
	PAGE
UNITED NATIONS (*cont.*)	
International standards for statistics. Directory of (Russian translation)	314
International training course on vital and health statistics . . . 1960, Report on	175
Retail price comparisons for international salary determination	518
Sample surveys of current interest, 9th report	518
Standard international trade classification, revised	174
Statistical indicators of housing conditions	665
Statistical yearbook, 1961	665
World energy supplies, 1957–1960	518
Yearbook of international trade statistics, 1960	517
——— national accounts statistics, 1961	665
UNIVERSITY students, staff and recurrent grants, pre-war and post-war. *See* BUTLER, E. B.	PAGE
WHEAT yields in England, 1815–59. *See* HEALY, M. J. R. and JONES, E. L.	
WHOLESALE prices in 1961. *See* STATIST, THE	
WINSTEN, C. B. *See under* BARNARD, G. A., JENKINS, G. M. and WINSTEN, C. B.	117 118
ZARKOVICH. S. S. A supplement to "Note on the history of sampling methods in Russia"	580–582
ZEITSCHRIFT für Wahrscheinlichkeitstheorie und verwandte Gebiete, Die. Note by M. G. Kendall	666



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art 4
PAGE
117
118
0–582
666

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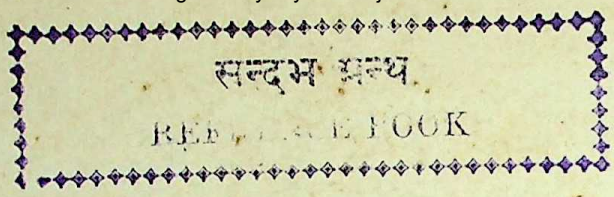

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